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IZA DP No. 17078

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ISSN: 2365-9793

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ABSTRACT

Assessing Dishonesty in Cocoa Value Chains: Lab-in-the-Field Evidence from Middlemen in Côte D'Ivoire*

This study investigates dishonest behavior among cocoa middlemen in Côte d'Ivoire, focusing on the role of observability and financial penalties in deterring such behavior. Using on a modified version of the "die-under-cup task", we examine the cheating behaviors of 151 cocoa middlemen over several interaction rounds. Our findings reveal that cheating is prevalent among cocoa middlemen, with 78% of players cheating at least once during the game. However, we found heterogeneous cheating patterns: 59% of cocoa middlemen consistently cheated when faced with a losing outcome, even when the risk of detection and sanction is high, 22% of them never cheated, and 19% did so occasionally. Key factors influencing cheating include age, religion, and risk attitudes. The study finds that introducing monitoring and sanctions significantly reduces cheating, highlighting the effectiveness of such mechanisms in deterring dishonest behaviors. By shedding light on the prevalence and determinants of cheating among cocoa middlemen, this study contributes to the experimental literature on dishonest behavior and understanding middlemen's role in agricultural value chains.

JEL Classification: C91, C93, D82, D91

Keywords: dishonesty, middlemen, agricultural value chain, observability, financial penalties, lab-in-the-field experiment

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* We thank Tanguy Bernard, Marie Boltz, and three anonymous referees for their valuable insights. The authors are grateful to Touton S.A. for their valuable partnership and to the enumerators in Côte d'Ivoire for their exceptional efforts in collecting data for this study.

1 Introduction

Intermediaries play an essential role in agricultural global value chains, connecting producers to international markets. Yet, the prevalence of information asymmetries within these value chains, often composed of many geographically dispersed players, and the weak enforcement mechanisms are likely to encourage dishonest behavior, such as the misreporting of quantities and qualities (Mitra et al. 2018; Rustagi and Kroell 2022). Despite the potential for widespread misconduct, the extent of dishonest behavior among intermediaries is likely to vary. While conventional economic theory posits that homo economicus engage in dishonest behavior based on a cost-benefit analysis and should therefore lie whenever it is beneficial (Becker 1968), recent experimental research challenges this paradigm. Individuals do not always lie or do not lie to the maximum extent, even when it would benefit them and they run no risk of being caught, indicating the existence of an intrinsic cost of lying (Fischbacher and Föllmi-Heusi 2013). The decision to engage in dishonest behavior is thus shaped by several individual characteristics, such as the preference for truth-telling, guilt aversion, self and social image concerns, and social norms (Abeler et al. 2019; Bašić and Quercia 2022; Huber et al. 2023; Khalmetski and Sliwka 2019). Understanding the drivers of dishonest behavior among middlemen is crucial for designing effective interventions to enhance the integrity and transparency of agricultural value chains prone to information asymmetries.

This paper considers the case of cocoa middlemen in Côte d'Ivoire, operating in certified value chains. Their role in the cocoa supply chain is crucial, as they must buy and transport cocoa from numerous small, remote farmers on behalf of local cooperatives or buying centers. They rank first at the bottom of the cocoa value chain after farmers, before the cocoa is distributed to cooperatives, and then to local processors or exporters. Operating in certified value chains means the farmers they source from comply with certification standards, making the information on the cocoa's origin (certified or conventional) critical, as they are the first to introduce it into the supply chain. However, since cocoa certification is a credence characteristic, indiscernible to the naked eye, and certified cocoa commands a higher market price, middlemen are incentivized to misreport the nature of the cocoa they deliver to cooperatives. This behavior is likely, as recent evidence shows significant discrepancies between the quantities of certified cocoa introduced into the supply chain by farmers and those recorded at the cooperative level (Bernard et al. 2024).

We implemented an adapted version of the "die-under-cup task," an experimental game commonly used to study dishonesty (Fischbacher and Föllmi-Heusi 2013). In this task, participants roll a die in private and report the outcome to the experimenter. Players are subsequently rewarded based on their reported outcome, incentivizing them to lie to increase their financial rewards. In our settings, players use a digital die on a tablet, which records the actual results, enabling us to determine individual cheating afterward.[1] The payouts associated with each dice outcome were predefined based on a risk-elicitation experiment similar to Holt and Laury (2002). Implementing an experimental game allows us to investigate middlemen's cheating behavior in a controlled environment, overcoming current difficulties in identifying their real-life behavior. Indeed, monitoring middlemen in real-life settings would require an effective traceability system to capture information at each supply chain level and prevent them from misreporting the nature of the cocoa in the system, but such a tool does not yet exist. Additionally, it is not possible to test for fraud because the physical characteristics, such as quality, size, or other properties, of certified and non-certified beans are indistinguishable. Similarly, directly asking them about their opportunistic behavior would have been prone to social desirability bias.

Beyond individual characteristics, the experimental literature has recently focused on contextual factors that may increase or discourage dishonest behavior. Along with the payoff frame (wins vs. losses) and size, the potential observability of dishonest behavior is one such factor likely to influence an individual's decision to engage in dishonest acts (Charness et al. 2019; Fries et al. 2021; Garbarino et al. 2019; Kajackaite and Gneezy 2017). Being observed typically increases social concern, as individuals do not want to be seen as misbehaving by others. Therefore, observability has been found to deter cheating behavior (Bašić and Quercia 2022; Fries et al. 2021; Gneezy et al. 2018). Existing studies on the impact of observability on cheating are usually set in one-shot games, thus offering no means of examining its long-term impact on individual cheating behavior in repeated interaction games. Moreover, little attention has been paid in the economic literature to financial sanctions as a tool for reducing dishonest behavior, which could be effective for individuals more influenced by loss aversion than social image concerns (Dai et al. 2018; Thielmann and Hilbig 2018). Our paper aims to address these gaps by introducing a new game feature to assess whether implementing a monitoring system—namely, the possibility of being observed and subsequently penalized if caught lying—can help reduce cheating behavior. The experiment was divided into three treatments, each featuring a different level of monitoring. In the first treatment, players were never observed by the experimenter. In the second treatment, the experimenter could observe the die-roll outcome after players reported the number displayed on their tablets. Observation was not systematic but determined by the outcome of a physical die-roll, with an even die leading to moni-

[1] Using a tablet for dice rolling rather than a cup may lead participants to question the confidentiality of their behaviors. In such a scenario, participants are likely to engage in less dishonesty. Thus, we argue that our findings represent a conservative estimate of dishonest behavior among participants.

toring by the experimenter. In the third treatment, in addition to the 50% risk of being observed, we introduced the possibility of incurring a financial penalty if the experimenter detected that the number displayed on the tablet did not match the one declared by the player. Each treatment consisted of five rounds, with the probability of losing, i.e., the probability of getting a number that yields no reward, progressively increasing over rounds. Final payouts were determined by randomly selecting one round for payment in each treatment.

Overall, 151 cocoa middlemen operating for 19 cooperatives sourcing certified cocoa participated in the game, and took part in a 20-minute survey about their activities as cocoa middlemen before the experiment. Our empirical analyses are drawn from 2,265 observations, corresponding to 15 rounds played by each of the 151 middlemen. We find that cheating is a widespread behavior among cocoa middlemen, occurring in 39% of the rounds played and rising to 73% when considering only losing rounds. Given the possibility of retrospectively observing each player's behavior across 15 rounds, players are classified into three categories based on their cheating behavior. Most players systematically cheat when faced with losing rounds (59%), 22% never cheat, while 19% cheat only occasionally. These results align with previous literature, showing that some individuals do not cheat when they have an opportunity to do so and that it would benefit them, even when there is no risk of being caught. Using a multinomial logit, we find that being a "sometimes" or "always" cheater is influenced by a few individual characteristics, such as age, religion, and attitudes toward time and risk. We investigate whether introducing observation and sanction affects the cheating behavior of "occasional cheaters" using a conditional logit with player fixed effects. We find that the mere risk of observation, and observation combined with a penalty, is associated with a reduction in cheating among players. Players are 42% less likely to cheat when they risk being observed and 52% less likely when they risk being observed and penalized for cheating. Findings also show that having actually been observed while cheating previously is negatively correlated with cheating in the current round, reducing the likelihood of cheating by 89%. Besides, the more financial penalties players receive, the less they cheat. We find no significant effect of previous losses and cheating history on cheating behavior in the current round.

Our contributions to the literature are twofold. First, we contribute to the experimental literature on cheating behavior, particularly the emerging literature using lab-in-the-field experiments to study fraud and corruption behaviors in developing countries (Armand et al. 2023; Hanna and Wang 2017; Harris et al. 2022; Rustagi and Kroell 2022). In line with Abeler et al. (2019) and Rustagi and Kroell (2022), our computerized version of the "die-under-cup-task" allows us to observe individual cheating behaviors, rather than aggregated ones. We provide additional evidence that, even when players face no risk of being caught cheating and that doing so benefits them, some of them never engage in cheating. Age, and particularly religion, seem to play an important role in the decision to engage in cheating behavior, likely by influencing both self and social image concerns. Additionally, players classified as risk-takers are significantly more likely to engage in cheating than others. Beyond better distinguishing intrinsic dishonesty from the randomness of die rolls (Rustagi and Kroell 2022), observing individual behaviors rather than aggregate ones provides an opportunity to analyze the effect of monitoring and sanction mechanisms on individual lying. Thus, we also contribute to the smaller body of literature that seeks to understand which mechanisms can inhibit dishonest behaviors (Bolton et al. 2021; Fries et al. 2021; Gneezy et al. 2018; Jiang and Villeval 2022; Kajackaite and Gneezy 2017). In this paper, we show that for players who have a finite cost of lying, and are thus likely to react to incentives aimed at reducing cheating, both simple monitoring of behavior and monitoring combined with a penalty for cheating are effective mechanisms.

Second, we contribute to the literature investigating middlemen's behavior in agricultural value chains with information asymmetries (Rubinstein and Wolinsky 1987). Recent empirical evidence in developing countries shows that intermediaries are prone to exploit market failures, such as low market competition or price information imbalances, to increase their profit margins (Bergquist and Dinerstein 2020; Bergquist and McIntosh 2021; Casaburi and Reed 2022; Chatterjee 2023; El Makhloufi et al. 2018; Renier et al. 2023). We provide evidence that such behavior can be observed in an experimental setting and concerns many middlemen. Focusing on the cocoa value chain, we offer quantitative insights into middlemen's socio-economic characteristics and activities and insights into their cheating behavior when faced with asymmetric information through our lab-in-the-field experiment. This provides valuable information about cocoa middlemen, who have been neglected in the literature on cocoa and certified value chains despite being considered the most opaque tier in the cocoa supply chain (Stoop et al. 2021).

The remainder of this article is organized as follows. Section II provides contextual elements that frame our experimental game. Section III presents the experimental design and procedure, and describes cocoa middlemen activities based on first-hand survey data. Results are discussed in sections IV et V. The last section concludes.

2 Context

Traceability in the certified cocoa supply chain. Our lab-in-the-field experiment takes place in Côte d'Ivoire, the world's largest cocoa producer, where approximately one million small-scale farmers generate an annual output of over 2 million tons of cocoa beans and contribute to 40% of the global cocoa production (Renier et al. 2023). We implemented our cheating game among middlemen, important players of the complex Ivorian cocoa value chain, which involves many geographically dispersed actors. Cocoa farmers sell their cocoa to middlemen, operating on behalf of cooperatives. Exporting companies then directly acquire cocoa from these cooperatives. We focus on middlemen working in certified value chains, i.e., those responsible for sourcing cocoa from certified farmers. The latter are farmers who comply with a set of criteria established by certification bodies, such as Rainforest Alliance or Fairtrade. Although the concept of certified cocoa theoretically implies the physical traceability of cocoa back to the farmer, farm-level traceability remains an important challenge in the cocoa industry. Despite nearly 60% of Ivorian cocoa farmers affiliating with certification standards (World Bank 2019), most industry players only reach traceability at the cooperative level. In fact, as of 2022, 50% of global cocoa production remained completely untraceable (Huetz-Adams 2022; Renier et al. 2023).

Achieving cocoa traceability down to the farmer level is difficult, as interactions between supply chain players are fraught with information asymmetries. Indeed, two types of cocoa are available on the market, conventional or certified, the latter being sold at a higher price than uncertified cocoa. As certified cocoa is a credence good, i.e. it is impossible to determine its type with the naked eye, this situation gives rise to information asymmetries. Middlemen working in certified cocoa value chains have thus a strong incentive to engage in side-sourcing, which involves sourcing uncertified cocoa but reporting it as certified to the cooperative in order to increase their profits. The supply of truly certified cocoa to cooperatives, therefore, depends on these middlemen's honesty. Yet, they are considered the most opaque link in the cocoa value chain (Stoop et al. 2021), with little information available about their activities, which are difficult to monitor.

Significant reporting discrepancies were observed in terms of volume of certified cocoa between farmers and cooperatives for a set of 22 cooperatives in Côte d'Ivoire (Bernard et al. 2024). In fact, the cocoa volumes recorded in the cooperatives' traceability registers were at least two to ten times higher than those declared by the farmers for the same period. This suggests a parallel supply of supposedly certified cocoa (defined as side-sourcing) to the volume sourced from the cooperatives' farmers. We argue that the likelihood of side-sourcing occurring at the farmer level is low since cocoa farms located in the same community tend to be certified, making it difficult and costly for farmers to collect non-certified cocoa. Thus, discrepancies between volumes reported by farmers and cooperatives registers found in Bernard et al. (2024) suggest that middlemen and/or cooperatives engage in side-sourcing. This companion paper also investigates the impact of a mobile traceability application designed to increase transparency in cocoa transactions between farmers and middlemen. Not all middlemen used this tool despite the absence of any technical issues, suggesting reticence among middlemen to use a tool that monitors their cocoa supply behavior. Among those who did use it, only small volumes of certified cocoa were digitized in the application, well below the volumes actually purchased by middlemen. The study, however, does not reveal whether middlemen are prone to dishonest behavior, the determinants of such behavior, or whether monitoring or sanctions would be effective tools to increase their honesty.

Detecting dishonest behavior. Measuring intermediaries' propensity to engage in side-sourcing using traditional observational data or surveys is challenging because such behaviors are hidden and subject to social desirability bias (Rustagi and Kroell 2022). Direct questions about opportunistic behaviors often lead to underreporting, as individuals tend to provide socially acceptable rather than truthful answers, especially when their responses might be judged or result in negative consequences. Additionally, since there are no physical differences between certified and non-certified beans, a quality control approach is inappropriate. The fact that whether the cocoa is certified or conventional is a credence feature, that the middlemen are involved with many farmers, and that purchasing activities are relatively informal and opaque, further makes accurate monitoring difficult. Even when traceability information does exist, the fact that it is partly manipulated by those who generate it adds complexity to the process of measuring fraud or dishonest behavior from supply chain players (Bernard et al. 2024).

We thus rely on a lab-in-the-field experiment on cheating to precisely measure dishonest behaviors and analyze the underlying factors of engaging in dishonesty, such as personal characteristics and economic incentives. Indeed, experimental approaches offer a controlled alternative to measure of individuals' dishonesty. Armand et al. (2023); Harris et al. (2022); Mitra et al. (2018); Rustagi and Kroell (2022) and Stoop et al. (2021) demonstrate how experimental games can predict real-world behaviors. Armand et al. (2023) conducted a corruption game in Mozambique where citizens could send bribes to local leaders. They found a significant correlation between the bribes sent in the game and the actual misappropriation of community funds, indicating that the game accurately reflected real-world corrupt behaviors. Stoop et al. (2021) used a "Mind game" where participants guessed the color on the backside of a card to achieve high payoffs, showing a significant correlation between lab cheating and the likelihood of not reporting overpayment in real-world scenarios. Mitra et al. (2018) used a subsidy application game with potato farmers in India, showing that those who underreported yields in the game also did so in real life, thus establishing the game's predictive power for real-world dishonesty. Rustagi and Kroell (2022) investigated milk quality reporting using a lab game where participants could adulterate milk, finding that lab behaviors correlated with actual market misreporting. Harris et al. (2022) used corruption games with police officers, showing that experimental measures of corruption correlated with observed corruption behaviors. Collectively, these studies underscore the predictive power of experimental games in understanding and forecasting dishonest behaviors across various real-world contexts.

3 Experimental design

3.1 Protocol

The game's design is grounded in the assumption that the decision to engage in dishonest behavior involves a trade-off between anticipated gains and costs, which vary according to the probability of being caught, the severity of sanctions upon detection, and individuals' willingness to take risks (i.e., their risk attitude). Consequently, we merged our cheating game with a conventional risk elicitation task to compare and assess individuals' general risk attitudes in such a game. To this end, we divided the experiment into two phases, summarized in Figure 1 below.

Phase 1: Risk elicitation game. The initial phase of the game is an adapted version of the classic Holt and Laury (2002) experiment, which was designed to assess risk preferences. During this phase, participants face a series of five paired lottery choices (Table 1), where they had to choose between two options, A or B, with lottery B being the riskier choice. These paired lottery choices were designed to reflect different probabilities of success. The lottery selection phase was of significant importance, as the participants' payoffs in the next part of the experiment depended on the chosen lottery option. Therefore, we can differentiate the preference for dishonesty from risk attitudes. Both players and experimenters recorded the players' lottery choices on paper, which was kept on hand throughout the game to prevent any mistakes related to expected payouts (see player sheet in Appendix A).

Figure 1: Structure of the experimental game

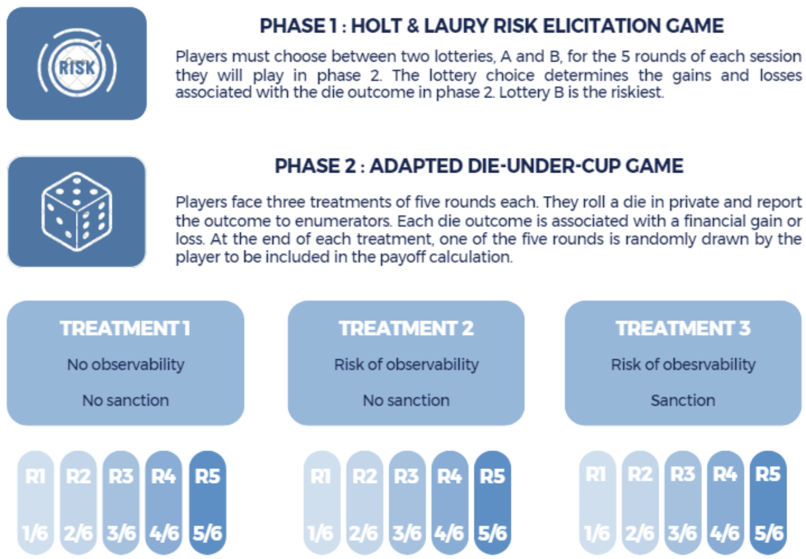


Table 1: The Five Paired Lottery-Choice Decisions

	Option A	Expected Payoff Option A	Option B	Expected Payoff Option B	Expected Payoff Difference
Lottery 1	5/6 of FCFA 1,500 1/6 of FCFA -1,000	1,083	5/6 of FCFA 3,000 1/6 of FCFA -2,000	2,167	-1,083
Lottery 2	4/6 of FCFA 1,500 2/6 of FCFA -1,000	667	4/6 of FCFA 3,000 2/6 of FCFA -2,000	1,333	-667
Lottery 3	3/6 of FCFA 1,500 3/6 of FCFA -1,000	250	3/6 of FCFA 3,000 3/6 of FCFA -2,000	500	-250
Lottery 4	2/6 of FCFA 1,500 4/6 of FCFA -1,000	-167	2/6 of FCFA 3,000 4/6 of FCFA -2,000	-333	167
Lottery 5	1/6 of FCFA 1,500 5/6 of FCFA -1,000	-583	1/6 of FCFA 3,000 5/6 of FCFA -2,000	-1,167	583

Source: Authors' calculation.

Phase 2: Adapted die-under-cup game. The second phase of our experimental setup draws inspiration from the cheating game originally introduced by Fischbacher and Föllmi-Heusi

(2013), the "die-under-cup task", in which participants are asked to roll a die in private and report the outcome to the experimenter. Participants are then rewarded according to the reported outcome, giving them an incentive to lie to increase their financial rewards. We implemented a modified version of this game.

First, we introduce a feature designed to observe retrospectively the true individual outcomes of the die rolls. Participants roll a six-sided fair die using a specially designed tablet application that records the true die outcomes after the experiment ends. Similar to Abeler et al. (2019) and Rustagi and Kroell (2022), we are thus able to identify individual cheating behavior. The remaining steps of the experiment follow the standard protocol: participants are asked to report the number displayed on the screen. However, because the experimenter does not have visual access to the tablet, participants can lie to maximize their earnings. During the experiment, players and experimenters face each other, maintaining a distance of at least two meters (Figure A1). No other person is in the classroom, ensuring only players can see the content on their tablet.

Second, we introduce the risk that experimenters will observe the result of players' dice rolls, and the risk of players incurring a financial penalty in the event of cheating. Thus, the game is divided into three treatments. In the first one, participants are never observed by the experimenter. The second treatment introduces the possibility of experimenters observing players' true outcomes directly on their tablets. The observation is not guaranteed but depends on the outcome of a physical die-roll, resulting in a 50% chance of being observed.[2] At this stage, no penalty is applied if players are caught cheating. The third treatment combines the risks of observability and of being financially penalized. Following the same observation procedure as in treatment 2, if experimenters detect that the number displayed on the tablet does not match the one declared by participants, a penalty of -2,000 FCFA is applied for that round, representing 2/3 of the maximum amount a player can win per round.

Finally, when most die-under-cup experiments take place in a one-shot game setting, we introduce repetition to simulate real-world scenarios with recurring opportunities for dishonesty. The participants played a series of three treatments, each consisting of five rounds. The probability of losing increases in each round by the same amount over the three treatments. Players have thus a one-in-six chance of losing in the first round compared with a five-in-six chance in the fifth round.

[2] In treatments 2 and 3, after rolling the die on the tablet, players then roll a physical die to determine whether the experimenter will observe their actual outcome. An even die on this second dice roll leads to monitoring by the experimenter.

This allows us to observe whether the likelihood of cheating is higher when the risk of loss increases. The potential earnings are contingent upon the lottery choices that participants made during the previous risk-elicitation game.

Overall, participants roll the die fifteen times over three treatments, and payouts are randomly determined at the end of each treatment. Players draw a chip from a bag containing five chips numbered 1 to 5, corresponding to the five rounds (Figure A1). The round drawn is the one selected for payment for this treatment. Once the three treatments are completed, the experimenter calculates the total winnings or losses based on the three selected rounds.

3.2 Field implementation

One of the main cocoa purchasing companies in Côte d'Ivoire gave us access to its partner cooperatives. We selected 19 cooperatives from a pool of 29 (see Figure A2), considering time, budget, and logistical constraints, and excluding those situated in isolated areas. These 19 cooperatives provided us with comprehensive lists of cocoa middlemen working for them, referred to as *délégués*, and/or their assistants who perform the same activities but under their supervision, known as *sous-délégués*. We randomly selected middlemen and assistants from each cooperative's list to participate in our experiment. We informed the selected individuals, as well as the cooperatives they worked for, that they would be participating in a quantitative survey on their activity as cocoa middlemen and of their opportunity to play a game in which they could win money. Except for travel expenses incurred to reach the survey and experiment location, we did not inform them regarding the potential amounts they could earn from their participation. The field experiment involved 17 sessions from August 16 to September 10, 2021, across nine cities in key cocoa-producing areas: Soubré, Gnipi, Cecchi, Alépé, Sikensi, Lakota, Gagnoa, Daloa, and Guiglo. The sessions were conducted in empty classrooms in primary or secondary schools, and a total of 215 middlemen attended informational sessions. As most of them lived in villages away from the experiment's cities, where the schools were located, we compensated them with 5,000 FCFA to cover their travel costs, equivalent to about 8 dollars. Four experimenters and one supervisor were assigned the responsibility of ensuring the attendance of the chosen participants on the day of the experiment.

The experiment started with a brief quantitative survey aimed at gathering socio-demographic and activity-related information. Middlemen received a reward of 6,000 FCFA for completing this 20-minute short survey. Subsequently, all participants gathered in a classroom for an introductory explanation of the experiment, providing them with a comprehensive understanding of the game, including details about potential associated payouts. Practice sessions with volunteer participants were conducted to ensure all middlemen understood the rules. Participation in the experiment was entirely voluntary, and those who agreed to play staked the 6,000 FCFA received from the survey. At this stage, 41 middlemen chose not to proceed and were released from the experiment. To ensure strict confidentiality and to prevent any potential peer influence that could influence participation in the game, we assigned each enumerator-player pair to a separate classroom, where the potential payoffs were displayed on a blackboard (Figure A1 in Appendix). Each experimenter briefly re-explained the game rules (without mentioning the possibility of lying), and the entire experiment was first tested with no money at stake to ensure that each player had a clear understanding of the game's steps and rules. Following this test version, the actual game, with money involved, begins. While 174 middlemen agreed to participate in the test, only 151 decided to take part in the actual experiment. Except for age and education, we observe no significant difference in player's profile between individuals who chose to participate and those who opted out (see Appendix B and Table B1). Despite the fact that most individuals who declined to participate cited a lack of time or prior commitments, we do observe a potential selection bias in risk attitudes, as risk lovers are significantly more likely to participate in the game. Our results are thus likely to be a lower bound as we over-estimate the proportion of cheaters. In total, the average duration of the survey and the experiment was about two hours.

3.3 Pre-lab quantitative survey

Before the experiment, middlemen were surveyed to gather socio-economic and activity-related information. This section summarizes the key characteristics and activities of middlemen who participated in our study.

Player profile. A total of 151 middlemen participated in the actual experiment. On average, players are approximately 39 years old (Table C1). 19% of middlemen have not received any formal education, 27% have completed primary education, 24.5% have attained middle school education, and 29% have achieved high school or higher education. The players are mainly Christians (47%) and Muslims (41.5%), with the remainder being animists. Regarding their professional background, most middlemen had significant experience in the cocoa sector. On average, they had been working as middlemen for their current cooperative for four years, with 62% of them having previous experience as middlemen, either working independently (as *pisteurs*) or for another cooperative. Although not all participants had prior middlemen experience, 94% of them had previously worked in the cocoa sector, mainly as cocoa farmers (in 95% of cases). The study included both middlemen (59.5%) and their assistants. It is worth noting that 94% of middlemen are involved in other activities, mainly cocoa farming (90%) and selling other agricultural products (25%). Only 40% of participants consider that being a cocoa middleman is their main activity.

Cocoa middleman activity. Not all middlemen source cocoa from the same number of farmers, as they are allocated a group of farmers within a specific geographical area defined by the cooperative they work for. On average, *délégués* work with approximately 114 farmers, although some source cocoa from up to 500 farmers (Table C2 in the Appendix). In our sample, middlemen delivered on average 90.5 tons of cocoa to their cooperative during the cocoa season preceding the survey,[3] 2/3 of it being collected during the main crop (from October to March) and 1/3 during the light crop (from April to September). Half of the middlemen were set delivery targets by the cooperative, and 90% of them failed to meet these targets. On average, *délégués* received a commission of 34 FCFA per kilo of certified cocoa delivered during the main crop. Annual income generated solely from cocoa middleman activities averaged around 1.5 million FCFA (equivalent to 2,476$) during the 2020-2021 cocoa season, with a relatively high standard deviation, indicating a significant income disparity among *délégués*. Some of them reported yearly earnings of up to 5,000,000 FCFA (8,255$), suggesting the presence of highly successful middlemen. Consistent with previous figures indicating that *délégués* delivered 2/3 of their annual volume during the main crop, the income generated by their cocoa trading activity during the main crop also accounts for 2/3 of their total income over the cocoa season.

Most middlemen (79%) reported difficulties purchasing cocoa from their regular farmers over the past 12 months. Transportation issues due to poor road conditions (77%) and the lack of operational means of transport (46%) are the main difficulties experienced by traders, along with changes in the farm-gate cocoa price over the cocoa season (70%). Additionally, 31% faced financial difficulties when purchasing cocoa. In fact, 30% of *délégués* receive no cash advance from their cooperative, and 80% of those who do receive such an advance report that the money lent by the cooperative is never or rarely sufficient to cover all their cocoa purchases. In such cases, 78% of middlemen encountering cash shortages opt to pay farmers at a later time, and 32% use money from their personal savings. Middlemen face significant transportation-related costs, including fuel, vehicle expenses, road tolls, and driver payments. Moreover, 41% employ external helpers, incurring additional salary expenses. Storage costs also vary, with 42% owning or renting their own facilities. On average, operating as

[3] Covering October 2020 to September 2021.

a middleman cost participants 951,533 FCFA over the past 12 months. Middlemen also take on the role of credit providers to farmers, with 92% of them reporting that they lend money to the farmers they source cocoa from. Among these, 35% lend to up to 25% of their farmers, and 47% to up to 50%. Despite 93% of middlemen having a mobile money account, only 13% use it to pay cocoa farmers. Most prefer cash transactions (90%) as they are untraceable, and 48% report that farmers prefer cash payments as well.

4 Cheating behavior of cocoa middlemen

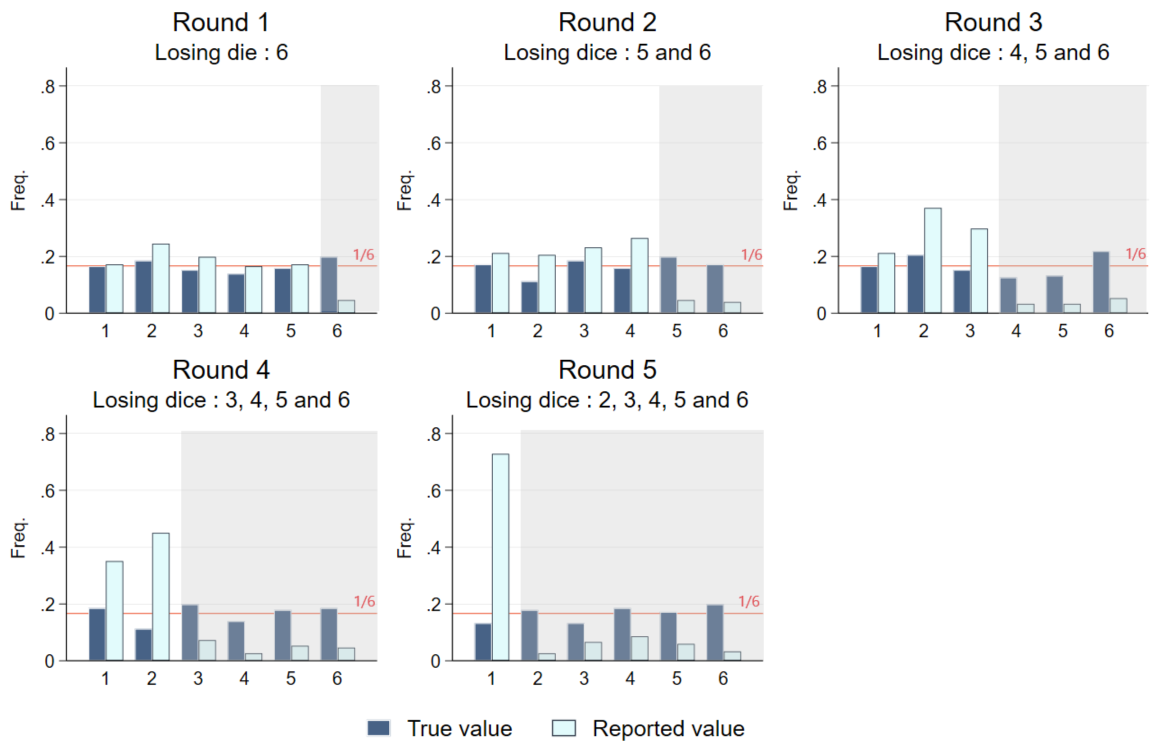
4.1 Cheating rates

Our final sample consists of 2,265 observations, corresponding to 15 rounds played by each of the 151 middlemen. The digital dice used by the players are fair, as they had an equal number of losing and winning rounds (an average of 7.7 losing rounds), and the distribution of the real die-roll outcomes, recorded on the tablet, closely approximates the theoretical distribution of a random dice roll (represented by the red line in Figure 2).

Deviation from theoretical distribution. Figures 2, D1, and D2 show for each treatment the difference between the distribution of actual dice outcomes captured by the tablet and the outcomes reported by players. The grey area indicates the dice outcomes that result in a loss for the corresponding rounds, i.e., those that do not lead to a monetary win. Results show that players less frequently report numbers associated with losses, and that numbers associated with wins are therefore over-represented compared with the distribution of a fair die. The shift to the left in the distribution of outcomes reported to enumerators compared to the theoretical distribution indicates some misreporting from players.[4] When players provided the experimenter with a different number from the actual one they rolled during a losing round, they tended to report the threshold winning number, i.e. the minimum required number for them to secure a win, in 65.6% of cases (Figures 2, D1 and D2). This observation provides further descriptive evidence of cheating, strongly suggesting

[4]Note that in a negligible proportion of rounds (0.5%), the reported number is higher than the one obtained, with a significant difference between winning rounds (1%) and losing rounds (0%). Over-reporting is not rational, as it decreases the probability of earning some money. It may be attributed to a lack of attention or reporting errors by the player or enumerator. About 4.6% of players over-report at least once during the experiment. Such errors typically occur only once per player, except for one of them who repeated this error four times over 15 rounds. Consequently, we consider over-reporting as random noise, which should not affect our analyses.

Figure 2: Distribution of true die roll outcomes and outcomes declared by players for Treatment 1



Source: Authors' calculation.

Note: The red horizontal line indicates the theoretical distribution of a random draw. This graph shows the distribution of actual and reported roll-dice outcomes for treatment 1, where the observability and penalty features are not integrated. Similar patterns are observed in treatments 2 and 3 (in Appendix).

that the numbers reported by players are not the result of a random process.

Overall cheating rates. The unconditioned cheating rate, which indicates the proportion of rounds, whether winning or losing, in which players behave dishonestly, is 39%.[5] When focusing on losing rounds, players under-report their die-roll outcome in 72.5% of cases. In contrast,

[5]This is relatively low compared with other studies involving a die-rolling task in a lab experiment. For instance, in a meta-analysis of 129 die-rolling tasks by Gerlach et al. (2019), the average proportion of individuals exhibiting dishonest behavior was 52% (CI=[47%; 57%]). A closely related study of ours is Rustagi and Kroell (2022), which investigates the role of innate honesty in explaining the variation in milk quality sold in naturally occurring markets in India, also employing a die-rolling experiment. They report that 50 percent of the milkmen exhibited dishonest behavior.

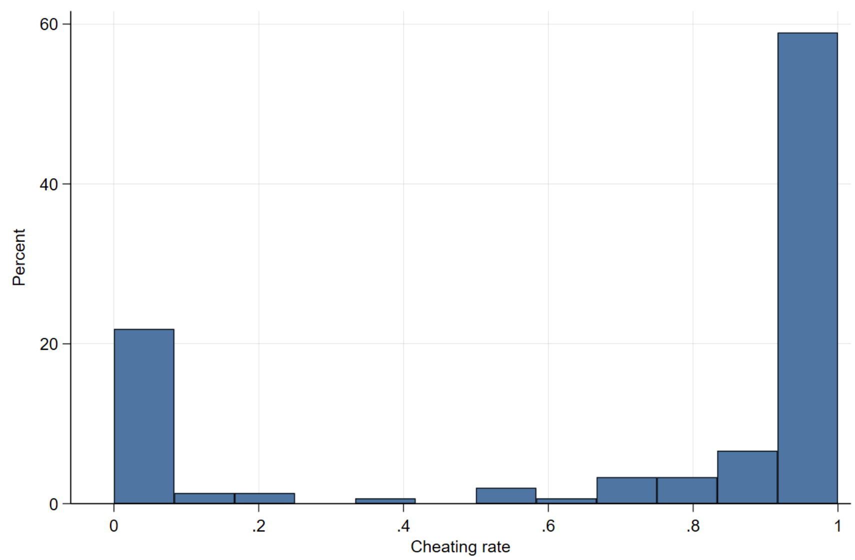
under-reporting occurs in only 3% of cases during winning rounds. The substantial occurrence of misreporting in losing rounds indicates that players were well aware of the potential for dishonesty and occasionally exploited this opportunity to increase their payouts. This discrepancy of behaviors between winning and losing rounds further stresses that the misreporting observed during losing rounds was likely a strategic choice. The individual overall cheating rate is quite high, as 78% of middlemen cheated at least once when facing a losing die. Figure D3 reinforces the notion that lying is strategic, as it shows a positive correlation between players' cheating rate over the 15 rounds and their payoffs. We further find suggestive evidence of a positive relationship between cheating behavior in the game and a preference for using cash in transactions for confidentiality reasons rather than mobile money. However, due to our small sample size, we lack the statistical power to make this result significant.

Influence of the probability of losing. The proportion of players engaging in dishonest behavior increases as the probability of losing increases over the rounds, regardless of whether it is a losing or a winning round. For instance, in round 2 (where the probability of losing is 2/6), the probability of cheating increases by approximately 11 percentage points compared to round 1 (where the losing probability is 1/6) (Table D1). In the last round, where the probability of losing is 5/6, the likelihood of cheating rises by 42 percentage points compared with the first round. A more straightforward interpretation of these findings directly considers the theoretical losing probabilities. Transitioning from a null losing probability to a certain losing probability results in an increase of 63 percentage points in the likelihood of cheating. On average, this corresponds to an increase of approximately ten percentage points for every 1/6 increment in the probability of losing (i.e., for each successive round).

4.2 Cheater profiles

Individual cheater profiles. An analysis of middlemen's behavior over the 15 rounds uncovers three distinct player profiles: those who never cheat (22%), those who cheat occasionally (19%), and those who consistently cheat when facing a losing roll-die outcome (59%) (see Figure 3). Players who consistently refrain from cheating, even when it could yield benefits, are characterized as the "ethical type" in the existing literature. In our game, 22% of the players were always honest even when it did not yield any tangible benefits, which suggests inherent honesty. For these players, lying carries infinite intrinsic costs, both direct, such as an aversion to deception, and indirect, such as guilt aversion (Battigalli et al. 2013; Battigalli and Dufwenberg 2007), an aversion to violating social norms (Kajackaite and Gneezy 2017), or a desire to maintain a positive social and/or self-image (Bénabou and Tirole 2006). Occasional cheaters suffer the same direct and indirect cost of lying, but to a limited extent: they cheat when the benefits of dishonest behavior outweigh their finite costs of lying. Although representing only 19% of our sample, this type of player is the most likely to adjust their behavior based on observability and sanction features of our game. Finally, 59% of players fall into the "economic type" category, as defined in previous studies (Gibson et al. 2013; Kajackaite and Gneezy 2017). These individuals exhibit a zero cost of lying and engage in cheating whenever it benefits them. Middlemen who consistently cheated in our settings thus earned an average gain of 6,753 FCFA, representing 38% more than the average gain of occasional cheaters (4,872 FCFA) and three times that of never cheaters (2,318 FCFA) (Table D2).

Figure 3: Distribution of individual cheating rates



Source: Authors' calculation.

Note: The overall individual cheating rate is the proportion of rounds in which individuals under-report the dice outcome relative to the proportion of losing rounds.

Cheater profile determinants. We explore the effects of players' characteristics on their cheating profile, i.e., whether they never cheat, sometimes cheat, or cheat consistently, using a multinomial logistic regression with the reference category being players who "never" cheat. Table 2 shows that few socio-demographic characteristics influence cheating behavior. We find that age is associated with a 10% reduction in the probability of being classified as "always cheaters" compared to "never cheaters" (column 2), which is in line with previous findings (Gerlach et al. 2019). While the level of education has no significant effect on the player's cheating profile, religion seems to play a major role in the decision to engage in dishonest behavior. Compared with Animists, we find that the probability of being an "occasional cheater" and an "always cheater", compared with being a "never cheater", was respectively reduced by 94% and 92% when the player was Muslim. Christian players, compared to Animists, are also 94% less likely to belong to the "always cheaters" category. These results align with field observations and qualitative discussions with players, as some cocoa middlemen who refused to take part in the actual game or indicated that they had never cheated during the game mainly mentioned religious considerations.

We further find that players' risk and time preferences influence cheating behavior. An inclination towards risk significantly increases the probability of being a 'sometimes cheater' or an 'always cheater,' making it the primary individual determinant of cheating. This finding can be expected and is consistent with the hypothesis that risk-lover individuals engage in more risky behavior to maximize their payoffs. We further find a significant and negative relationship between the subjective discount factor and the probability of always cheating when facing a losing round, compared to never cheating. Conversely, this means that players who value the present (and therefore exhibit a low subjective discount factor) are more likely to cheat and belong to the category of always cheaters.

Cheating behavior could also be associated with various factors related to middlemen's activities, including their income as cocoa middlemen, their role as main middlemen, and the number of years of professional experience as cocoa middlemen. One might think that a middleman with more experience and a higher income would have less incentive to cheat to increase his potential earnings. However, our findings do not provide significant evidence supporting this hypothesis.

Whether the cheating behaviors measured in this study correlate with middlemen's real-life behavior remains uncertain. Without real-life traceability measures of dishonest behavior among middlemen, we cannot provide a definitive answer. However, middlemen who never cheated during the game are more likely to pay cocoa farmers by mobile money (23%) compared to those who always cheated (8%) (Table **??**). Similarly, those in the 'never cheaters' category are significantly less likely to prefer cash to mobile money for confidentiality reasons (78%) compared to occasional cheaters (96%) and always cheaters (92%).

Table 2: Individual determinants of cheating

	Relative risk ratios	
	(1)	(2)
	Sometimes cheater	Always cheater
Socio-demographic characteristics		
Age	0.940	0.900*
	(0.041)	(0.040)
Primary education	1.025	0.688
	(1.140)	(0.672)
Middle school and higher	1.666	2.104
	(2.019)	(2.108)
Ivorian	1.075	1.196
	(1.217)	(1.383)
Christian	0.081	0.057*
	(0.118)	(0.078)
Muslim	0.059*	0.077*
	(0.074)	(0.095)
Middleman's activity characteristics		
Income from cocoa	1.005	0.888
	(0.131)	(0.106)
Main trader	0.330	0.640
	(0.248)	(0.414)
Years of experience as cocoa middleman	1.029	1.044
	(0.030)	(0.027)
Risk and time preferences		
Subjective discount factor	0.126	0.052**
	(0.138)	(0.053)
Risk-lover	8.033*	8.594*
	(8.077)	(8.861)
Enumerators FE	Yes	Yes
Region FE	Yes	Yes
N	142	142
Pseudo R2	0.349	0.349

Source: Authors' calculation.

Note: Reported coefficients come from multinomial logistic regressions, using "Never cheater" as the reference category. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

5 Effect of observability and sanction among occasional cheaters

The second part of this paper seeks to investigate the effect of introducing observability and financial penalties on players' cheating behavior. We hypothesize that the likelihood of cheating decreases under the following conditions: (1) the presence of an external observer (i.e., the experimenter) able to sometimes monitor players' dishonest behaviors, and (2) the potential of penalties if the experimenter detects cheating. Previous literature highlights that being observed may be enough to reduce players' cheating behavior, as they could be concerned about being seen as displaying socially undesirable behavior in the eyes of the experimenter (Bašić and Quercia 2022; Fries et al. 2021). To our knowledge, no previous study tested the combined effect of observability and penalty.

5.1 Empirical specification

In the previous section, we observed that a non-negligible proportion of players never cheated to secure higher payoffs, even in the absence of observability and sanctions. On the other hand, a significant proportion of players always cheated, even in the presence of observability and sanctions, indicating that these game features did not influence these players, or were not sufficiently disincentive to alter their cheating behaviors. In this section, we are therefore interested in the sub-sample of players who exhibited heterogeneous cheating behavior across treatments, and how the introduction of observability and financial penalties influences their behavior.

We perform the following conditional logistic regression with player fixed effects:

$$Cheating_{ir} = \beta_1 + \beta_2 Treatment2_i + \beta_3 Treatment3_i + \beta_4 \alpha_i + X_{ir} + \delta_i + \epsilon_i \quad (1)$$

where $Cheating_{ir}$ is a dummy variable taking the value of 1 if the player i cheated during the round r, and 0 otherwise. *Treatment2* and *Treatment3* are dummies indicating the treatment being played. In some specifications, we also include α, which is a vector of variables related to players' previous game experience. δ_i stands for individual fixed-effects. X_{ir} is a set of round control variables, which include the round number and the lottery selected during the Holt and Laury first phase of the game, to respectively control for the probability of losing and the risk attitude. As a robustness check, we also provide results in the Appendix using the linear probability model (Table E1) and extending the same specification to the full sample (Table E2).

5.2 Findings

Threat of observability and sanction. We first explore whether the mere introduction of observability and observability coupled with penalties for cheating, inhibits dishonest behavior in our game. In treatment 2, players face the possibility of experimenters directly monitoring the true outcome of their die roll on tablets without penalty. After rolling the die on their tablets, players physically roll a die to determine whether the experimenter will observe their actual outcome. This risk is 50%, as the experimenter observes the player's reported result only if the second roll's outcome is even. Since not all individuals exhibit social-image concerns (resulting in being observed by a third party having no impact on their cheating behavior) (Huber et al. 2023), the combination of observation with a financial penalty could potentially alter the players' cost-benefit analysis of cheating (Thielmann and Hilbig 2018). In treatment 3, players are thus confronted with the risk of getting a financial penalty if the experimenter observes a discrepancy between the number they reported and the actual outcome displayed on the tablet, following the same rules as in treatment 2.

Table 3 (column 1) presents findings indicating that introducing the mere risk of behavior monitoring decreases players' likelihood of cheating. Specifically, players are 41.5% less likely to cheat in treatment 2 than in treatment 1. This likelihood is further reduced when observability risk is combined with a penalty risk in the event of cheating, as they are 51.6% less likely to cheat in treatment 3 compared to treatment 1.

Previous rounds history. Considering the game's iterative nature with fifteen rounds, we argue that players' behavior and outcomes in previous rounds are likely to influence their decisions in subsequent rounds. We look at the player's game history in previous rounds, considering both previous losses and cases of cheating, to explore how these factors may influence cheating behavior in the current round.

The "previous loss rate" indicates the number of times a player has experienced losses relative to the total number of previous rounds, while "lost in previous rounds" is a dummy variable indicating whether the player had already lost at least once in previous rounds. A positive effect would suggest that players are likely to engage in cheating after a series of losing rounds, indicating a potential compensation effect to secure a win. Results in Table 3 (columns 2 and 3) reveal a positive but non-significant association between the previous loss history and the likelihood of cheating in the current round among occasional cheaters.

Table 3: Effect of history loss, previous cheating, and game features on cheating among occasional cheaters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment 2	0.585*	0.566*	0.505*	0.724	0.705					
	(0.158)	(0.158)	(0.171)	(0.221)	(0.240)					
Treatment 3	0.484**	0.463**	0.387*	0.589	0.585	0.845	0.912	1.421	2.421*	
	(0.132)	(0.135)	(0.160)	(0.179)	(0.202)	(0.233)	(0.289)	(0.460)	(0.930)	
Previous loss rate		1.762								
		(2.200)								
Lost in previous rounds			1.336							
			(0.533)							
Previous cheat rate				0.566						
				(0.225)						
Cheated in previous rounds					0.683					
					(0.295)					
Previous observation rate						0.451				
						(0.326)				
Observed in previous rounds							0.723			
							(0.392)			
Previous cheating observation rate								0.058***		
								(0.046)		
Observed while cheating in previous rounds									0.113***	
									(0.060)	
Number of times players get a sanction										0.060***
										(0.047)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample selected on Treatments #	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	2, 3	2, 3	2, 3	2, 3	3
N	435	435	435	435	435	270	270	270	270	110
R2	0.136	0.137	0.138	0.141	0.138	0.110	0.106	0.166	0.182	0.331

Source: Authors' calculation.

Note: Reported coefficients are odd-ratios from conditional logistic regression. Controls include the selected lottery and round number. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Additionally, we consider the "previous cheat rate", which is the ratio of previous cheats to the number of times they faced a losing die, and a binary variable indicating whether the player has previously cheated in rounds. Although the relationship is not statistically significant, our results suggest a negative association between having cheated in previous rounds and the likelihood of cheating in the current round (Table 3, columns 4 and 5). Specifically, the probability of cheating decreases by 31.7% when the player has previously cheated at least once. This pattern could be explained by players feeling comfortable cheating occasionally but not frequently, perhaps fearing being perceived as regular cheaters by the experimenter or being concerned that cheating more often would increase their risk of getting caught.

Previous observability and penalty. We further hypothesize that previous game experiences, such as having been observed and having been observed while cheating, are likely to influence the player's cheating behavior in the current round. We thus examine the influence of having been observed in previous rounds using the previous observation rate, which corresponds to the number of times a player has been observed relative to the number of rounds, and a dummy indicating whether the actual outcome of the players' die-roll has already been observed in previous rounds. Similarly, we look at the previous cheating observation rate, which is the number of times a player was observed cheating out of the number of times they cheated, and a dummy indicating whether the player has already been observed while cheating.[6]

When investigating the effect of having actually been observed by the experimenter in previous rounds (columns 6 and 7), whether they were observed cheating or reporting the true result, we find no significant impact on players' behavior in the current round. Although the relationship's direction aligns with expectations (i.e., less cheating when the player has been observed by the

[6] It is worth noting that, as treatment order was not randomized, we acknowledge a potential order effect regarding cheating behaviors in treatments 2 and 3. More specifically, if treatment 3 (observability and penalties) occurred before treatment 2 (observability), we argue that players would have been less likely to cheat in session 2 due to their previous experience in treatment 3 involving penalty. We therefore interpret the proportion of players who cheat in session 2 as a higher bound. Similarly, the fact that treatment 3 always comes after treatment 2, i.e. after players have already experienced observability, may have an effect on players' behavior in treatment 3. The direction of this effect is likely to vary according to player profile: a player who was sensitive to social image concerns is likely to have cheated less during treatment 3 if he has already been observed cheating in treatment 2. A player who was not sensitive to social image concerns is unlikely to have been affected in treatment 3 after having played under treatment 2.

experimenter), it is not statistically significant. In contrast, we observe a negative and statistically significant relationship regarding the probability of cheating when players have been observed cheating in previous rounds (columns 8 and 9). Having been observed while cheating reduces the probability of cheating in the current round by 89%. When considering being observed cheating at least once in previous rounds, players appear to cheat more in treatment 3 than in treatment 2. Although not statistically significant, there is a shift in the relationship direction. We suggest that this might be explained by the fact that when players have already been observed cheating once, their concern about damaging their social image decreases, leading them to take any opportunity to increase their payoffs.

Finally, we explore the impact of having received a penalty during treatment 3, using the number of previous instances in which the player received a penalty (column 10). Financial penalties appear to decrease the likelihood of cheating when enforced. Specifically, our findings indicate that in the third treatment, the more penalties players have incurred in previous rounds, the less likely they are to cheat in the subsequent ones.

6 Conclusion

Pressure on cocoa-producing countries and companies to improve cocoa traceability has been growing steadily recently.[7] However, achieving traceability at the farmer level is a major challenge, given that the cocoa value chain in Côte d'Ivoire is made up of a large number of geographically dispersed players whose relationships are fraught with information asymmetry. Cocoa middlemen, the intermediaries linking cocoa farmers and cooperatives, play a key role in traceability as being the first to supply cocoa into the supply chain. However, middlemen are likely to engage in opportunistic behavior and misreport the nature of the cocoa they bring to the cooperative (whether certified or conventional), to increase their profits. Yet, middlemen are currently excluded from the monitoring and traceability mechanisms in place in the cocoa sector, despite their crucial role in the supply chain.

[7] Import restrictions on a number of commodities, including cocoa, are becoming stricter in consumer markets. For example, the European Union recently banned the import of commodities derived from deforestation (see the Deforestation Regulation (EU) 2023/1115, which came into effect on June 29th, 2023) and has agreed on a new regulation aimed at banning the import of products that have involved forced labor (see the Proposal for a regulation on prohibiting products made with forced labor (COM(2022)0453 – C9-0307/2022 – 2022/0269(COD))).

This paper relies on a lab-in-the-field involving an experimental cheating game with 151 cocoa middlemen operating in certified cocoa value chains, to investigate middlemen's dishonest behavior in situations of information asymmetry. This experimental approach, an adapted die-under-cup task, allows us to control the environment related to the decision to misbehave, thereby separating individuals' intrinsic (dis)honesty from other confounding motives. We argue that this approach provides an effective way to measure middlemen's propensity to engage in opportunistic behavior, given the challenges of using traditional observational or survey data, as these behaviors are hidden and subject to social desirability bias. Besides investigating whether middlemen do engage in opportunistic behavior and to what extent, we investigate the key determinants associated with such dishonest behavior. Our experimental design also provides an opportunity to assess whether middlemen behave differently when they are observed and sanctioned for being caught cheating, mechanisms that could be implemented in real life as part of improved traceability monitoring.

We find that 78% of middlemen cheated at least once when they had a losing die, to maximize their payouts. Using individual data from each player over the 15 rounds, middlemen are classified into different categories based on their cheating behavior. We find that 59% of players consistently lie, while 19% do so occasionally. Remarkably, 22% of cocoa middlemen never lie, which is in line with previous experimental studies showing that some players consistently prioritize honesty, even when presented with opportunities to increase profits without consequences (Gerlach et al. 2019; Kajackaite and Gneezy 2017; Rustagi and Kroell 2022). Few individual characteristics, such as age, religion, and attitudes toward time and risk, influence cheating behavior. Further, we show that the simple threats of being observed and of getting a financial penalty for cheating induce a reduction in the probability of cheating among middlemen with heterogeneous cheating behaviors over the course of 15 rounds (i.e., the occasional cheaters). The increased costs associated with social image when there is a risk of being observed have thus a strong inhibiting effect, even when the observation has not actually taken place, and even in the absence of sanction. Being observed in previous rounds has no deterrent effect on cheating beyond the mere threat, except when players have been caught cheating and/or sanctioned for it. Financial penalties work, as the probability of cheating decreases considerably with the number of times players are sanctioned. However, whether this is due to a social sanctioning effect that discourages players from cheating after being caught once, concerns about further damaging their social image by repeatedly being observed cheating, or fears of increased penalties, remains an open question.

Whether monitoring systems should be extended to middlemen to reduce side-sourcing in the certified cocoa sector is a question that goes beyond the scope of this study. Our paper presents evidence of middlemen engaging in opportunistic behavior in a controlled environment, and presents some limitations. First, although representative of middlemen in the certified cocoa sector in Côte d'Ivoire, our sample is rather small, which may affect the statistical power of our results. One should also keep in mind that a few middlemen who attended the game's information sessions decided not to take part in the experiment, thus probably excluding from our sample the most risk-averse individuals. Second, while we provide evidence that observation and sanction mechanisms are effective in reducing dishonest behavior among certain types of players (the ones rational in the sense of Becker (1968), i.e., those adjusting their behavior toward honesty when the costs of cheating exceed the benefits), the actual proportion of middlemen falling into this category remains uncertain. Although this group of "occasional cheaters" represents only 19% of the players in our experiment, suggesting that such monitoring and sanction mechanisms may be ineffective with most middlemen, it is likely that this proportion is inherent on game design. Indeed, the probability of being observed and the magnitude of financial penalties remain constant in the experiment. Yet, it is likely that a higher penalty or a greater likelihood of incurring penalties could have influenced the cost-benefit analysis for the "always cheaters" who might have fallen into the "occasional cheaters" category. Further, while labs-in-the-field allow for the unbiased measurement of cheating behavior without the influence of confounding factors, we acknowledge that predicting the real-life impact of monitoring and sanction mechanisms remains challenging. Various factors, including repeated interactions with farmers and cooperatives, may constrain middlemen's cheating behaviors in real-life scenarios. These dynamics introduce trust and reputational considerations which may influence middlemen's likelihood to cheat (Banerjee and Duflo 2000; Macchiavello and Morjaria 2015). In light of such complexities, it is likely that a greater proportion of "occasional cheaters", who are responsive to monitoring and sanctions, are found in real-life situations than what our experimental game may capture.

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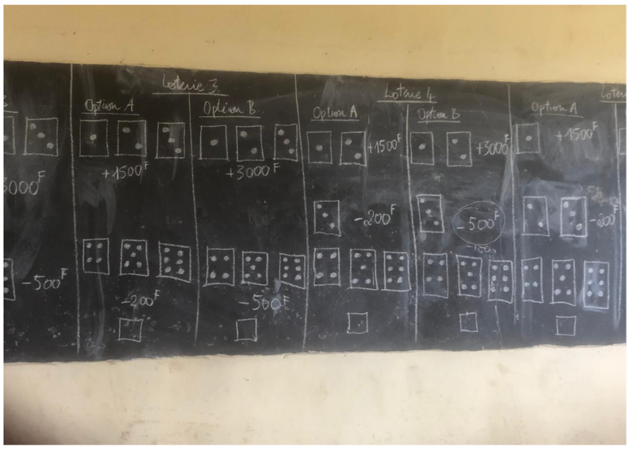
Appendix

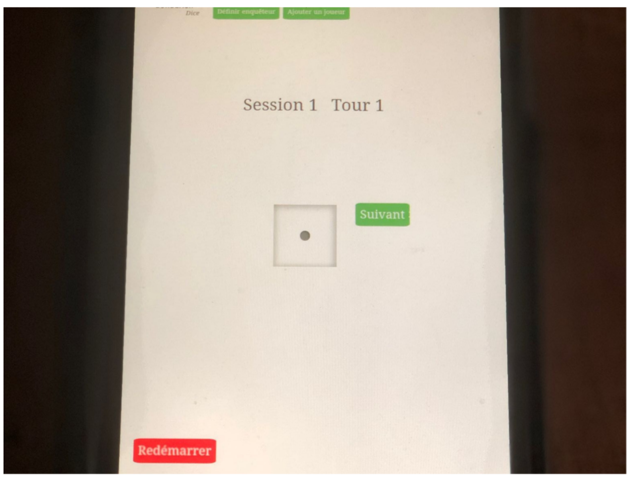
Appendix A : Lab-in-the-field experiment protocol

Figure A1: Lab-in-the-field experiment setting and tools



Visual aids displaying the amounts with each decision

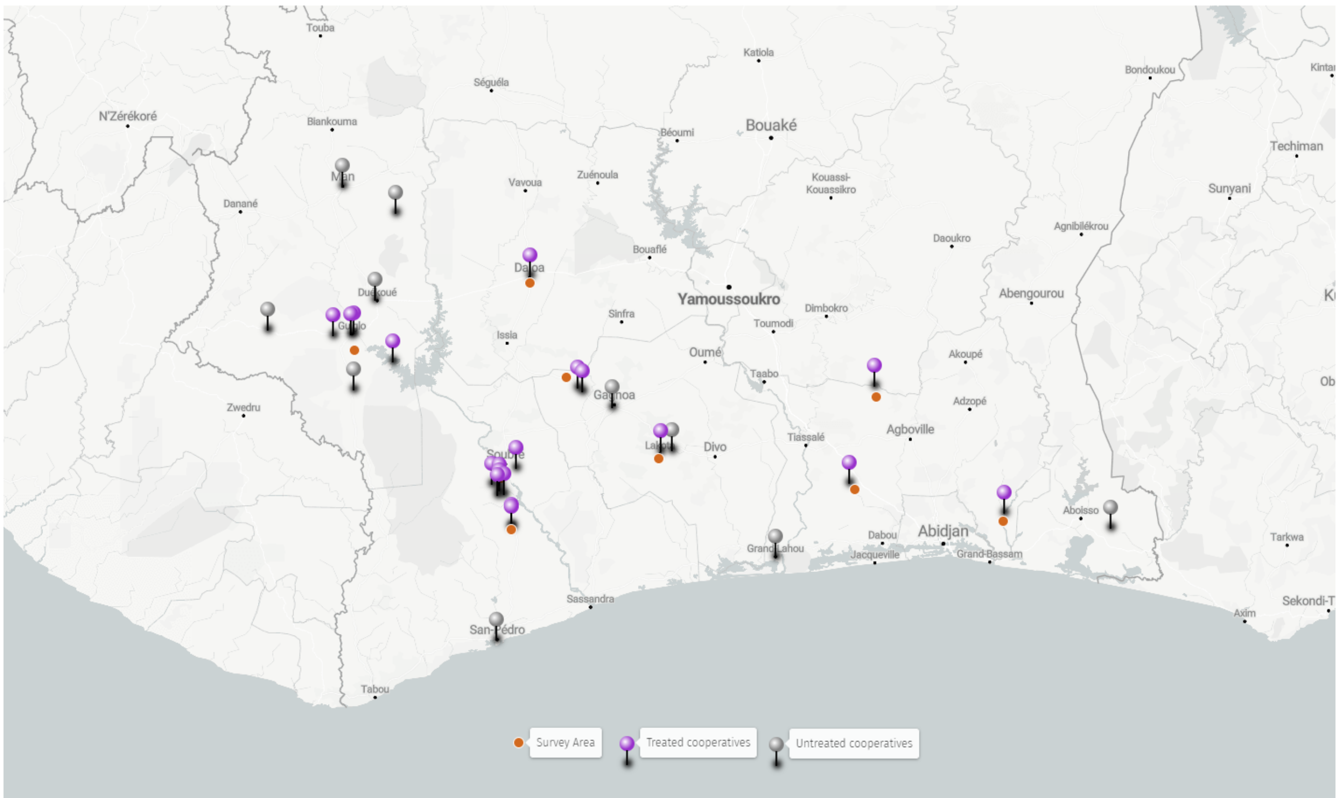

Typical session



Game for player

Observation and penalty tools

Figure A2: Survey areas



Experimenter sheet

Player n°

Lottery choices

Report here the options chosen by the player.

	Option A	Option B
Lottery 1	☐	☐
Lottery 2	☐	☐
Lottery 3	☐	☐
Lottery 4	☐	☐
Lottery 5	☐	☐

Double-check with the player to ensure there are no mistakes before proceeding to the next part.

Session 1 : Basic die roll

The player rolls a die on the tablet. Record the number declared by the player here. Repeat the procedure for each round.

Round number	Outcome declared by the player
Round 1	
Round 2	
Round 3	
Round 4	
Round 5	

The player will draw the round number selected for payment.

The round selected for payment is round number:	
For this session, the middleman lost/won:	

Session 2 : Die roll with observation risk

The player rolls a die on the tablet. Record the number declared by the player here. Repeat the procedure for each round.

The player will now roll a physical die: if the outcome is an even number (2, 4, or 6), move to observe the roll-die outcome on the player's screen and indicate the observed number in the second column. If the roll-die outcome is an odd number (1, 3, or 5), do not move and make a cross in the corresponding box.

Round number	Outcome declared by the player	Outcome observed by the enumerator (if 2, 4, 6)
Round 1		
Round 2		
Round 3		
Round 4		
Round 5		

The player will draw the round number selected for payment.

The round selected for payment is round number:	
For this session, the middleman lost/won:	

Session 3 : Die roll with observation and penalty risks

The player rolls a die on the tablet. Record the number declared by the player here. Repeat the procedure for each round.

The player will now roll a physical die: if the outcome is an even number (2, 4, or 6), move to observe the roll-die outcome on the player's screen and indicate the observed number in the second column. If the roll-die outcome is an odd number (1, 3, or 5), do not move and make a cross in the corresponding box.

Repeat the procedure for each round.

Round number	Outcome declared by the player	Outcome observed by the enumerator (if 2, 4, 6)
Round 1		
Round 2		
Round 3		
Round 4		
Round 5		

The player will draw the round number selected for payment.

The round selected for payment is round number:	
For this session, the middleman lost/won:	

CONSENT FORM

To be read to the respondent before the experimental game

The experimental game will mainly focus on your risk attitude. No specific skills are required for participating in this game. Your games choices and payouts will remain confidential. The experiment will be divided into 4 parts. In the first part, you will choose the loterries you prefer to play. There are no correct or incorrect choices; you should make your decisions based on your best judgment. In the second, third, and fourth parts, you will play the game based on the lotteries you selected by rolling a die. A test game will be conducted to explain the rules, during which you can ask any questions you may have. Instructions will be reiterated at the beginning of each session.

An experimenter will be present to record your lottery choices in the first part and the results of your die rolls in the second, third, and fourth parts of the game. At the end of each game session, one round will be randomly selected for payment. At the end of the experiment, your payouts will be paid on your Mobile Money account. Your payouts are contingent upon the lotteries you choose in the first part of the experiment, and the random outcomes of your die rolls. You have already received 6,000 FCFA for your participation in the survey. By participating in this game, you can earn a maximum of an additional 9,000 FCFA. Nevertheless, there is a possibility that you may incur losses; in the worst-case scenario, you could lose up to 6,000 FCFA.

The information you provide will help us gain a better comprehension of cocoa middlemen activity and, consequently, a more comprehensive understanding of the cocoa value chain in Côte d'Ivoire. The information we collect will remain confidential and will under no circumstances be disclosed to third parties or to the cooperative. The data collected will be encrypted and anonymised by researchers from the University of Bordeaux, within the strict limits of research activities and with due respect for individual privacy. The data and encryption key will not be communicated to any third parties. Survey findings will be published in academic journals.

Your participation in this experiment is entirely voluntary. You have the option to decline participation, and you can stop the experiment at any point without any consequences. If you have questions about the study at any time, you may reach out for clarifications.

In compliance with applicable laws and regulations, you have the right to access, rectify, obtain a copy of, or delete your data, as well as to restrict its processing or object to it. You can use these rights by contacting us via email at the following address: [redacted]@u-bordeaux.fr.

CONSENT FORM (2 copies, one for the participant, one for the experimenter)

Note: This consent form was originally written in French and will be provided to study participants after translation into the local language.

I, the undersigned _______________________ (the participant), confirm that:

- I have been informed of the above details, which have been thoroughly explained to me.
- I have a clear understanding of this information.
- I have had the opportunity to seek clarification and received satisfactory answers to all my questions.
- My participation in the experiment is entirely voluntary.
- I have received a copy of this informed consent form.
- I am aware that I can contact one of the study's coordinator [redacted] if I have any questions about the study or if I wish to stop the experiment.

Date /___/___/___/ Location ____________________

Player signature	Signature of the person who obtained consent

Player n°

Which of these two options do you prefer?
Tick the one you prefer and let the enumerator know.

Lottery 1

Option A		Option B	
⚀ ⚁ ⚂ ⚃ ⚄	1 500 FCFA	⚀ ⚁ ⚂ ⚃ ⚄	3 000 FCFA
⚅	-1 000 FCFA	⚅	- 2 000 FCFA
☐ **Option A**		☐ **Option B**	

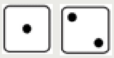
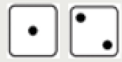
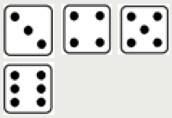
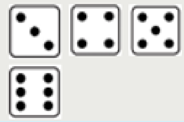
Lottery 2

Option A		Option B	
⚀ ⚁ ⚂ ⚃	1 500 FCFA	⚀ ⚁ ⚂ ⚃	3 000 FCFA
⚄ ⚅	-1 000 FCFA	⚄ ⚅	- 2 000 FCFA
☐ **Option A**		☐ **Option B**	

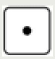
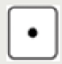
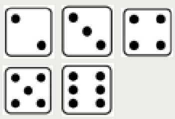
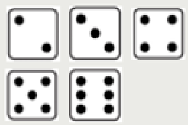
Lottery 3

Option A		Option B	
⚀ ⚁ ⚂	1 500 FCFA	⚀ ⚁ ⚂	3 000 FCFA
⚃ ⚄ ⚅	-1 000 FCFA	⚃ ⚄ ⚅	- 2 000 FCFA
☐ **Option A**		☐ **Option B**	

Lottery 4

Option A		Option B	
	1 500 FCFA		3 000 FCFA
	-1 000 FCFA		- 2 000 FCFA
☐ Option A		☐ Option B	

Lottery 5

Option A		Option B	
	1 500 FCFA		3 000 FCFA
	-1 000 FCFA		- 2 000 FCFA
☐ Option A		☐ Option B	

Appendix B : Test for selection bias in game participation

Table B1 aims to detect potential self-selection biases between middlemen who agree to participate in the game (151 individuals) and those who opted out (64 individuals). We compare the two groups' key demographic, work-related, and time-preference variables. For each variable, the table presents the number of observations (N), mean value, and standard deviation for both groups. Subsequent columns highlight the raw difference in means between the two groups, the normalized difference according to Imbens and Rubin (2015) for assessing the magnitude of this difference independent of sample size,[8] and a p-value to test the statistical significance of the observed differences.

On average, those who refused to participate are older than those who accepted, with a normalized difference of 0.45 and a p-value of 0.04, and less educated, with a normalized difference of -0.27 and a p-value of 0.04. There is no significant difference in terms of marital status and religion. We cannot find any difference in work-related characteristics and time preferences, as all the variables exhibit low normalized differences and high p-values. However, we do find a significant difference between participants and non-participants in terms of risk attitudes. Non-participants are more likely to be risk-averse, positively correlated with never cheating in our regressions. Thus, we are likely to overestimate the proportion of players cheating consistently. Therefore, our analysis results are a lower bound of honest behavior.

[8] Normalized difference is used to assess the balance of observed covariates between two distinct groups. The advantage of using normalized differences over traditional p-values from statistical tests (such as t-tests) is that sample size does not affect normalized differences. Traditional tests might fail to detect imbalances in small samples, like ours, due to low statistical power.

Table B1: Balancing test comparing individuals who accepted vs. those who declined participation in the game

	(1) Accepted to participate			(2) Refused to participate			(2) - (1) Difference		
	N	Mean	St. Dev.	N	Mean	St. Dev.	Raw diff.	Norm. diff.	P-value
Demographics									
Age	151	39.40	9.16	64	43.66	9.71	4.26	0.45	0.04
Married	151	0.92	0.27	64	0.97	0.17	0.05	0.22	0.50
Educated	151	0.83	0.37	64	0.72	0.45	-0.11	-0.27	0.04
Christian	151	0.46	0.50	64	0.33	0.47	-0.13	-0.27	0.10
Muslim	151	0.40	0.49	64	0.48	0.50	0.08	0.16	0.27
Work									
Years of experience as a trader	151	21.58	15.07	64	20.59	15.92	-0.99	-0.06	0.23
Worked in the cocoa sector before being a trader	151	0.90	0.30	64	0.87	0.33	-0.03	-0.06	0.08
Receive help from coxeurs	151	0.40	0.49	64	0.36	0.48	-0.04	-0.08	0.65
Delivered cocoa volume (tons)	151	89.51	95.92	64	86.65	111.40	-2.86	-0.03	0.63
Overall declared income (ln)	133	13.85	1.01	51	13.78	1.06	-0.07	-0.07	0.16
Always experience cash shortages	151	0.30	0.46	64	0.33	0.47	0.03	0.06	0.95
Provide credit to farmers	151	0.92	0.27	64	0.91	0.29	-0.01	-0.04	0.46
Own storage facility	149	0.41	0.49	64	0.53	0.50	0.12	0.24	0.10
Time and risk preferences									
Subjective discount factor	151	0.59	0.32	64	0.58	0.33	-0.01	-0.03	0.59
Risk-lover	151	0.46	0.50	23	0.09	0.29	-0.37	-0.91	0.00

Source: Authors' calculation.

Note: Norm. Diff. stands for normalized differences and were assessed using t-tests following the method of Imbens and Rubin (2015).

Appendix C : Descriptive statistics

Table C1: Socio-economic characteristics of traders

	Obs.	Mean (%)	Std. Dev.
Socio-demographic characteristics			
Trader's age			
Age	151	39.39	9.16
35 years or less	151	35.10	
Between 36 and 45 years	151	41.72	
More than 45 years	151	23.18	
Marital status			
Married/in-relationship	151	92.05	
Widower	151	1.32	
Single	151	6.62	
Education level			
No education	151	19.21	
Primary education	151	27.15	
Middle school	151	24.50	
High school or higher	151	29.14	
Household size			
Number of adults	151	4.48	3.48
Number of children	151	4.40	2.60
Religion			
Christian	147	46.94	
Muslim	147	41.50	
Animist	147	11.56	
Risk and time preferences			
Risk-lovers	151	38.41	
Subjective discount factor	151	0.59	0.32
Working characteristics			
# of years since trader started working for current cooperative	151	4.30	2.54
# of years of experience as cocoa trader	151	21.58	15.07
Had worked in the cocoa sector before its current position	145	93.79	
# of years of experience in the cocoa sector	151	24.42	8.20
Trader is also a cocoa farmer	151	90.07	
Cocoa trader as main activity	151	39.73	

Source: Authors' calculation.

Table C2: Characteristics of cocoa traders' activities

	Obs.	Mean (%)	Std. Dev.
Business size			
Number of farmers	151	113.67	85.84
Cocoa delivered during 20-21 cocoa season (in tons)	151	85.57	84.33
Use of assistants (coxeurs)	151	40.40	
Performance			
No objective set by the cooperative	151	49.01	
Failed to meet objectives	77	89.61	
Met or exceeded objectives	77	10.39	
Income			
Commission per kilos during main crop (CFA)	151	34.07	26.94
Income from cocoa trader's activity (CFA 1 000)	146	1,430	1,362
Costs			
Fuel	151	88.08	
Vehicle leasing or purchase	151	28.48	
Vehicle maintenance	151	17.88	
Road tolls	151	66.89	
Driver salaries	151	66.22	
Coxeurs salaries	151	32.45	
Average costs (CFA 1 000)	146	938.47	990.60
Challenges in buying cocoa			
Faced difficulties in the last 12 months	151	78.81	
Lack of operational means of transport	119	46.22	
Poor road conditions	119	76.47	
Farm-gate price fluctuations	119	70.59	
Lack of money to buy cocoa	119	30.25	
Cash shortage			
The cooperative does not give cash advances	151	29.80	
Always cash shortage	151	30.46	
Often cash shortage	151	25.83	
Sometimes cash shortage	151	0.66	
Rarely cash shortage	151	1.99	
Cocoa storage			
Own/rent a warehouse to stock cocoa	151	41.72	
Use the cooperative's warehouse	151	19.21	
Use storage facilities provided by the cooperative (in communities)	151	39.07	

Source: Authors' calculation.

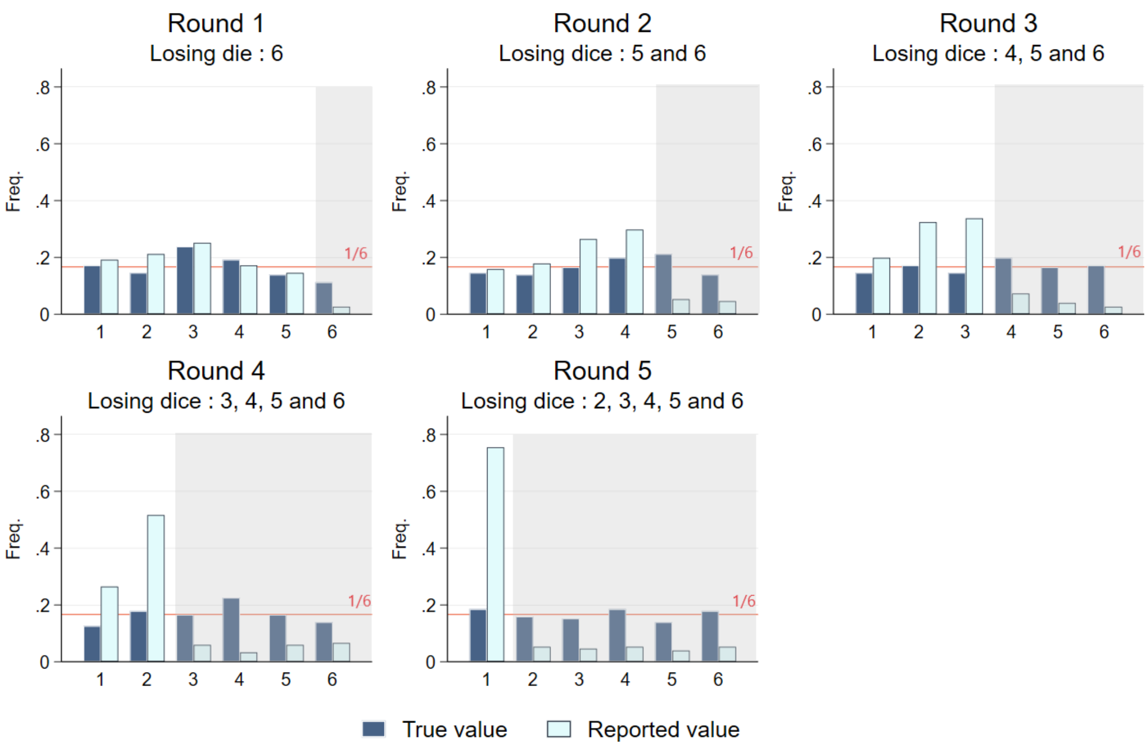
Table C3: Traders' relationship with farmers

	Obs.	Mean (%)
Loans		
Provides loans to farmers	151	92.05
Proportion of farmers to whom the trader lends money		
0-25%	139	35.25
More than 25% and 50%	139	47.48
More than 50% and 75%	139	11.51
More than 75%	139	5.76
Reasons for lending money to farmers		
School fees	139	89.21
Agricultural input purchases	139	55.39
Ceremonial purposes (e.g. funeral)	139	46.76
Medical costs	139	85.61
Food purchases	139	28.06
Mobile money		
Trader sometimes pays farmers by mobile money	141	12.06

Source: Authors' calculation.

Appendix D : Additional experimental results

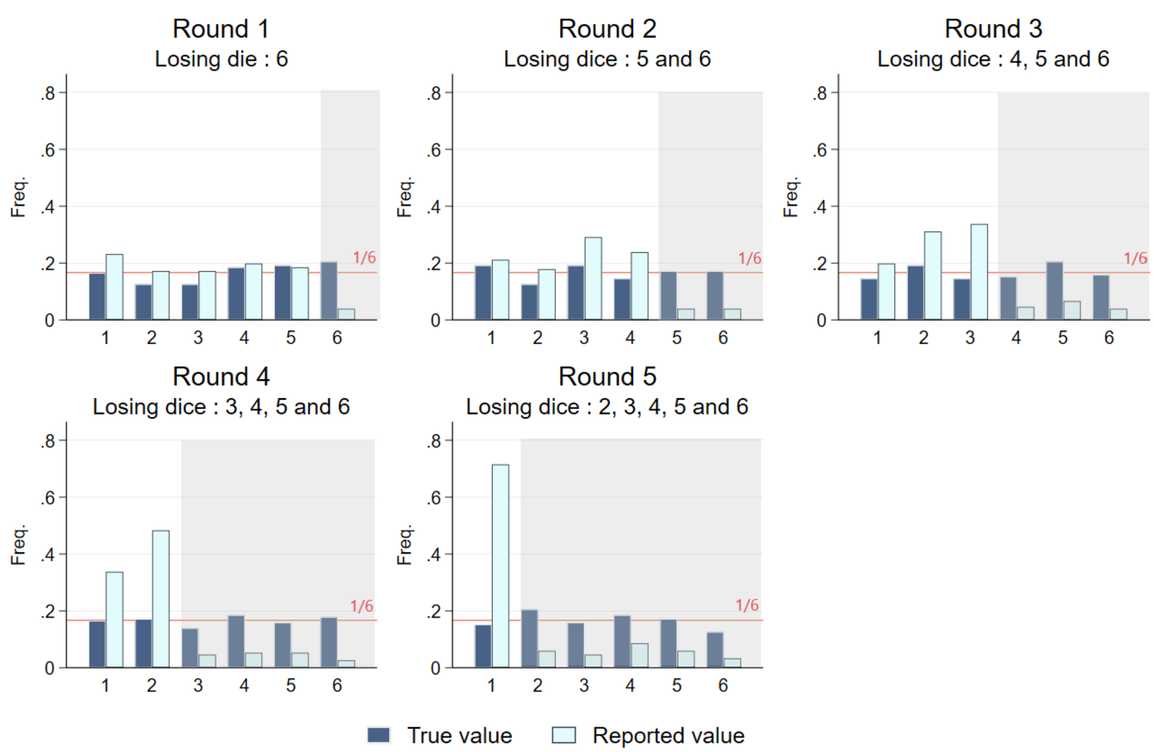
Figure D1: Distribution of true die roll outcomes and outcomes declared by players (Treatment 2)



Source: Authors' calculation.

Note: The red horizontal line indicates the theoretical distribution of a random draw. This graph shows the distribution of actual and reported roll-dice outcomes for treatment 2, where players face the risk of observability.

Figure D2: Distribution of true die roll outcomes and outcomes declared by players (Treatment 3)



Source: Authors' calculation.

Note: The red horizontal line indicates the theoretical distribution of a random draw. This graph shows the distribution of actual and reported roll-dice outcomes for treatment 3, where players face the risk of observability and financial penalty in case of cheating.

Table D1: Effect of the probability of losing on unconditioned cheating, using linear probability model (LPM)

	(1)	(2)
Round 2	0.113***	
P(loss)=2/6[a]	(3.65)	
Round 3	0.212***	
P(loss)=3/6[a]	(6.87)	
Round 4	0.327***	
P(loss)=4/6[a]	(10.60)	
Round 5	0.419***	
P(loss)=5/6[a]	(13.60)	
Theoretical losing probability[b]		0.632***
		(15.28)
N	2,265	2,265
R2	0.094	0.093

Source: Authors' calculation.

Note: Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

[a] : Reference category : Round 1, where P(loss)=1/6.

[b] : The theoretical losing probability is 0.167 for round 1, 0.333 for round 2, 0.500 for round 3, 0.667 for round 4, and 0.833 for round 5.

Figure D3: Relationship between cheating rate and gains obtained

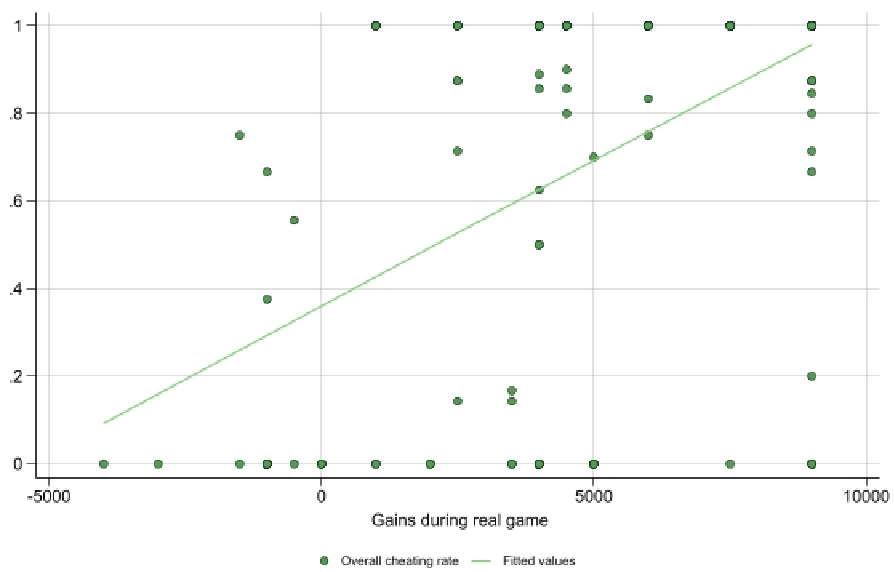


Table D2: Payoffs distribution according to players' cheating profile

Payoffs (FCFA)	Never cheaters (%)	Occasional cheaters (%)	Always cheaters (%)	Total (%)
-4,000	3.03	0.00	0.00	0.66
-3,000	3.03	0.00	0.00	0.66
-1,500	3.03	3.45	0.00	1.32
-1,000	18.18	6.90	0.00	5.30
-500	3.03	3.45	0.00	1.32
0	9.09	0.00	0.00	1.99
1,000	6.06	0.00	4.49	3.97
2,000	6.06	0.00	0.00	1.32
2,500	0.00	13.79	4.49	5.30
3,500	6.06	6.90	0.00	2.65
4,000	18.38	17.24	12.36	14.57
4,500	0.00	10.34	7.87	6.62
5,000	12.12	3.45	0.00	3.31
6,000	00.00	6.90	11.24	7.95
7,500	3.03	0.00	19.10	11.92
9,000	9.09	27.59	40.45	31.13
Total	100.00	100.00	100.00	100.00

Source: Authors' calculation.

Table D3: Differences in payment modes with farmers by players' cheating profiles

	(1) Never cheaters		(2) Sometimes cheaters		(3) Always cheaters		T-test P-value		
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)	(1)-(3)	(2)-(3)
Pay farmers through mobile money	30	0.233 (0.079)	27	0.111 (0.062)	84	0.083 (0.030)	0.233	0.032**	0.664
Prefer cash for confidentiality reasons	23	0.783 (0.088)	24	0.958 (0.042)	77	0.922 (0.031)	0.074*	0.062*	0.546

Note: The value displayed for t-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Appendix E : Robustness checks

Table E1: Linear Probability Model (LPM) on the subsample of occasional cheaters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment 2	-0.103*	-0.108*	-0.130*	-0.071	-0.089					
	(0.052)	(0.054)	(0.065)	(0.060)	(0.066)					
Treatment 3	-0.138**	-0.144**	-0.176*	-0.107	-0.122	-0.025	-0.015	0.054	0.126*	
	(0.052)	(0.054)	(0.077)	(0.059)	(0.069)	(0.052)	(0.062)	(0.056)	(0.063)	
Previous loss rate		0.068								
		(0.207)								
Loss in previous rounds			0.050							
			(0.076)							
Previous cheat rate				-0.083						
				(0.077)						
Cheated in previous rounds					-0.028					
					(0.081)					
Previous observation rate						-0.126				
						(0.124)				
Observed in previous rounds							-0.055			
							(0.097)			
Previous cheating observation rate								-0.433***		
								(0.124)		
Observed while cheating in previous rounds									-0.358***	
									(0.088)	
Number of times players get a sanction										-0.376***
										(0.111)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample selected on Treatments #	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	2, 3	2, 3	2, 3	2, 3	3
N	435	435	435	435	435	270	270	270	270	110
R2 (within)	0.129	0.129	0.130	0.131	0.129	0.095	0.092	0.133	0.147	0.192

Source: Authors' calculation.

Note: Controls include the selected lottery and round number. Standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table E2: Linear Probability Model (LPM) on overall sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment 2	-0.023	-0.012	0.005	-0.023	-0.023					
	(0.021)	(0.022)	(0.023)	(0.024)	(0.025)					
Treatment 3	-0.011	0.001	0.022	-0.011	-0.011	0.009	-0.002	0.005	0.039	
	(0.021)	(0.022)	(0.023)	(0.024)	(0.025)	(0.022)	(0.026)	(0.023)	(0.026)	
Previous loss rate		-0.152								
		(0.090)								
Loss in previous rounds			-0.117^{**}							
			(0.037)							
Previous cheat rate				0.000						
				(0.031)						
Cheated in previous rounds					0.002					
					(0.034)					
Previous observation rate						0.026				
						(0.051)				
Observed in previous rounds							0.038			
							(0.039)			
Previous cheating observation rate								0.034		
								(0.050)		
Observed while cheating in previous rounds									-0.063	
									(0.037)	
Number of times players get a sanction										-0.153^{***}
										(0.042)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample selected on Treatments #	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	1, 2, 3	2, 3	2, 3	2, 3	2, 3	3
N	2,265	2,265	2,265	2,265	2,265	1,510	1,510	1,510	1,510	755
R2 (within)	0.125	0.126	0.129	0.125	0.125	0.123	0.123	0.123	0.124	0.145

Source: Authors' calculation.

Note: Controls include the selected lottery and round number. Standard errors in parenthesis. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Disclosure statement

Declaration of competing interest

We, Delphine Boutin, Marine Jouvin and Louis Olié, the authors of the manuscript entitled "Information Asymmetries, Risk and Dishonesty: A behavioral experiment among Middlemen in Certified Cocoa Value Chain", gratefully acknowledge financial support from Bordeaux School of Economics (University of Bordeaux) and from Touton S.A.

We have no material or financial interests related to this investigation, and there are no other funding sources to disclose. During her PhD, Marine Jouvin served as a CSR Policy Impact Assessment Researcher at Touton S.A., a cacao exporting company that generously supplied us with a comprehensive list of cooperatives for our study. However, the findings expressed in this paper are those of the authors and do not necessarily represent the views of Touton or its partners.

Data availability

Anonymized data will be made available upon reasonable request.

Ethics

Our institutions do not have an Institutional Review Board (IRB) system. However, in adherence to ethical principles, all study participants were provided with detailed information about the study and voluntarily gave informed consent before participating. The consent process included explaining the nature of the research, potential risks and benefits, and the voluntary nature of participation. Participants were assured of the confidentiality and anonymity of their responses. This study strictly adhered to ethical guidelines, and all procedures were conducted in accordance with the principles outlined in the CITI-SBE trainings.