Four Essays in Behavioural Finance and Experimental Economics

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a. Acknowledgement

I would like to express my sincerest gratitude to my supervisors Professor Thorsten Chmura and Dr Hang Le for being very patient and for guiding me with comments, corrections, and encouragement. It would not be an overstatement that my supervisors have deepened my understating of academic research in the area of finance and experimental economics. A special thanks goes to Dr Kun Jiang and Professor Marcus Giamattei who have provided me with constructive comments and valuable feedbacks. I would also like to thank Dr Pia Weiss and Professor Roberto Hernan Gonzalez for providing me with valuable suggestions.

b. Introduction

The PhD thesis consists of 4 separate but related pieces of works. All of them are associated with various behavioural phenomena that can affect the way individuals or a group of individuals make economic decisions. The PhD thesis adopts two methods of research: empirical and experimental.

In Paper one, we examine the connection between shareholder activism and an important individual difference in the form of narcissism. We find that narcissistic CEOs are more likely to be targeted by shareholder activists. We also observe that a high level of narcissism in CEOs can be an obstacle to post-event performance improvement, and large shareholders are less likely to be associated with narcissistic CEOs. Despite the growing literature on how individual managers can affect different company-level variables, the evidence regarding how the CEO's personality influences shareholder activists' view of the firm remains very scarce. Our findings make an original contribution to shareholder activism and managerial personality traits literature. First, the paper contributes to research on the determinants of shareholders' activism. By documenting a positive association between CEO narcissism and shareholder activism, we are able to show a mechanism through which individual CEOs' personality characteristics can also significantly influence activist shareholders' decision to target a firm. Thus, we contribute to a growing stream of research exploring personal attributes and behavioural biases of executives that can help explain cross-sectional variation in corporate events. Moreover, the study examined a critical corporate issue based on a sample of firms mostly from the S&P 1500. Most prior studies in this area restrict their collection of data to S&P 500 firms or Fortune 500 (which only includes the largest firms in the U.S. capital market). By incorporating mid-sized and small companies to our analysis, we have shown the effects of CEO narcissism at non-large firms in the economy. The study is based on using a CEO data set for the period 2006-2015. An unobtrusive measure is used to incorporate CEO narcissism to the empirical analysis by combining information on CEO's picture prominence in the company annual report, relative cash pay of the CEO, and relative non-cash pay of the CEO (Chatterjee and Hambrick, 2007). The study also presents experimental evidence that is in line with the empirical findings that narcissistic CEOs are more likely to face intervention in the form of an activism event. Experimental data show that narcissistic leaders are more likely to be punished by the group members.

Designing and running experiments in the Centre for Research in the Behavioural Sciences (CRIBS) lab at the University of Nottingham Business School had been a significant part of my PhD. The controlled experiments shed light on whether the decisions participants make are associated with different behavioural biases. Economic incentives were provided to research participants who were mainly recruited from an existing database (ORSEE Jointly used by CeDEx and CRIBS) of students affiliated with The University of Nottingham who has agreed to receive information about prospects for participating in economic experiments.

In Paper 2, we study accountability in a principal-agent set-up where a leader makes risky decisions on behalf of a three-person group. This is an experimental study. Overall, 285 subjects were recruited at the CRIBS laboratory of the University of Nottingham, using ORSEE (Greiner 2015). The experiment was conducted on computers using zTree (Fischbacher, 2007). We investigate the influence of different accountability mechanisms on the behaviour of the leader. We also check how the recipients in a group use different accountability tools and the effectiveness of different accountability mechanisms in producing social benefits. In the literature of decision making for others, most attention is paid to the behaviour of the individuals making decisions. The behaviour of the parties affected by the decision has largely been ignored is this strand of literature. In this study, we pay attention to the actions and behaviour of the recipients who are affected by the leader's choices. We study six accountability treatments (in addition to two no-accountability treatments) where a member from each group (an active member) can punish or/and reward the leader. Punishment and rewards are costly. Leaders exhibit more risk-seeking behaviour when the principal is unable to punish. We observe that recipients find it most satisfactory when the leader chooses the option they (recipients) prefer. We also show that the punishment-only environment leads to the lowest amount of misalignment between the leader's choices and other recipients' preferences. Finally, we find that unlike reward decisions, principals' punishment decisions are in line with a hybrid accountability system.

Paper 3 is also an experimental study. The study aims to understand the impact of responsibility on asset market price stability. In our experiment, a trader trade on behalf of a passive subject, i.e., traders take the responsibility of buying and selling of assets (i.e., shares). We observe that responsibility reduces overpricing in markets where traders receive a fixed salary for performing the task. While there is some research on responsibility in economics in general, the issue is largely overlooked in the area of experimental asset markets. We, therefore, know little about the preparedness of a trader to take on risk when his trading choices affect other another individual. The study contributes to both the literature of decision making for others and the literature experimental asset pricing by showing a link between responsibility and asset price stability. Two hundred and sixteen subjects from a major university in the UK participated in the responsibility with variable salary treatment, and responsibility with fixed salary treatment. Experiments were conducted at the CRIBS lab of the University of Nottingham. Subjects were recruited using ORSEE (Greiner, 2015). The experiments were computerised using zTree (Fischbacher, 2007). We ran 12 sessions (markets) with 18 subjects each. The results are based on comparisons between twelve markets where traders are responsible and six baseline markets where traders are not responsible for someone's economic welfare.

Paper 4 consists of two experiments. First, we examine the influence of different types of intervention on lying behaviour. The study uses interventions in the form of written messages and video clips to observe whether those have any impact on the propensity of truth-telling. We conducted sessions in Nottingham. Overall, 318 subjects participated in the experiment. Participants report privately observed

outcomes associated with a task anonymously. They report a payoff once before the intervention and then one more time after the intervention. We selected the die-rolling experiment following Fischbacher and Föllmi-Heusi (2013). We observe that a negative message about dishonest actions encourages some subjects to cheat less. Our study also finds that the emotional state believed to be contentment (elicited by a short video clip) can encourage truth-telling. We reveal a strong and positive association between the subjects' level of narcissism and propensity of lying, i.e., narcissistic individuals are more likely to report a higher payoff. Moreover, we show that there is a clear link between individuals' cognitive abilities as measured by the CRT and the extent to which an intervention between two reporting stages influences subjects. Our results suggest that high CRT individuals are less likely to be manipulated by an intervention. In experiment two, we observed that truth-telling was less evident in the presence of added responsibility. We find that both men and women tell a white lie (i.e. a lie that helps another individual) with or without an economic incentive, but men are more likely to tell a white lie in the presence of an economic incentive.

One might argue whether an economist should pay attention to personality dimensions such as narcissism and other individual differences. The conventional economics and finance theories, which are primarily focused on the outcomes, suggest that behavioural factors and personality dimensions are unimportant and in aggregate their influence is averaged out. The alternative school of thoughts, however, has been forcefully arguing that those do matter, and an economist can create valuable knowledge by studying them (Barberies and Thaler, 2003). We showed that narcissism, cognitive ability, and individual dimensions such as gender could explain decisions in different contexts. The findings in Paper 1 are consistent with the findings in Paper 4. In Paper 4, we showed that narcissistic individuals are more likely to cheat, which supports the findings (in Paper 1) that high level of narcissism is can attract interests from activist shareholders. The findings with regards to cognitive ability in different works are also in line with each. Both in Paper 3 and Paper 4, we find that individuals with higher cognitive ability can extract more economic benefits, perhaps because they are less susceptible to behavioural biases. We also show that individuals are often motivated by social goals and not always driven by economic objectives. We observed that the feeling of being responsible for someone else's economic wellbeing could have a significant impact on how a decision-maker makes choices, even in the absence of economic incentives.

Experiments conducted in a laboratory set-up have been useful to address a myriad of issues in economics. Laboratory experiments can be used to verify the consistency of the predicted and actual behaviour of economic agents and to investigate how various mechanisms and institutions influence the way economic agents act. This investigation methodology has been gaining popularity among researchers and is employed to explore an array of public policy matters. Findings from laboratory experiments are usually taken as having a high level of internal validity since the causal relation among factors can be accurately established. Nevertheless, as far as external validity goes, experimental studies are usually considered inferior to studies based on market data and field data. The situation is

reversed when our attention is shifted to internal validity. Although our experimental results are in line with some important works in the respective literature, we remain cautious in making comments regarding the quantitative external validity of our findings. Levitt and List (2007, 2009) argued whether insights gained by analysing data generated inside laboratories are useful to understand the realm beyond. Studies in this stream primarily argue that experimental results might fail to generalise to the real-world marketplaces. Camerer (2012), on the other hand, forcefully argued that overemphasising external validity is distracting for a large group of laboratory experiments. He suggested that external validity matters more for laboratory experiments aiming to whisper into the ears of policymakers. A significant number of experimental studies still pursue different objectives, and hence ensuring external validity is less of a concern. According to Camerer (2012), experimental studies aiming to understand general principles do not necessarily have an obligation to ensure a high degree of generalizability. The quantitative external validity of experimental results can only be verified by comparing the laboratory findings with felid data. As per Kessler and Vesterlund (2015), the relevant question regarding experimental results should be whether the qualitative experimental results have external validity. Researchers should be concerned more about the sign of the effect in experimental results rather than the precise magnitude of the effect. Although experimental works in laboratories may not promise quantitative external validity, they do promise qualitative external validity.

Paper 1

CEO Narcissism and Shareholder Activism

Abstract

The study finds that CEOs with a higher level of narcissism are significantly more likely to be targeted by shareholder activists. The study is based on a sample of 778 CEOs associated with a sample of firms from the Execucomp for whom we could calculate narcissism scores. We use proxies for CEOs' level of narcissism which is based on CEO picture prominence in the annual report, relative cash pay, and relative non-cash pay. We test the hypothesis that narcissistic CEOs are more likely to face activism events with a signature-based measure of CEO narcissism and find similar results. Moreover, there is some evidence that institutional blockholders, in general, are less likely to invest in firms with narcissistic CEOs. It appears that there is a negative association between a high level of narcissism in CEOs' and long-term value creation following activism news announcement. The results imply that CEO narcissism can have a significant influence on the investor-management relationship. The study also presents experimental evidence that is in line with the empirical findings that narcissistic CEOs are more likely to face intervention in the form of an activism event. Experimental data show that narcissistic leaders are more likely to be punished by the group members.

Key words: CEO narcissism, Shareholder Activists

1. Introduction

A strand of behavioural finance literature proposes that personality characteristics of top executives can have a substantial influence on their decisions (Baker et al., 2007). This, in turn, can affect corporate policies and the financial outlook of a company. The study adds to the shareholder activism literature by investigating whether a specific CEO personality trait is associated with the likelihood that shareholder activists will target a firm. While previous research on shareholder activism focuses on different firm, environmental, and governance variables that trigger or impede activism events, this research sheds lights on CEO's level of narcissism in order to understand the motivations behind shareholder activism (Brav et al., 2008; Ertimur et al., 2011).

Studies describe narcissism as a stable, multidimensional personality trait which is related to a sense of superiority and a tendency to involve in questionable behaviour (Blickle et al., 2006; Judge et al., 2006). Highly narcissistic individuals can lack moral sensibility, exploit others, aggressively pursue what they believe is theirs, and think that they are above the law (Twenge and Campbell, 2003; Exline et al., 2004; Campbell et al., 2011). The objective of the study is to understand whether, narcissism, a key CEO personality characteristic (Chatterjee and Hambrick, 2007; 2011), has an impact on the investor-manager relationship. The research studies CEO narcissism and arguably the most influential investor group (Renneboog and Szilagyi, 2011) in the form of shareholder activists. It is expected that activist shareholders are more likely to target firms that are managed by CEOs with a high level of narcissism.

"Shareholder activism" is defined as actions taken by shareholders with the explicit aim of affecting firms' policies and activities, instead of as covert intentions embedded in ownership stakes or trading behaviour. Monks and Minow (1991) noticed that the risk of poor diversification forbids many fund operators from selling shares in underperforming firms. An increase in shareholder activism was observed in the middle of the 80s, mainly fuelled by institutional shareholders' inability to sell shares in underperforming firms because of diversification concerns. The start of investor capitalism (Useem, 1996) has been marked by increased shareholder activism (Greenwood and Schor, 2009; Kahan and Rock, 2010; Renneboog and Szilagyi, 2011), ranging from investor confrontations with managers to express their dissatisfaction (David et al., 2007; David et al., 2001) to formal interferences to modify corporate strategy and performance (Song and Szewczyk, 2003; Westphal and Bednar, 2008).

The richness of issues in the literature of activism, for instance, the firm, activist, and environmental features that encourage or inhibit activism, in consort with the extensiveness of activism's issues, means, and processes, provide an abundance of opportunities and challenges (both theoretical and empirical) for researchers in this literature. Studies identify many factors that can motivate activist shareholders to engage in activism. Most of the works in this literature

focus on different firm-level traits that pave the way for activism events. The list of variables that have attracted attention from researchers includes firm size (Cai and Walkling, 2011; Ertimur et al., 2011), firm performance (Bradley et al., 2010 and Brav et al., 2008), level of cash holding (Faleye, 2004; Klein and Zur, 2009), leverage, degree of executive ownership (Bizjak and Marquette, 1998; Faleye, 2004), executive compensation, board structure, and institutional ownership (Greenwood and Schor, 2009; Kahan and Rock, 2010).

It is predicted that the actions of highly narcissistic CEOs, compared to less narcissistic ones, are more likely to be labelled as self-serving by the outside entities (e.g. potential shareholder activists), which in turn paves the way for activism events. The CEO's quest of narcissistic supply can result in a stronger chance of a CEO to take steps that are potentially detrimental to the organization. According to the works of Chatterjee and Hambrick (2007 and 2011), narcissistic CEOs are more likely to be associated with bold and headline grave actions. Firms managed by narcissistic CEOs, therefore, are likely to be more attractive to activist groups owing to a greater level of visibility. We conjecture that there is a positive association between CEO narcissism and the likelihood of facing activism events by the firm. In addition, the study investigates whether there is a link between CEO narcissism and the level of institutional blockholding. Finally, we shed lights on the long-term-value creation resulted from activists' intervention concerning CEO narcissism and presents experimental evidence that supports the empirical findings.

We test the prediction by using a data set for the period 2006-2015 containing mostly S&P1500 firm-year observations (95 per cent of the firms in our sample are from S&P1500). An unobtrusive measure is used to incorporate CEO narcissism to the empirical analysis by combining information on *(i)* CEO's picture prominence in the company annual report, *(ii)* relative cash pay of the CEO, and *(iii)* relative noncash pay of the CEO (Olsen et al., 2013; Chatterjee and Hambrick, 2007; 2011). We used experimental data to validate the link between picture prominence preference and narcissism. The data is based on a large sample of university students. The study employs conventional psychometric scales of narcissism to demonstrate that there is a positive correlation between picture

prominence preference and narcissism. We also show that picture prominence information and traditional measures of overconfidence, both empirical and experimental, are not significantly correlated.[1]

The study finds that firms associated with highly narcissistic CEOs are more likely to be approached by shareholder activists compared to firms with less narcissistic CEOs at the helm. The results imply that a high level of narcissism among CEOs can prompt a greater level of monitoring. This result is documented after controlling for different firm and CEO level determinants of activism events. The study also checks whether there is a link between CEO narcissism and institutional blockholding level. An analysis of institutional shareholding by large shareholders (we look at shareholders with more than 5 per cent of shares of a firm and top 5 shareholders) reveals a significant and negative association between CEO narcissism and aggregate institutional blockholding level in a firm. The result suggests that institutional blockholders are less likely to invest in companies that are run by CEOs high in narcissism. The result does not contradict our primary finding, i.e. activist shareholders are more likely to target firms with narcissistic CEOs at the helm. We argue that passive shareholders with large blocks of shares are less likely to choose a firm with a narcissistic CEO as their objectives are very different and they are less inclined to incur monitoring cost. Post-activism performance analysis shows that long-term value creation following the initiation of activism campaigns is negatively (with marginal significance) associated with CEO narcissism. The study also provides experimental evidence which is in line with the main finding. Data collected from experimental sessions conducted in Nottingham shows that narcissistic group leaders are more likely to be punished by group members.

Despite the growing literature on how individual managers can affect different company-level variables, the evidence regarding how the CEO's personality influences shareholder activists' view of the firm remains non-existent. Our findings make an original contribution to shareholder activism and managerial

[1] Although closely related, the concept of narcissism does not carry the same meaning as overconfidence (Aktas et al., 2016).

personality traits works of literature. First, the paper contributes to research on the determinants of shareholders' activism. As noted earlier, the majority of the research in this area examines the effect of firm-level characteristics and governance factors on activism. By documenting a positive association between CEO narcissism and shareholder activism, we are able to show a mechanism through which individual CEOs' personality characteristics can also significantly influence activist shareholders' decision to target a firm. Thus, we contribute to a growing stream of research exploring personal attributes and behavioural biases of executives that can help explain cross-sectional variation in corporate events. Moreover, the study examined a critical organizational issue based on a sample of firms listed on the S&P 1500. Most prior studies in this area restrict their collection of data to S&P 500 firms or Fortune 500 (which only includes the largest firms in the U.S. capital market). By incorporating mid-sized and small companies to our analysis, we have shown the effects of CEO narcissism at non-large firms in the economy. Our work complements the literature on executive psychology in behavioural finance that is associated with the hubris hypothesis (Roll, 1986). The study is closely linked to Chatterjee and Hambrick (2007 and 2011) and Aktas et al. (2016). The empirical work is also linked to works on CEO overconfidence (e.g., Malmendier and Tate, 2008; Hirshleifer et al., 2012), however, studies a different psychological concept (i.e. narcissism), highlights how investors view a very important personality trait. The study also presents a real-life setting that is linked to laboratory-based works in the psychology literature focussing on narcissism (Campbell et al., 2004a). The study complements notable works in the literature of shareholder activism works (such as Brav et al., 2008; Greenwood and Schor, 2009) by documenting the impact of CEO narcissism on activists' decision to target a firm.

The next section contains a review of related literature and Part 3 develops the hypotheses. Part 4 presents the discussions on methodology and describes the data collection process; Part 5 presents descriptive statistics; Part 6 presents the main results; Part 7 presents robustness checks and Part 8 discusses experimental results. Finally, Part 9 concludes.

2. Literature Review

2.1 Shareholder activism

The shirking problem or more formally known as the agency problem in organizations involving joint team production, has been the focus of many academic works in the field of economics and finance. According to Alchian and Demsetz (1972), although there are many benefits of team production, it comes at a cost as it is hard to match compensations to each person's contribution. The problem of shirking is especially severe in corporations. The seminal work of Berle and Means (1932) focuses on the agency problem. Their work suggests that agency problem arises due to the fact that management of a corporation does not own 100 per cent of the firm, i.e. ownership is separated from control. Studies show that large shareholders can discipline management in two ways: "exit" and "voice" (see Cronqvist and Fahlenbrach, 2008; Edmans, 2014; Holderness, 2007). The notion of an exit mechanism is simple; large shareholders sell their shares in the firm if they are not content with the management's performance. The other mechanism, voice, involves direct intervention in the firm (see Grossman and Hart, 1980; Shleifer and Vishny, 1986). Voice theories suggest that shareholders can influence firm value by communicating directly with management. This mechanism is popularly known as shareholder activism.

The work of Alchian and Demsetz (1972) suggests "modifications" are necessary to deal with the shirking problem that is inherent in modern corporations. Shareholder activism can be described as one of those "modifications". A direct intervention can be hostile or non-hostile and can take many different forms: proposing a strategic change via a public shareholder proposal or a private letter to management or voting against directors at an annual meeting (Cronqvist and Fahlenbrach, 2008). In early days, activism was viewed merely as an administration of the yearly stockholders' meeting (Vogel, 1983). Some went as far as labelling activism as "a waste of top management's time and firm's resources" (Cane, 1985: 70). The current views, nevertheless, claim that activist shareholders are leading the main event and have seized the centre ring (Duhigg, 2007). Stockholder activism has targeted corporate governance and performance

(Dimitrov and Jain, 2011), along with public, political, and conservational issues (Clark and Crawford, 2012; Reid and Toffel, 2009), initiating a movement away to a political from a trading-based model of corporate governance (Karpoff et al., 1996; Wahal, 1996).

An upsurge in shareholder activism in the middle of 80s overlapped with the rise of institutional shareholdings, mainly among funds looking to replicate stock index earnings. Often it is inconvenient for fund managers to sell stocks (i.e. using the exit mechanism) in underachieving firms' due to diversification concerns (Monks and Minow, 1991). Therefore, many entities involved in activism to nudge companies into better directions and improved performance. As per Pound (1992), a restrictive outside market for corporate control is the reason behind increased level of shareholder activism (also see Black, 1992). Shareholder activism is now a pervasive phenomenon. The work by Renneboog and Szilagyi (2011) reports 2,436 shareholder proposals associated with 548 firms for the sample period 1996-2005. Thurm and Benoit (2012) document that almost a quarter of the target companies that faced activism in the year 2012 (24 per cent to be precise) had market capitalizations that goes beyond $1 billion. The corresponding figure was 8 per cent and 11 per cent in 2009 and 2011 respectively.

2.2 Narcissism, overconfidence, and other relevant concepts

Keynes stated: “even apart from the instability due to speculation, there is the instability due to the characteristics of human nature that a large proportion of our positive activities depend on spontaneous optimism rather than mathematical expectation, whether moral or hedonistic or economic” (Keynes, 1936, page 144). Aspects such as confidence, fairness, corruption and anti-social behaviour, money illusion can be associated with animal spirit and can enlighten our understanding of economic decision-making behaviour (Akerlof and Shiller, 2009). Prospect theory (Kahneman and Tversky, 1979) proposes that individuals are not entirely rational and for that reason often end up making less-than-optimal decisions. Studies in the managerial bias literature embrace the view of Kahneman and

Tversky (1979). These studies are based on the assumption that managers sometimes gravitate towards sub-optimal decisions, mainly due to the influence of behavioural biases.

Narcissism is a well-defined concept with a strong theoretical background, and there have been impressive developments in the psychology literature concerning measures of narcissism. Although this psychological phenomenon is a crucial concept that shapes the behaviour of individuals, it has attracted scant attention in the literature of corporate finance (see Ham et al., 2018). The rich literature of the narcissism concept has led to the construction of theoretically-sound indirect measures that can be useful for a large sample (Raskin and Shaw, 1988).[2] Narcissism, surprisingly, has attracted limited attention in the behavioural corporate finance literature despite the fact that it is a vivid feature associated with many CEOs.[3] The reason is perhaps that narcissism is often treated as a fanciful concept by the researchers in the finance literature. The lack of attention, nevertheless, presents an opportunity to explore the effect of this very important personality characteristic on various corporate decision-making and financial outlook of the company. A handful of empirical studies in the finance literature that studied CEOs' narcissistic tendencies concluded that narcissism is not a good sign.

The popularity of narcissism, as a psychological concept, is mainly due to the works of Sigmund Freud (Freud 1914/1957). Freud's works treat narcissism as a clinical concept, but narcissism is also considered as a subclinical concept by many other studies. In order to reconceptualize narcissism as a personality dimension, rather than just as a clinical syndrome, psychologists have developed psychometric scales to measure narcissism. The Narcissistic Personality Inventory (NPI) developed by Raskin and Hall (1979) has been trendy among researchers,

[2] According to Campbell et al. (2004), narcissism can influence an individual's cooperation, trust, risk-taking, motivations, decision-making, and long-term performance.

[3] The scale used to measure normal narcissism (the narcissistic personality inventory, or NPI, Raskin and Terry, 1988) was adopted from the Diagnostic and Statistical Manual of Mental Disorders (DSM-III) criteria for NPD (American Psychiatric Association, 1980), to explore potential effects and correlates in nonclinical populations of a trait that, in extreme forms, constitutes a distinct mental disorder.

especially in the field of psychology. It was originally developed to measure narcissism by using the DSM-II (Diagnostic and Statistical Manual of Mental Disorders; American Psychiatric Association, 1968) behavioural criteria as a template. In order to make it more accommodating, the instrument was condensed to fewer items (revised in Raskin and Terry, 1988; Campbell et al., 2004), which have been extensively used by researchers. The proposed study deals with the personality variable of narcissism (also known as normal narcissism) rather than a clinical disorder of narcissistic personality disorder (NPD).[4] The trait narcissism is based on the clinical criteria for NPD but relates to the scale of normal functioning. The proposed study will use the term "narcissists" to label those scoring high on this continuum. The study mainly focuses on the personality trait of narcissism and does not deal with NPD.

In the literature of economics and finance, a number of effects are condensed under the notion of overconfidence. The narcissism phenomenon is closely related to hubris and overconfidence, but it has unique features. Overconfidence is often understood as a systematic overestimation of the accuracy of someone's knowledge and involves an underestimation of the variance of the variable under consideration. This form of overconfidence, according to Lichtenstein et al. (1982), is also known as miscalibration (also see Biais et al., 2005). Greenwald (1980) suggests another variation of overconfidence which is given by improbable self-judgements. A simple example is presented by Svenson (1981), the study reveals overconfidence in students where subjects were asked to rank themselves. Surprisingly, 82 per cent of the student subjects thought they were in the top 30 per cent of the sample. Most experiments trying to understand this psychological phenomenon are based on responses to questions that ask the respondents to come up with some point estimates and then the confidence interval for the value of a random variable (Menkhoff et al., 2013). Burks et al. (2013) propose three theories of relative overconfidence. First, overconfidence can be caused by Bayesian updating from a common past and true statements if people are not sure about their

[4] NPD is a rare character disorder, and according to APA (American Psychiatric Association, 1994), only 1 per cent of the general population suffer from this.

type. Secondly, overconfidence can result from self-image concerns since this can disproportionately affect how someone chooses to receive novel information about his abilities. Finally, overconfidence can be stimulated by an individual's need to convey positive signals about his abilities.

The most-researched managerial personality characteristic is overconfidence. Studies revealed strong associations among managerial overconfidence and a myriad of corporate finance issues. For example, Malmendier and Tate (2005) and Hirshleifer et al. (2012) showed how CEO overconfidence affects firms' investment; Hribar and Yang (2016) linked executive overconfidence to management earnings guidance; Malmendier and Tate (2008) found mergers and acquisitions activities to associated with CEO overconfidence; Schrand and Zechman (2012) showed a link between overconfidence and financial irregularities and fraudulent activities. As mentioned in the previous sections, many studies, theoretical as well as empirical, also attempt to examine the influence of overconfidence on decisions related to different asset market outcomes (Lichtenstein and Fischhoff, 1977; Yates et al., 1997). As per Odean (1998), overconfident investors are highly likely to overestimate their abilities and for that reason hold riskier portfolios, i.e. portfolios that are suboptimal considering the risk-return trade-off. Overconfident investors are also more likely to destroy utility by trading more. This tendency is again connected to the fact that they overestimate the accuracy of their knowledge.

Judge et al. (2006) suggest that narcissistic individuals rate themselves unreasonably highly on many dimensions such as intellectual ability, inventiveness, competence, and leadership abilities (also see Farwell and Wohlwend-Lloyd, 1998). So, it cannot be denied that highly narcissistic individuals will also exhibit a high level of overconfidence. An experiment conducted by Campbell et al. (2004) demonstrates that narcissism is a significant predictor of overconfidence. Nevertheless, narcissism is a multidimensional personality trait and goes beyond overconfidence. It is essential to understand that overconfidence is just one of the dimensions of a narcissistic individual. This personality characteristic (narcissism) is associated with both genetic factors and

family members and is characterised by numerous critical dispositional tendencies (see Emmons, 1984 and Bogart et al., 2004). The same cannot be concluded about overconfidence. From a cognitive viewpoint, narcissists are highly confident about their ability; what is more, they also continuously desire to have their superiority to be confirmed by others.

Post (1993) suggests that narcissism can be classified as an extreme form of overconfidence. Nevertheless, a unique feature of a narcissist is the combination of self-love the craving for the spotlight. Overconfidence may hinge upon many individual characteristics. For instance, Vissing-Jorgensen (2004) shows that more sophisticated investors are unlikely to suffer from this cognitive bias. Studies show that overconfidence can be stemmed from the type of culture in which the agent is operating. For example, Statman (2010) implies that individuals in societies where collectivism is the norm are more likely to exhibit heightened overconfidence compared to their counterparts located at the other end of the continuum, i.e. individuals in more individualistic societies (also see Hofstede, 2001).

Narcissists are individuals for whom achieving status and esteem is very important. It is so important that much of their inner and social lives is focused on this objective (Campbell et al., 2004). They constantly look for opportunities to display their abilities and establish their superiority (Morf and Rhodewalt, 2001). Maintaining their sense of worth is vital for them; highly narcissistic individuals actively pursue self-enhancing situations and expect to excel in tasks with the potential for self-glory (Farwell and Wohlwend-Lloyd, 1998). Overconfidence, however, makes individuals put more weight on the occurrence of a particular outcome, i.e. when a person places an inflated subjective possibility on the occurrence of a specific result. Thus, it can be stated that many non-narcissistic individuals will exhibit heightened overconfidence. The work of Anderson and Tirrell (2004) implies that narcissistic individuals are likely to make financial decisions that improve their social status. Their study shows that narcissistic managers are more likely to make decisions that have positive impacts on the financial appearance of their firms and by extension the appearance of the managers themselves, the purpose being to support their ego and self- esteem.

Similarly, Hales et al. (2012) suggest that individuals high in narcissism inflate their reported performance when it is related to social status.

Narcissism is an intricate, multidimensional pattern (unlike overconfidence) and is not a single cognitive characteristic (Bollaert and Petit, 2010). As per the study of Foster and Brennan (2011), highly narcissistic individuals are inclined to invest in risky projects. These studies suggest that this behaviour is not because narcissists are irrational; they behave in this way because they relish the notion of a big payoff, not because they are unable to measure the level of risk associated with the investment. So, it boils down to the fact that narcissism is not a cognitive bias. Overconfidence, on the other hand, is a cognitive bias. Narcissists are conscious of what they are doing and do it for motivational reasons (De Bodt et al., 2015). Narcissists see life as a stage for accomplishing status, success, and esteem, each of which leads to a positive self-concept.

3 Hypotheses

3.1 Investor activism and CEO narcissism: According to Campbell et al. (2004), narcissism can influence an individual's cooperation, trust, risk-taking, motivations, decision-making, and long-term performance. Though narcissism is an important characteristic in leaders (Bergman et al., 2010), little attention has been paid to investigate this phenomenon within a large sample of CEOs empirically. The rich shareholders' activism literature has not paid attention to top managers' personality traits, and therefore research on this topic is warranted. Most of the empirical study in this literature has focused on firm-level variables, typically studying companies based on an agency theory framework (Goranova and Ryan, 2014). The focus of the study is the CEO because they are the most influential and visible figure within the firm and have the highest level of influence on the ethical environment of the organisation (Rijsenbilt and Commandeur, 2013). CEOs with a high level of narcissism make decisions with the intention of ''maintaining a positive sense of self, engaging in ego-defensive behaviour and

preserve their self-esteem'' (Brown, 1997). The study aims to see how these tendencies affect the way outside investors view the firm.

Some studies describe narcissism as an extreme form of overconfidence (see Post, 1993; Schrand and Zechman, 2012). A unique feature of an individual suffering from a high level of narcissism is ''self-love'' and the sheer desire to be always under the spotlight. The self-love aspect of narcissism often drives the narcissistic individual to behave in a way that is not beneficial for others around them. Narcissistic CEOs were found to be associated with many big scandals in the past. Cases such as those at Enron, Tyco and Ahold are indicative of the damaging consequences of CEO narcissism (Rijsenbilt and Commandeur, 2013). We predict that a narcissistic CEO will find the presence of an activist less than ideal as it will restrict his actions, especially, actions that capture attention but not necessarily improve firm value (Helwege et al., 2012). But CEOs of publicly held companies are unable to stop an activist from entering the picture. The set-up is such that companies do not choose their shareholders; rather, they are chosen by the shareholders. The critical question that the study investigates is whether activist shareholders are more likely to target a firm that is being run by a narcissistic CEO.

Various works revealed that activist bodies typically target companies that are underperforming compared to their peer firms (e.g. Bethel et al., 1998). Consistent with agency theory, companies generating poor operating performance are more likely to be targeted by shareholder activists (Ertimur et al., 2011; Renneboog and Szilagyi, 2011). Studies suggest that activists create value by intervening. Research works, such as Chatterjee and Hambrick (2007) show that CEOs high in narcissism are more associated with bold and risky decisions that grab headlines. These risky actions destabilise their firms' position (also see Chatterjee and Hambrick, 2011). They observed that CEOs high in narcissism have the tendency to deliver extreme performance, i.e. big profits or big losses, and fluctuating performance for their organisations. Petrenko et al. (2016) showed that narcissistic CEOs tend to misuse firm resources to engage in CSR activities to reinforce their image and fulfil their private need for attention. They also document the fact that narcissistic CEOs reduce the effectiveness of CSR activities by using them non-strategically.

There are quite a few reasons why we believe that narcissistic CEOs are attractive targets for shareholder activists. Various studies claim that shareholder activism is more likely to be associated with firms prone to agency problem (Ertimur et al., 2011; Renneboog and Szilagyi, 2011). High level of narcissism in CEOs is expected to be associated with a greater magnitude of agency problem in the firm as the CEO is more likely to engage in self-serving tactics. Narcissistic CEOs with their bold and headline-grabbing actions and self-centred behaviour are more likely to appear on the radar of activist groups. Furthermore, it is possible that due to activities driven by narcissistic tendencies, a narcissistic CEO is already unpopular among the existing shareholders of the company. A shareholder activist would see that as an opportunity and might want to capitalise on that. Going against a narcissistic CEO could be more effective in getting support from the public, thus solidifying the identity and position of the activist shareholder(s). The activist shareholder might think that that it would be easier for them to get the supports from those shareholders who for some reasons choose not to pursue a costly active strategy to force the CEO to make financial decisions that are more in line with their choices or unable to sell their share due to risk associated with diversification (Monks and Minow, 1991). The following hypothesis is the focus of this study:

H1: Activist investors are more likely to target firms that are managed by narcissistic CEOs.

3.2 CEO narcissism and in the level of institutional block ownership. The ownership structure in the US capital market provides individual shareholders with opportunities to diversify their portfolios. The opportunity, however, results in free-rider problems, since no small shareholder assumes the monitoring role. In the work of Grossman and Hart (1980), monitoring is understood as a public good, and the free-rider problem arises because it is costly for shareholders of a diffusely owned firm to provide this public good, i.e. it is not cost-effective for a small shareholder to intervene directly in management. So the result is the free-rider problem that Grossman and Hart (1980) persuasively defined. A consequence of the absence of a solution to this problem can be a high level of agency problem which, according to corporate finance theories, should have a negative impact on

firm-value (Grossman and Hart, 1980). A growing stream of research in the areas of finance and accounting has started to investigate the connection between the personal characteristics of highest ranked managers and company policies and corporate outcomes (e.g., Graham et al., 2013; Bertrand and Schoar, 2003). The empirical study explores the possibility that an institutional blockholder might look at the CEO personality traits of the firm under consideration while making a block investment decision.

A considerable body of research has focused on the role of institutional blockholders as corporate monitors (Edmans, 2014). Our study tries to understand whether institutional blockholders are more (less) likely to be associated with narcissistic CEOs. Cronqvist and Fahlenbrach (2008) suggest that blockholders are different in terms of their beliefs, expertise or preferences, e.g. they can hold different views regarding how to influence or choose companies. In addition, they can differ in terms of their preferences regarding policies, i.e. what mixture of company policies will maximize shareholders' wealth/ value. The results associated with prior empirical studies in relation to blockholding (e.g. Holderness and Sheehan, 1988), where no significant blockholding influence was found, are evidence that supports the views of Edmans (2014) and Cronqvist and Fahlenbrach (2008). It is essential to note that not every blockholder wants to intervene in the management of the firm. Cronqvist and Fahlenbrach (2008) find a blockholder fixed effect regarding corporate policies for some groups of blockholders – e.g. activists, pension funds, corporations, individuals, private-equity firms and mutual funds – and no effect for some other groups –insurance companies, banks or money managers. CEOs' personality traits could be a crucial factor for those blockholders who intend to force some changes to the corporation. These shareholders are the ones at the "most active" end of the blockholder-activism continuum.

The study conjectures that institutional blockholding is negatively associated with CEO narcissism. We examine the conjecture by looking at the level of institutional blockholding for companies with narcissistic CEOs at the helm. We suspect that institutional blockholders will be less inclined to invest in companies with CEOs with a high level of narcissism. Narcissism has frequently been

considered as a dark side of the personality, and indeed forms a portion of the dark triad of personality traits, since a narcissistic individual is often associated with arrogance and immodesty (Resick et al., 2009; Paulhus and Williams, 2002). Campbell et al. (2004) suggest that narcissism can influence an individual's cooperation, trust, risk-taking, motivations, decision-making, and long-term performance. Highly narcissistic CEOs are expected to value themselves highly and to be less attentive to the needs of others. Moreover, due to an ever-present concern about themselves, narcissistic individuals often lack moral sensibility (Campbell et al., 2011). The study anticipates that institutional blockholders will invest less in a firm with narcissistic CEOs.

Various studies documented the fact that different classes of blockholders pursue different types of goals. As per Edmans et al. (2013), some groups of these blockholders are not associated with activism, i.e. they pursue a passive investment strategy. It is, for that reason, possible that many of these blockholders would not want to be associated with a narcissistic CEO. These blockholders hold large stakes purely for diversification purposes and hence do not intervene. Since it is often costly to sell large stakes, unless the large shareholder is an activist, it is unlikely that it would be interested in investing in a firm whose CEO is a narcissist. Following previous studies in the literature of psychology, we assume that CEOs with a higher level of narcissism are more likely to dictate firm decisions (Nevicka et al., 2011), and will be continually looking to overshadow the presence of key people within the organization (Raskin et al., 1988). We also assume that a narcissistic CEO would ignore opinions from others as well as the outcome associated with their own decisions in the past (John and Robins, 1994), making their firm an unattractive choice for passive blockholders.

The presence of a highly narcissistic CEO is not an ideal scenario for the blockholder who is unwilling to incur a high monitoring cost. It is therefore likely that a blockholder will be less inclined to invest in a firm that is managed by a highly narcissistic CEO due to a potentially high level of monitoring costs. We argue that the assumption is not contradicting our first hypothesis, which is activist shareholders are more likely to target a narcissistic CEO, as the objective of a

passive blockholder is highly likely to be very different from an activist shareholder. Almazan et al. (2005) suggest that institutional investors have a varied level of willingness and ability to monitor managers of the investee firm. The study implies that one key determinant of the approach investors have toward monitoring the management is the cost associated with it, especially firm-specific cost of monitoring. Even though it is more cost-effective for a large shareholder to monitor the management, the cost might be too high if they have to deal with a CEO with narcissistic tendencies. An overwhelming number of large shareholders falls under the category of passive investors, as suggested by some prior studies. For example, Greenwood and Schor (2009) studied 990 activism events, but at the beginning, they started with more than 173,000 13D failings.[5] This highly deficient proportion is an indication that most of the large shareholders are following a passive investment strategy. We suggest that large shareholders, in general, are less likely to be involved with a company that is run by a narcissistic CEO. We propose the following hypothesis based on the previous discussion.

H2: Passive institutional blockholders are less likely to be associated with firms managed by narcissistic CEOs.

3.3 Activism, long-term value creation, and CEO narcissism: The study also focuses on the long-term value creation resulted from activist's intervention. In an imperfect market short-term market reaction around activism, announcement dates do not capture all the value resulting from the intervention (Ikenberry et al., 1995). So, it is necessary to study long term value creation to understand the impact of activism events on firm value fully. Several studies documented long term value creation as captured by earning variables such as market return from investment in firm share and return on firm asset. Earlier studies on non-hedge fund activism did not show any effect of non-hedge fund activism on earrings variables (e.g. Carleton et al., 1998; Karpoff et al., 1996; Smith, 1996). Recent studies, such as Brav et al. (2009) and Boyson and Mooradian (2011), documented post-activism performance

[5] Investors are required to file a 13D filing with the Security Exchange Commission inside ten days of attainment of 5 per cent or more shares of any classification of a firm.

improvement associated with firms targeted by hedge funds. They documented performance for target firms in the year following activism. Our study chooses two prominent measures of performance in the forms of share return (the variable is named as RET in the analysis) and returns on asset (the variable is tagged as ROA in the analysis).

The narcissistic personality is full of inconsistencies. Although Self-love is a prominent feature of narcissistic individuals, they have a continuing need for their self-concept to be protected; therefore, they have a high, but extremely flimsy, sense of self-image (Kernis, 2005). Narcissistic individuals desire praise and endorsement, then again, they tend to operate in ways that result in disdain, and that put off others (Bradlee and Emmons, 1992; Farwell and Wohlwend-Lloyd, 1998). Studies have documented that narcissists do not pay attention to objective indicators concerning their performance and show hypersensitivity to social praises. According to Raskin et al. (1991), narcissists rate themselves very highly and are unwilling to accept the fact that there are rooms for improvement. One possibility is that the long-term value creation is higher when the CEO of the target is more narcissistic simply because there are more rooms for improvements (due to serious agency problem within the company resulting from CEO's pursuit of self-glory). As per Campbell et al. (2011), the construct narcissism is characterized by grandiosity, self-love and overestimated self-views. So we suspect that it would be more difficult for shareholder activists to create value in the presence of a narcissistic CEO as the CEO is less likely to be accommodating and would be unlikely to accept changes. The study considers the following hypotheses:

H3: The long-term value creation resulting from intervention is lower for targets with high narcissism CEOs.

4. Methodology and Data

4.1 How did we arrive at the final data set?

In the first quarter of 2016, we accessed the Execucomp of the Comopustat in order to find the list CEOs who were on Execucomp in the fiscal year 2014. We

then narrow the list down to the group of CEOs who were at the helm of the corresponding firm for at least two years during the sample period of 2004-2015. The next step was visiting these CEO firms' website to collect picture information for these CEOs. We required that the firm has at least one non-10-k form annual report during the period 2004-2015; otherwise, the firm or the CEO is excluded from the sample. The CEO picture prominence information was required to calculate the narcissism score. The details of that are discussed in one of the following sections. In the next step, we collect data on firm-level variables associated with the fiscal year observations. We require that yearly observations have data (one-year lagged values) on the following set of variables: firm-size captured by market capitalization, market-to-book ratio, leverage, return on asset, stock return, and research and development expense. We require that for each yearly observation between the year 2005 and 2015, we have lagged values for these variables (Please see the appendix for variable descriptions). We follow the Fama-French 30 industry classifications to assign our firms to an industry class.

We then collected activism data for every year (fiscal) from 2005 to 2015. We find that there was no activism event in the fiscal year 2005 associated with our sample. Since we use a probit regression model for our primary multivariate analysis, we exclude all the observations from the fiscal year 2005. Moreover, if a particular industry class (as per Fama-French 30 industry classifications) in our data set does not have an activism event, we exclude all the observations associated with that industry class. In our final data set, we have observations from 24 (out of 30) industries.

Our primary dataset has 4766 yearly observations associated with 778 CEOs and 687 firms for the period 2006-2015. For every observation, we have data on activism and one-year lagged values for all the six variables discussed earlier and approximately 96 per cent of those observations (and 95.63 per cent of our firms) are associated with the S&P1500 index.

4.2 Measuring narcissism: Indirect and direct measures

4.2.1 CEO narcissism measure

The success of the study depends (to a large extent) on finding a sensible way to measure CEO narcissism. Survey research has been the most popular choice when it comes to assessing personality traits such as this. The 40-item version of the Narcissistic Personality Inventory (NPI-40) is an effective way to measure narcissism (Raskin and Terry, 1988). The NPI-40 is a forced-choice and self-reported measure of narcissism. Nonetheless, any empirical study exploring managerial behavioural biases or personality traits such as overconfidence and narcissism is potentially problematic since it is almost impossible to run experiments using a relatively large sample of managers. It will be very difficult to get CEOs of renowned public firms to fill surveys due to low response rates and the social desirability bias. The same problem appears when it comes to measuring managerial overconfidence. The rich literature of the narcissism concept has led to the construction of theoretically-sound indirect measures that can be useful for a large sample (Raskin and Shaw, 1988). One workable option is using unobtrusive measures to capture narcissistic tendencies (Webb and Weick, 1979). Our study uses objective variables in order to assess the narcissistic personality dimension of CEOs.

Following prior studies on CEO narcissism, we consider three items in order to measure narcissism level in a CEO. The items are the size and composition of the CEO's picture in the firm's annual report (Chatterjee and Hambrick, 2007; 2011). CEO's relative cash pay, and CEO's relative non-cash pay (Chatterjee and Hambrick, 2007; 2011).

CEO picture prominence ranking: While the CEO photograph could be considered a standard feature of an annual report, not all companies include a CEO photograph, and when one is included, the prominence varies. Chatterjee and Hambrick (2007) considered the prominence of a CEO's photograph to be an appropriate unobtrusive measure for narcissistic personality tendencies because CEOs with higher narcissistic tendencies seek out recognition and admiration to

support their inflated self-concept. A more prominent photograph in the annual report would draw the attention of the financial statement user and promote the recognition that the narcissistic CEO desires (see Chatterjee and Hambrick, 2011; Gerstner et al., 2013; Olsen et al., 2014).

Our study creates five categories to distinguish the eminence of the CEO's photograph on the firm's annual report. We rate the prominence of the CEO's photograph in the annual report as follows:

(1) No photograph of the CEO.

(2) The CEO is either photographed with other executives or photographed alone, but other executives' pictures are also on the page, and the picture occupies less than 35 per cent of the page.

(3) The CEO is either photographed with other executives or photographed alone, but other executives' pictures are also on the page, and the picture occupies more than 35 per cent of the page.

(4) CEO is photographed alone, and his/her picture is the only picture on the page, and the picture occupied less than 35 per cent of the whole page

(5) CEO is photographed alone, and his/her picture is the only picture on the page, and the picture occupied more than 35 per cent of the whole page.

Each CEO gets a picture prominence ranking; the value ranges between 1 and 5 (discrete values) depending on the prominence of his/her picture on the company annual report. In order to determine the picture rankings, we collected annual reports from the company websites. To supplement our analysis, we used experimental data (see section 4.2.2 and 8) and survey data. The survey data is based on responses from 384 student subjects. Subjects were recruited at the CRIBS laboratory of the University of Nottingham, using ORSEE (Greiner, 2015). Survey data show that there is a positive association between subjects' narcissism level and their picture prominence preference.

Relative cash pay and non-cash pay: CEO's relative cash pay is defined as the ratio of the CEO's salary and bonus to that of the second-highest (highest if the CEO is not the highest-paid executive) paid executive at the CEO's firm. Similarly, our measure of relative non-cash pay is the ratio of the CEO's total compensation less cash compensation to that of the second-highest (highest if the CEO is not the highest-paid executive) paid executive at the CEO's firm. Cash pay is the sum of

salary (SALARY in ExecuComp), and bonus (BONUS in ExecuComp) and non-cash pay is computed by deducting the sum of salary and bonus is deducted from the total compensation (TDC1 in ExecuComp). O'Reilly et al. (2014) document that narcissistic CEOs receive higher direct compensation and exhibit more significant disparities in pay compared to other top managers at the CEO's firm. These two measures are included in various studies on CEO narcissism: Zhu and Chen (2015), Chatterjee and Hambrick (2011; 2007), and Patel and Cooper (2013).

The study considers a composite measure of CEO narcissism based on CEO's photograph prominence rating, CEO's relative cash pay, and CEO's relative non-cash pay. The variable is labelled as NS for analysis purposes. Factor analysis is performed on the three components discussed above (relative cash pay, relative non-cash pay, and the photograph prominence rating). Consistent with the notion that three components are capturing some aspect of the same construct (i.e., narcissism), all of the components load on a single factor (with an eigenvalue in the neighbourhood of 1.60). In order to create a continuous summary measure of CEO narcissism, first, we calculate the medians of each component before converting them into standardized values. In the subsequent step, we use the corresponding factor weightings together with standardized values of the three components to calculate a continuous narcissism score for each CEO. The narcissism scores are applied for the period 2006-2015.[6] Since narcissism is a stable psychological trait in individuals (Campbell et al., 2011), the same narcissism score is used for a CEO each year during his/her tenure in the sample period 2006-2015. Table 2 (Panel A) presents the Pearson correlation coefficients among the median values of the three components. All three correlation coefficients are statistically significant (p-value is < 0.01). The correlation coefficients are of similar magnitude compared to the correlation coefficients calculated in some studies on CEO narcissism (e.g. Olsen and Stekelberg, 2015).

[6] We winsorize the continuous variable at 1 per cent and 99 per cent.

4.2.2 The link between narcissism and picture prominence preference: Evidence from survey data

In order to support the main results of the study, we use experimental data collected by running sessions at a renowned university based in central England. The sessions were conducted in the year 2017. The details of the experimental results are discussed in one of the following sections. We use survey data from several experimental sessions conducted at the University of Nottingham. So, we have a pool of 384 subjects for whom we calculated the narcissism score. We calculated the narcissism score for subjects based on the responses associated with forty questions in the NPI-40 developed by Raskin and Terry (1988). The results related to the NPI-40 survey are in Table 1. The highest possible narcissism score is 40. As shown in Panel A of Table 1, the mean (median) narcissism score based on the NPI-40 for the sample is 15.18 (15.50). The corresponding standard deviation is 6.23, suggesting heterogeneity among the subjects. We also use responses associated with a survey question focusing on picture prominence preferences of the participants. Subjects were asked to do the following: "Imagine you are the male person in red sweater in following photographs. You just learned that a local magazine wants to publish a report on you, and they need a picture of you to go with that report. Please indicate which of the 5 photographs you would choose to send to the magazine." Subjects' responses were coded as discrete values 1,2,3,4, and 5 (where 1 is lowest value and 5 is the highest value). The Pearson correlation coefficient between picture prominence score and NPI-40 is statistically significant with a p-value < 0.05 (see Panel B of Table 1). This indicates that there is a significant and positive association between individuals' picture prominence preference and their narcissism level.

The study also used the 10-item version of the Narcissistic Personality Inventory (NPI-10) following Ham et al. (2018). The NPI-10 is derived from the 16-item measure of narcissism, i.e. the NPI-16, in individuals. Ames et al. (2006) developed and validated the NPI-16 which uses a subset of questions in the NPI-40. We calculated the NPI-10 scores for subjects by removing six questions from the NPI-16 that are mostly associated with overconfidence. The study does this in order to

demonstrate that picture prominence preference is distinct from overconfidence. Results are reported in Panel A and Panel B of Table 1. The mean (median) narcissism score based on the NPI-10 for our subject pool is 3.08 (3.00) out of a possible value of 10. The Pearson correlation coefficient between picture prominence score and the NPI-10 is also statistically significant (p-value < 0.05). In contrast, the correlation coefficient between the six items (NPI-6) associated with overconfidence and picture prominence score is insignificant (p-value > 0.10). The insignificant association between NPI-6, which can be used as a proxy for overconfidence level in a subject, and picture prominence score suggest that our archived based measure of CEO narcissism is distinct from overconfidence.[7]

Insert Table 1 from Appendix I

4.3 Sample selection and data collection

The main studies of CEO narcissism thus far have either only investigated big companies or been limited to smaller samples. For example, Aktas et al. (2016) studying the link between CEO narcissism and mergers and acquisition activities is based on 146 deals; the study conducted by Chatterjee and Hambrick (2007) investigates only 111 firms. In the same way, Ham et al. (2018) and Olsen et al. (2015) consider only S&P 500 and Fortune 500 universe (respectively) to investigate the link between CEO narcissism various financial variables.

The study is based on 778 CEOs for the sample period 2006-2015. The size of the sample is significant compared to some notable studies in CEO narcissism literature. The study by Zhu and Chen (2015) considers 300 companies that were part of fortune 500; Chatterjee and Hambrick (2011) consider only 152 CEOs; Patel and Cooper (2013) observe 392 CEOs in their study focusing on CEO narcissism. One strength of our study is that it considers all the major industries and all types of (small, mid-sized and big) firms. More than 64 per cent of the sample firms in this study belong to the non-large firm (firms not included in the

[7] The Spearman correlation coefficients (not tabulated) also show the same pattern, i.e. a positive and significant association between picture prominence preference and narcissism.

S&P 500) category. Unlike S&P 500 or Fortune 500, the S&P 1,500 universe is a proper representation of the US market as it includes small-cap (S&P 600) and mid-cap (S&P400) firms in addition to the largest firms in the US market. The inclusion provides us with an opportunity to study CEO narcissism outside of the typically large-firm universe.

The Fortune 500 and the S&P 500 companies are generally high-profile firms that may attract people with narcissistic tendencies. It seems reasonable that the inclusion of mid-sized and smaller firms would improve the validity of a conclusion by removing the bias that is associated with studies focusing only on more prominent companies. Incorporation of smaller and midcap firms also ensures that the executive characteristics would vary across these firms.

The study chooses to analyse a sample of CEOs from a wide variety of Global Industry Classification Standard industry groups for which data on different CEO specific variables are available from different data sources. Our dataset covers 52 different industry groups (when groups are defined based on two-digit SIC codes). Moreover, our sample has 24 industries out of a total of 30 Fama-French-30 industry groups. The study requires that a CEO was at the helm of a firm for at least two years between the fiscal year 2006 and 2015. This condition is necessary to compute lagged values for different variables for the regression purposes. The study also necessitates that the annual reports from which we collected signatures and pictures are not in 10-k form. 10-k format annual reports do not contain any picture information and usually do not have CEO signatures. We collect annual reports directly from firm-websites. The study requires that the firm has at least one non-10-k format annual report on its website. Most of the firms (approximately 86 per cent) in our sample have at least two, and 75 per cent of the firms have at least three or more annual reports.

Data on activism, the main interest of the research, is collected for testing the hypothesis. This data is mainly coming from Thomson Reuter's database (Shareholder Activism Intelligence). This section contains data on different forms of activism initiated by activists' bodies. The key piece of information collected was the identity of the target and activism announcement dates. The sample period

is 2006-2015. The study is based on 121 activism events in total. If a firm faces multiple campaigns from the same activists, then all of them are counted as a single event, and the event date is set to the announcement date associated with the very first campaign. We have 138 campaigns in our sample. For the regression purpose, we create a binary variable which takes the value 1 if a firm is subjected to at least one activism event during a fiscal year. The value of the binary variable would be 0 otherwise, i.e. if there were no activism event in a given fiscal year.

Institutional holdings are defined as shares held by registered institutions such as insurance firms, investment companies, pension funds, banks, and money managers. A block is comprised of ownership of more than 5 per cent of the company's share for a given fiscal year. We use the calendar year-end (December 31) values to calculate the level of institutional ownership associated with a firm. The data is available form Thomson Reuters Institutional (13f) Holdings - Stock Ownership data set.

4.4 Control variables

The study controls for variables that might influence activists' decision to invest in a firm. The study also controls for variables associated with large shareholders' decision to invest in a firm and post-activism performance improvement. We obtain firm-level accounting data from Compustat. Firm-level variables are in line with the notable works in the shareholders' activism literature (see section 3.1). These are the variables that affect activists' decision to invest in a firm. The study also controls for CEO specific variables. The major challenge faced by researchers interested in CEOs' behavioural biases (such as overconfidence) and CEOs' personality traits (such as narcissism) has been to find valid empirical measures of the relevant construct. It is understood that the true level of overconfidence or narcissism in human subjects is unobservable, but this is required in order to differentiate the influence of irrationality form influences of typical agency concepts. Empirical studies in behavioural corporate finance mainly rely on proxies taken from financial reports or different forms of objective measures of

managerial irrationality. One way to improve the validity of the findings is to control for influential CEO specific variables. CEO specific control variables in the study include the CEO's age, gender, duality and level of overconfidence. It is noted that CEO age, CEO gender, and CEO duality might influence the behaviour of the CEO, and for that reason, how different groups of stakeholders view the CEO. Information on these variables is obtained from Execucomp.

Any study on CEO-narcissism must control for CEO overconfidence as these two variables are closely related. In order to control for CEO-overconfidence, the study uses an option-based proxy as a measure of CEOs' overconfidence. Several studies used the propensity of CEOs' voluntary option holding in their own companies to measure CEO-overconfidence (see Malmendier and Tate, 2015 for a review). As per the study by Malmendier and Tate (2005), not exercising in-the-money stock options (exercising in-the-money stock options produces immediate financial gains for the holder) in his own company is an indication that the CEO has overconfidence in his ability. The study collects data on CEOs' option holding from Execucomp. The study also collects data on institutional shareholding, market concentration form Thomson Reuters Institutional (13f) Holdings - Stock Ownership data set and data on board size from BOARDEX.

4.5 Econometric approaches

The principal interest of this research is the connection between CEO narcissism and the possibility of shareholder activism events. The study follows previous works in this area in constructing econometric models to check the link between the likelihood of shareholder activism and CEO narcissism. The study also explores some other important relationships that might hinge upon CEO narcissism.

The study is based on 778 CEOs associated with 687 firms and 4766 firm-year observations. To test the central hypothesis of the paper, we initially run a probit regression with only CEO observations. In the regression, the dependent variable is a dummy taking the value 1 if the CEO faces an activism event during the sample

period (2006-2015) 0 or else. We ignore the time dimension for this regression analysis and construct the following model:

$$ActivistCampaign_i = \alpha + \beta_1 * NS_i + \beta_2 * Z_i + \varepsilon_i \ ....(1)$$

The study uses a three-item CEO narcissism score based on the CEO's picture prominence in the annual report, CEO's relative cash pay, and CEO's relative non-cash pay. In multivariate analyses, this composite measure takes the variable name. Consistent with prior literature is assumed to be time-invariant (Chatterjee and Hambrick, 2007). In the regression, Z contains other time-invariant CEO specific control variables. We have just one time-invariant CEO-specific variable in the form of MALE (i.e. CEO gender) which takes the value 1 if the CEO is a male and 0 otherwise. The study uses robust standard error clustered by firm ID to calculate the significance levels in the regression analysis.

The probability of activism is then modelled as the outcome of a probit regression with robust standard error clustered by firm ID ((i.e. gvkey number for each firm) where we use all the firm-year observations. The dependent variable is set to 1 if an activist shareholder targets the firm in a given year, and 0 otherwise. Year fixed effects and industry fixed effects are included when we use all the firm year observations. We do include industry and year fixed effects in regressions based on matching firms as controls. In the multivariate hypothesis tests, we control for several other factors that may influence the possibility of activism events. Regression results are based on the following probit regression:

$$ActivistCampaign_{i,t} = \alpha + \beta_1 * NS_i + \beta_2 * X_{i,t-1} + \beta_3 * Y_{i,t-1} + \beta_4 * Z_i + \varepsilon_{i,t} \ ....(2)$$

Here X is the vector containing all firm-specific control variables, Y all time-variant CEO specific control variables, and Z contains other time-invariant CEO specific control variables. The control variables selected are in line with Brav et al. (2008), Edmans (2014), Bethel et al. (1998), and Del Guercio et al. (2008). The study uses a three-item CEO narcissism score based on the CEO's picture prominence in the annual report, CEO's relative cash pay, and CEO's relative non-cash pay. In multivariate analyses, this composite measure takes the variable name NS_i. All continuous variables included in the analysis are winsorized at the 1 per

cent and 99 per cent levels. The study controls for firm-size, as measured by the log of market capitalization, market-to-book ratio, operating performance, leverage, RandD expenses, annual stock return, institutional ownership percentage, and the number of institutional blockholders. CEO specific control variables in the study include the CEO's age, gender, duality and level of overconfidence. The choice of these variables is in line with studies on CEO personality traits (e.g. Chatterjee and Hambrick, 2007; Olsen et al., 2013; Aktas et al., 2016).

In addition, we model the probability of activism as the outcome of a random effects probit regression with robust standard error clustered by firm ID ((i.e. gvkey in Compustat). The estimations are based on panel data regression where panel variables are gvkey and fiscal year. The dependent variable is set to 1 if the firm is targeted by an activist in a given year and 0 otherwise. We include year fixed effects and industry fixed effects on the regression. In the multivariate hypothesis tests, we control for several other factors that may influence the possibility of activism events. Regression results are based on the following panel data regression:

$$ActivistCampaign_{i,t} = \alpha + \beta_1 * NS_i + \beta_2 * X_{i,t-1} + \beta_3 * Y_{i,t-1} + \beta_4 * Z_i + \varepsilon_{i,t} \quad ....(3)$$

Here X vector has all firm-specific control variables, Y all time-variant CEO specific control variables, and Z contains other time-invariant CEO specific control variables.

The second hypothesis, i.e. large institutional investors are less likely to be associated with narcissistic CEOs, is tested with the following specifications; a random-effects regression with robust standard error clustered by firm ID ((i.e. gvkey from the Compustat). The estimations are based on the following panel data regressions where panel variables are firm ID and fiscal year.

$$INSTBOWN_{i,t} = \alpha + \beta_1 * NS_i + \beta_2 * X_{i,t-1} + \beta_3 * Y_{i,t-1} + \beta_4 * Z_i + \varepsilon_{i,t} \qquad (4)$$

$$LRG5OWN_{i,t} = \alpha + \beta_1 * NS_i + \beta_2 * X_{i,t-1} + \beta_3 * Y_{i,t-1} + \beta_4 * Z_i + \varepsilon_{i,t} \qquad (5)$$

The dependent variables are INSTBOWN and LRG5OWN, log value of the shares owned by an institutional blockholder and log value of the number of shares owned by five largest institutional shareholders of the company, respectively. As

in the previous cases, vector X contains all firm-specific control variables, Y all time-variant CEO specific control variables, and Z contains other time-invariant CEO specific control variables. In addition to the control variables discussed previously, we include BOARDSIZE and HHI, the number of directors in the firm board as a proxy for board power and firm's market concentration measure, respectively.

4.6 Sample of peer firms

To test the hypotheses formed in the earlier parts of the paper, we use matched counterfactuals that are similar to the sample of target firms (in the year prior to activism). We follow three approaches to construct three sets of peer firms. In order to construct matched counterfactuals, one matching firm is selected that is similar to the target firm in the year prior (if activism event takes place in year *t* then a matching firm is selected in year *t-1*) to the event year. We find a firm which was similar to the target firm in year *t-1* but did not face any activism event in year *t*. We use the following approach to select the first set of matching firms: i. We identify a peer company from the same industry in the year (fiscal) before the event for which we have data on CEO narcissism. The industry classification is based on Fama-French 30 industry classification. ii. Every target and matching firms than ranked across SIZE and MB ratio in the year before the activism event. The study than scales the rank so that we have values between 0 and 1 for each firm. We subsequently find a peer firm from the same industry group for which the value of the following equation is the closest to zero in the prior year in relation to the activism-event year.

$$\sqrt{(\boldsymbol{SIZE\ scaled\ rank\ target} - \boldsymbol{SIZE\ scaled\ rank\ peer})^2 + (\boldsymbol{MB\ scaled\ rank\ target} - \boldsymbol{MB\ scaled\ rank\ peer})^2}$$

We calculate a pair-wise distance score employing the SIZE and MB scaled rankings (Jayaraman et al., 2015). In the first set of control firms we have firms that are similar to the target firms in terms of industry, the market value of equity (SIZE) and market to book ratio (MB).

In the same manner following another approach, we select a peer firm from the same industry group for which the value of the following equation is the closest zero or to the value associated with the target firm (the value for the target firm is 0) in the year before the event.

$$\sqrt{(\boldsymbol{SIZE\ scaled\ rank\ target} - \boldsymbol{SIZE\ scaled\ rank\ peer})^2 + (\boldsymbol{MB\ scaled\ rank\ target} - \boldsymbol{MB\ scaled\ rank\ peer})^2 + (\boldsymbol{ROA\ scaled\ rank\ target} - \boldsymbol{ROA\ scaled\ rank\ peer})^2}$$

In the second set of peer firms we have firms that are similar to target firms in the year prior to the activism event in terms of industry, the market value of equity (SIZE), market to book ratio (MB), and return on asset (ROA). In both approaches, we select a peer firm from the same industry group. Since there were 121 activism events in 114 fiscal years, we find 114 peer firms (and 114 CEOs) for each set of matching firms.

In addition, the study follows Brav et al. (2008) and Berbar and Lyon (1996) to construct the third matched counterfactual. We name this portfolio of firms the 10*10 portfolio. The study selects a company from the same SIC two-digit industry whose ROA in the year t-2 (relative to the event year) is not beyond 90 per cent or 110 per cent of the target firm ROA. If we do not get a firm in the same two-digit industry, we relax the assumption (following Brav et al., 2008) and find a firm from the one-digit industry. If we still do not get a firm with the 90 per cent or 110 per cent condition, we exclude that firm (event year) from the analysis using the 10*10 matching firm portfolio. There were 9 cases like this. SO, we had 105 matching firms in the 10*10 portfolio.

4.7 Activism news and abnormal return

We analyse market reaction to activism news announcements associated with the target firms in the sample during the period 2006-2015. The interest for this part of the research are the abnormal return surrounding the activism news announcement dates and the cumulative abnormal return (CAR) for a specific window. The study investigates an event window with 21 daily returns from day -10 to day +10 and an event window with 11 daily returns from day -5 to day +5.

CAR is the cumulative figure of corresponding daily abnormal returns for a specific window. In this paper, daily abnormal returns are based on the Fama-French Four-factor model. The calculation of CARs of relevant target firm portfolios is as follows:

$$AR_t = \frac{1}{N}\sum_{i=1}^{N} e_{it}$$

$$CAR(t_1, t_2) = \sum_{i=1}^{N} AR_t$$

Here AR_t is the average abnormal return (of a portfolio containing N assets) and CAR is the daily cumulative abnormal return figure. CAR approach uses each period's average abnormal returns (AR) between a time horizon of t_1 and t_2 (Kothari and Warner, 1994). In this study CAR approach is used to calculate the abnormal return of event firm portfolio around activism announcement dates and abnormal return prior to announcements of activism campaigns. Patel Z was used as the significance test for the mean CARs as it is the basic sign test for mean CAR.

The study uses the Fama–French four factor (market, size, book to market, and momentum) model as the asset-pricing model to generate the expected daily return. The estimation window contains 100 trading days, and the gap between the estimation window and the event window has 30 days. The abnormal return calculated is adjusted for the corresponding firm's systematic risk as captured by its market beta, size and book to market ratio. Fama-French Four-Factor Model uses abnormal returns defined as: $AR_t = R_t\text{-}E_{t-1}[R_t]$

The study also looks at the buy-and-hold abnormal return (BHAR) of event firm portfolios to estimate long-run abnormal return following the event. We choose the BHAR method as it replicates the true extent of returns from an investment strategy.

In this approach, the focus is on the difference between buy-and-hold returns associated with the companies that experienced an event and the matched portfolios based on some characteristics. The BHAR approach involves calibration of the

average return from a strategy of buying in all event firms and shorting otherwise comparable firms that did experience the event within a given period. We follow the Barber and Lyon (1997) and Kothari and Warner (1997) framework to calculate the BHAR.

The BHAR is defined as the difference between the realized buy-and-hold return and the normal buy-and-hold return:

$$BHAR_{it} = \prod_{t=1}^{T}(1 + R_{it}) - \prod_{t=1}^{T}(1 + E(R_{it}))$$

Where

$$\prod_{t=1}^{T}(1 + E(R_{it})) = \prod_{t}^{T}\left(1 + \sum_{i=s}^{n_t} w_{it} R_{it}\right)$$

For each event firm, the BHAR is the difference between the holding period return for that firm and the holding period return for some benchmark asset. Here R_{it} is the security return (security *i*) and *t* is the period of investment Here w_{it} is either $\frac{1}{n_t}$ where n_t is the number of securities in period *t* or a value-weight element. The benchmark return is a proxy for the normal or expected return of the event firm. Conventionally, the benchmark is a reference portfolio (see Barber and Lyon (1997), Lyon, Barber, and Tsai (1999), and Brav (2000)). That is, each event firm is matched to a firm in a portfolio of firms with similar attributes, such as size and book-to-market.

5. Descriptive Statistics

5.1 CEO narcissism score

Panel A of Table 2 reports descriptive statistics for the individual components of our narcissism measure for the 778 individual CEOs in the full sample. Panel B reports correlations among the three elements of the measure. Consistent with the idea that the three items capture some component of the same underlying construct, the three items, CEO's picture prominence in the annual report, CEO's relative cash pay, and CEO's relative non-cash pay are all highly significantly and positively correlated (all p-values are less than 0.01). Panel B also reports the

Pearson correlation coefficients among the option-based CEO overconfidence measure CONFIDENCE and three elements of our narcissism measure. We do not find any significant association between CONFIDENCE and CEO picture rating; the Pearson correlation coefficient is statistically insignificant. Similarly, there is no significant association between CONFIDENCE and CEO relative non-cash pay. Consistent with the prediction that overconfidence and narcissism are two distinct constructs, the Pearson correlation between CEO overconfidence measure (CONFIDENCE) and CEO narcissism measure (NS) is statistically insignificant at r =0.031 (p-value > 0.10). Similarly, the Pearson correlation between CEO overconfidence measure (CONFIDENCE) and CEO narcissism indicator measure (NS80) is statistically insignificant at r =0.004 (p-value > 0.10).

Insert Table 2 from Appendix I

Panel C of Table 2 reports the summary statistics associated with the CEO narcissism scores by partitioning the full sample into target and non-target CEO groups. The mean CEO narcissism score calculated based on CEOs picture, relative cash pay, and relative non-cash pay and as captured by the variable NS is -.043 with a standard deviation of 0.871, suggesting a substantial variation among CEOs in the sample. Out of a total of 778 CEOs in the full sample, 102 faced at least one activism event, and 676 did not face any activism event during the sample period of 2006-2015.

We then look at the level of narcissism associated with the CEOs in both groups. The mean narcissism score for CEOs targeted by activists is much higher than the mean score associated with the CEOs not targeted by shareholder activists. The mean for CEO narcissism measure variable NS is 0.158 for the target group, much higher than the sample average of -0.043. On the other hand, the mean value for NS for the non-target CEO group is -0.073. The mean difference between the target and non-target CEO NS is statistically significant with a p-value less than 0.05 (the two-tailed p-value is based on a two-sample t-test assuming unequal sample variances). It emerges that CEOs who faced activism events are more narcissistic compared to their counterpart CEOs who did not face activism events during the sample period 2006-2015. The results are in line with the hypothesis that

narcissistic CEOs are more likely to be targeted by shareholder activists. We also compare the target CEOs with a sample of CEOs matched on industry, SIZE, and MB ratio. The mean value (i.e. -0.120) for the variable NS associated with the non-target matched firms is again statistically significantly different (p-value < 0.05) compared to the target CEO mean.

In addition to a continuous measure of CEO narcissism NS, the study uses a narcissism indicator variable that compares CEOs with a relatively high narcissism level to less narcissistic CEOs. The indicator variable is NS80. The value of NS80 is 1 if the CEO is among the top 20 per cent of CEOs in our sample and 0 otherwise. A comparison between the target and non-target CEOs based on the narcissism indicator variable NS80 shows that the mean is significantly higher for the target CEO group (p-value < 0.05). This is true when we compare the target CEOs with the rest of the non-target CEOs as well as when we compare the target CEOs with CEOs of a matched counterfactual (see Panel C of Table 2) based on industry, SIZE, and MB. Panel D of Table 2 reports the frequency associated with the annual reports collected from company websites to construct the CEO narcissism proxy. The study collects annual reports (non-10-k format) from company websites.

In Table 3, we present the descriptive statistics associated with the key variables in this study. Variable definitions can be found in Appendix 1.1. The table reports mean, median, and standard deviation for each variable. All the continuous variables are winsorized at 1% and 99% level. We summaries different firm-specific, CEO-specific, and institutional shareholding variables in this table.

Insert Table 3 from Appendix I here

5.2 Activism events

Table 4 reports the details regarding the activism events in our paper. The study is associated with 121 activism events consisting of 138 activism campaigns that took place during the sample period 2006-2015. A significant portion of the activism events in our sample were initiated in the latter half of the sample period. The year 2014 and 2015 saw the highest number of activism events and the first

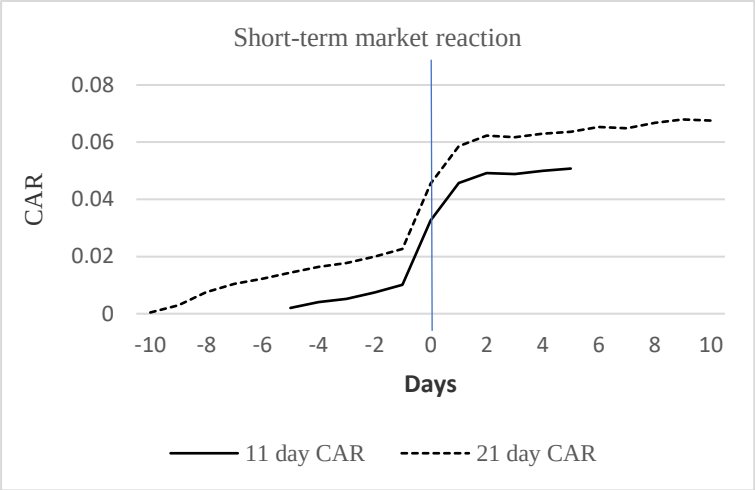
two years, 2006 and 2007, observed only three events combined. Some firms in our sample faced more than one activism event in the same fiscal year, and for that reason, the fiscal year where a firm initiates at least one activism event is less than 121. The primary multivariate analysis of the study uses 4766 fiscal-year observations, and 114 of those years were associated with at least one activism event. Majority of the firms (approximately 81 per cent) faced only one activism events. The number of target firms in the sample is101 and 19 firms in the sample faced more than one activism event with one firm facing three events.

Insert Table 4 from Appendix I here

The results related to short-term market reaction around activism campaign announcement dates are discussed in this part of the study. Short term market reaction is summarized in Figure 1. A positive short-term market reaction to activism news is observed. Short term market reaction is quantified based on a 21-day (-10, +10) CAR (black dashed line in Figure 1) and an 11-day (-5, +5) CAR (solid black line in Figure 1) using Fama-French four-factor model. The results are in line with the general notion that intervention by activist shareholders is perceived favourably by the stock market, with strong evidence of short-term run-ups in target firms' stock prices. An analysis of the 21-day (-10, +10) CAR shows that the portfolio of all target firms experienced an average cumulative abnormal return of 6.75 per cent which is significantly different from zero as indicated by a Patel z value more than 10 (p-value < 0.0001). The corresponding matching portfolio, however, earned zero abnormal return (untabulated) for the same window. The (-5, +5) CAR, which is slightly greater than 5 per cent (Patel z >11), is also indicating a positive reaction to the event announcements. We did not find any significant association between short term market reaction to activism news and CEO narcissism. Our results are consistent with the studies on hedge fund activism and contested proposals. Studies such as Brav et al. (2008), Clifford (2008), Greenwood and Schor (2009), Brav et al. (2009), Klein and Zur (2009), Boyson and Mooradian (2011), Becht et al. (2017) report average abnormal around the event date for various windows ranging between 3.39 per cent and 6.97 per cent. The reported test statistics (typically significant at 1 per cent) are also similar

to our test statistics. The comparison suggests that our sample of activism events is similar to some notable studies in the literature on shareholder activism.

Figure 1: Short term market reaction around activism news. Cumulated abnormal return (CAR) is calculated based on Fama-French four factor model.[8]



Panel B of Table 4 presents information on the type of activist in our sample. Sixty-eight unique activists undertook a total of 121 activism events. Hedge fund activists initiated approximately 56 per cent of the events, and the rest were associated with non-hedge fund activists such as non-hedge fund financial entities and corporations. Thirty-three unique hedge funds initiated a total of 68 events, and 35 non-hedge fund activists originated 53 events. There are noticeably more serial activists amongst hedge funds. For example, the hedge funds ValueAct Partners, Carl Icahn, JANA Partners, Trian Fund Management, Elliot Management, Starboard Value, Relational Investors each initiated four or more activism events in our sample. Table 4 also reports descriptive associated with the event outcome and demand of shareholder activists. The most frequent demands (Panel D of Table 4) are seeking alternatives and board representations with shareholder rights in the third position. Board control, spinoff, strategic discretions, force sale, hostile acquisition are the other forms of demands. The most frequent

[8]Available from: https://wrds-web.wharton.upenn.edu/wrds/ds/wrdseventstudy/index.cfm?navId=355

activism outcome was settled, followed by Management victory in the second place (Panel E of Table 4). The study checks whether there any relationship between the frequency of the known outcomes of activism events and CEO narcissism, there was not any such relationship.

Table 5 reports key statistics on firm-level variables separately for firms that faced activism events and for firms that did not face any activism event during 2006-2015. We compare all the firm-year values in our sample (2006-2015) associated with the target firms with the firm-year values of non-target firms in the full sample. It is noted that firms that faced activism events showed poor performance and are undervalued compared to firms that did not face activism events. In Table 5, we also compare the target firm years with firm-year observations associated with a set of peer firms. Target firms appear to be only marginally bigger (measured by the log of market capitalization), similar in terms market-to-book-ratio (MB), and similar in terms of stock return compared to non-target firms when we consider all the firm-year observations.

In Table 6, we present comparisons between target firms and three sets of matching firms. The comparison focuses on a set of key firm-level variables used in our multivariate analysis. We compare target firm values in the year before activism (e.g. values in year *t-1* if activism event is taking place in year *t*) to matching firm values in the same year. We do not observe any significant differences between the target firms and the matching firms. None of the variables is consistently significantly different across these comparisons. Perhaps stock return or RET is worth mentioning. The variable RET is significantly different when we compare target firm RET with industry, MB, and SIZE matched counterfactual RET and industry, MB, ROA, and SIZE matched counterfactual RET, but not different when we compare with the 10*10 matched counterfactual RET. In contrast, the mean CEO narcissism score (NS) for the target-firm group is significantly higher from the mean associated with the counterfactual across three comparisons; two-tailed P-value < 0.05 based on a t-test assuming unequal variance.

Insert Table 5 & 6 from Appendix I here

6. Main Results

6.1 CEO narcissism and shareholder activism

The main result of the study is presented in Table 7. Consistent with the prediction discussed in previous sections, significantly positive coefficients on the measures of CEOs' level of narcissism (i.e. NS and NS80) are found for different specifications. The results tabulated in Table 7 is indicative of the fact that there is indeed a positive association between CEO narcissism and the odds of activism events. This highlights the fact that high level of narcissism in individuals is associated with questionable behaviour, which attracts interests from shareholder activists. The Studies associated CEO narcissism with bold and risky actions, self-serving behaviour, and fraudulent activities (e.g. Chatterjee and Hambrick, 2007; Petrenko, 2016); these actions fuelled by narcissistic tendencies put CEOs under the spotlight, which evidently increases the chance of facing activism campaigns.

The probability of activism events is modelled as the outcome of a probit regression. We initially run a simple probit regression where the dependent variable takes the value 1 if the CEO faced an activism event during the period 2006-2015 and 0 otherwise. The total number of CEOs who received at least 1 activism event is 102, and the total number of CEOs who did not receive any activism event is 676. There are only two explanatory variables in this probit regression setting, and both are time-invariant. Panel A of Table 7 reports the results. The key explanatory variable is CEOs' level of narcissism captured by the variable NS calculated based on CEO's picture prominence, relative cash pay, and relative non-cash pay ranking (explained earlier). The other variable is CEO gender (i.e. MALE) that takes the value 1 if the CEO under consideration is a male. We also use a CEO narcissism indicator variable in the form of NS80. The variable NS80 takes the value 1 if the CEO's narcissism score is among the top 20 per cent of the narcissism scores in our sample of 778 CEOs and 0 otherwise. In specification 1 to 4 of Panel A, CEO narcissism score is a continuous value, and in specification 5 to 8, it is discrete (i.e. NS80).

In specification 1 and 5, we consider all the CEOs in our sample to identify whether the presence of a narcissistic CEO increases the probability of an activism event. We also employ matched sample analyses to check whether there is a positive and significant association between CEO narcissism and investor activism. In the rest of the specifications in Panel A of Table 7, the regression results are based on a pool of target CEOs and peer firm CEOs. In specification 2 and 6 (3 and 7), the regression uses a sample of matching firms based on industry, SIZE, and MB (industry, SIZE, MB, and ROA). The method we followed in selecting a peer firm for each event year is discussed in one of the previous sections. The number of fiscal years where at least one activism event occurred is 114, and therefore we selected 114 firms (and 114 CEOs) to construct a set of peer firms. In Model 2, 3, 6, and 7, the number of CEOs who received at least 1 activism event is 102, and the number of matching firm CEOs who did not receive any activism event is 114 bringing the total to 216 observations (CEOs).[9] Specification 4 and 8 are based on 10*10 portfolio matched firms.[10] The total number of observations is 201.

We use the continues measure of CEO narcissism (NS) as well as the narcissism indicator variable NS80. The coefficients associated with the CEO narcissism proxy in all the specifications are statistically very significant (Panel A of Table 7), indicating a strong and positive association between CEO narcissism and the likelihood of an activism event. The p-values are either less than 0.05 or less than 0.01 and suggestive of the fact that highly narcissistic CEOs are more likely to face an activism event compared less narcissistic CEOs. This is true when we use the full sample as well as when we use a matched counterfactual.

We compute the predicted probability in specifications (5 to 8 in Panel A of Table 7) where CEO narcissism dummies (NS80) are used as the primary

[9] It should be noted that some CEOs faced more than one activism event during the sample period. It should also be noted that some fiscal years in our sample observed more than one activism event. As mentioned before, a total of 121 activism events (faced by 102 CEOs) were associated with 114 fiscal year observations.

[10] The number of target CEOs for whom we could get a matching firm in the 10*10 approach is less than 102. Therefore, the total number of observations in this approach is lower (201) than the number of observations in two other approaches.

explanatory variable. The predicted probability of an activism event is always significantly higher when the CEO is high in narcissism. The predicted probability of at least one activism event during the period 2006-2015 in specification 5 (where we use the full sample of CEOs and NS80 as the narcissism indicator variable) goes up by approximately 69 per cent when the CEO is a narcissist, i.e. NS80=1. The difference is statistically significant at the 5 per cent level. Using matching firm observations produces the same pattern of results. The increase in predicted probability ranges between 47.20 per cent to 92.44 per cent when the CEO is a narcissist, i.e. NS80 = 1 (p-value < 0.01).[11] Highly narcissistic CEOs in every specification is significantly more likely to face an activism event during the sample period (2006-2015).

Since results in Panel A are based on only a handful of time-invariant control variables and shareholder activism can be affected by many other factors, we test the same hypothesis with more control variables. Panel B of Table 7 reports pooled probit estimations of factors that are related to an activist's decision to approach a firm. In Panel B, we have eight specifications. In column 1 to 4, CEO narcissism scores, i.e. NS, are continuous values. In column 5 to 8, CEO narcissism dummy, i.e. NS80, is used as one of the explanatory variables. In Model 1,3,5, and 7in addition to NS or NS80 as a proxy for CEO narcissism, some key firm-specific variables are included as controls to take into account of other related factors (see Appendix A for variable definitions). In the other specifications, we include firm-specific variables as well as CEO-specific variables and institutional ownership variables.

First, we model the probability of an activism event by using all the available yearly observations in our sample (See Column 1,2,5, and 6). The coefficient on CEO narcissism variable NS is positive and significant (p-value < 0.05) in Model 1 and 2. The dummy variable NS80 is also significantly associated with the probability of an activism event (p-value < 0.01 in specification 6). This is an

[11] The significance level indicates whether the difference in the coefficients for different levels of NS80 is statistically significant.

indication that relatively high narcissism CEO managed firms are more likely to be targeted by activist shareholders, compared to firms with relatively less narcissistic CEOs, even after controlling for a battery of standard factors that can affect the probability of activism events. The result is consistent with the notion that individuals with narcissistic tendencies are more likely to be associated with behaviour, which raises more questions due to their manipulative natures and senses of superiority and privilege. Among control variables, firm size, market to book ratio, return on asset, and stock return, are significantly associated with the probability of being targeted by an activist. The first variable is positively associated, and the latter three show a negative association with the likelihood of activism events. The results are generally in line with some notable studies in the literature. Studies suggest that underperforming and undervalued firms are more likely to face activism events. Clifford (2008), Cai and Walkling (2011), and Renneboog and Szilagyi (2011) showed a positive connection between activism and firm size, i.e. larger firms are more prone to shareholder activism events. Like our study, previous studies such as Del Guercio et al. (2008), Clifford (2008), Greenwood and Schor (2009), and Boyson and Mooradian (2011) show that firms with poor prior stock return and low market-to-book ratio (MB in our context) are more likely to be targeted by activists. We find similar signs for the control variables in specifications where we use all the available yearly observations. The return on assets (ROA) as measured by the operating performance appears to predict activism events suggesting that the non-market-based measure of firm performance too can predict shareholder activism events. We find that CEO overconfidence, a variable often closely associated with CEO narcissism, generally does not predict the probability of activist events.

In Panel B of Table 7, we also present results from probit regressions to identify the effects of all covariates using a sample of non-target matched firms. As discussed before, firms are matched on Fama-French-30 industry classification, SIZE, and MB. Column 3,4,7, and 8 in Panel B presents the results. CEO narcissism proxies NS and NS80 are statistically significant (with p-values < 0.01), suggesting that target firm CEOs are more likely to be a narcissist compared to

CEOs of similar non-target firms, i.e. firms of similar size and market-to-book ratio and from the same industry. Some of the covariates significant in the full sample are not consistently significant in the matching firm probit regressions, but CEO narcissism proxies are statistically very significant (p-value < 0.01) in all the specifications.

We report the predicted probability of an activism event in specifications involving CEO narcissism indicator variable (i.e. NS80). In line with our initial results, we find that highly narcissistic CEOs are associated with greater odds of facing an activism event. When we use NS80 as the narcissism indicator variable, in the full sample the predicted probability of an activism event goes up from 1.3 per cent to 2.3 per cent when the CEO is narcissistic (the difference is significant at 1 per cent level).[12] In the matched sample regressions, the increase in the predicted probability of an activism event when the CEO is a narcissist (i.e. NS80 = 1) is in the neighbourhood of 60 per cent. For example, in specification 7 the predicted probability of an activism event is 69.0 per cent when NS80 takes the value 1, significantly higher (p-value < 0.01) compared to a predicted probability of 44.3 per cent when the CEO is not highly narcissistic. In Panel B, we present the results. In specification 8 the corresponding increase in predicted probability is 66.1 per cent when the CEO is narcissist (i.e. when NS80 =1; p-value < 0.01).

We check whether the primary finding of the study that narcissistic CEOs are more likely to be confronted by shareholder activist is robust to using different peer firm samples for running regressions. We re-estimate Model 3,4,7, and 8 in Panel B of Table 7 with two more sets of peer firms. Results are reported in Panel C of Table 7. We run regressions using an industry, MB, SIZE, and ROA matched counterfactual (Column 3,4,7, and 8 in Panel C) and then a 10*10 portfolio counterfactual (Column 1,2,5, and 6 in Panel C) as controls. Using different sets of control firms did not change the results at all. We find that in both regressions, the variable CEO narcissism proxy NS is statistically very significant in predicting

[12] The significance level indicates whether the difference in the coefficients for different levels of NS80 is statistically significant.

activism events. In all the specifications, CEO narcissism proxies NS and NS80 are statistically significant (with p-values < 0.01). In the matched sample regressions, the increase in the predicted probability of an activism event when the CEO is a narcissist (i.e. NS80 = 1) ranges between 55 per cent to 99 per cent. For example, in specification 8 the predicted probability of an activism event is 70.4 per cent when NS80 takes the value 1, significantly higher (p-value < 0.01) compared to a predicted probability of 44.0 per cent when NS80 is equal to 0.

Insert Table 7 from Appendix I here

6.2 CEO narcissism and institutional blockholding

Table 8 presents the results related to the second hypothesis. The main interest here is the impact of CEO narcissism on the level of institutional blockholding. The study chooses the log value of the shares owned by institutional blockholders and the log value of the shares owned by five largest shareholders of the firm under consideration as the variables of interests. In the regression models, these two variables are the dependent variables. The variables are defined as INSTBOWN and LRG5OWN in the analysis.

The results presented in Table 8 are based on panel data random-effects regressions, where the significance levels are calculated using robust standard errors clustered by a firm identifier (gvkey). The key explanatory variable is the CEO narcissism proxy (NS) discussed in earlier sections. We use both the continuous measure of CEO narcissism and a dummy variable NS80 that takes the value one if the CEO's narcissism score is among the top 20 per cent scores in the sample. Model 1 and 3 have INSTBOWN as the dependent variable and Model 2 and 4 have LRG5OWN as the dependent variable. Model 1 and 2 use continuous values for the CEO narcissism proxy NS, and in two other specifications in Table 8, NS is defined as indicator values. The estimated coefficients on CEO narcissism variable concerning the dependent variables INSTBOWN and LRG5OWN are negative and statistically significant (p-value < 0.03 in Model 1,2, and 3 and <0.05 in Model 4). In addition to NS as a CEO narcissism proxy, all the specifications

have a set of firm-specific variables and a set of managerial level variables as controls. The result is consistent with the notion that the level of sizeable institutional shareholding is negatively associated with the level of narcissism in the CEO, i.e. large institutional shareholders are more likely to be discouraged by narcissistic tendencies in a CEO. This pattern can be linked to Edmans (2014) who suggests that many blockholder groups pursue a passive investment strategy, i.e. hold large stakes purely for diversification purposes.

Narcissism has frequently been considered as a dark side of personality and certainly shapes part of the dark triad of personality traits since a narcissistic individual is often associated with arrogance and immodesty (Resick et al., 2009; Paulhus and Williams, 2002). They are exploitative because they lack empathy (Brunell et al., 2008), and they seek recognition and praise as self-enhancement opportunities to support their grandiose senses of superiority (Campbell et al., 2004b; Wallace and Baumeister, 2002). So, it is not surprising that certain shareholder groups will find those tendencies unacceptable and hence try to avid CEOs with those tendencies.

Evidence suggests that an overwhelming proportion of large shareholding are passive forms of investment (see Greenwood and Schore, 2009). It is reasonable to believe that they will behave differently from activist shareholders as their objectives are dissimilar. The findings are not contradicting our initial hypothesis and the related finding that activist shareholders are more likely to target a narcissistic CEO. We suggest that most large shareholders are passive and passive shareholders are far less likely to assume the monitoring role. The work of Chatterjee and Hambrick (2007) showed that narcissistic CEOs are associated with extreme (positive or negative) outcomes and our results suggest that a passive shareholder may find this feature less than ideal. An activist shareholder might see the instability in a firm managed by a narcissistic CEO as an opportunity whereas a passive large shareholder is more likely to value stability which a narcissistic CEO is unlikely to offer. Our results are in line with studies that connect the personal dimension narcissism to lack of empathy and then to the exploitation of others (see Campbell et al., 2011). Our findings are also reinforcing the belief that

and they are more likely to pursue personal objectives instead of pursuing organizational goals (see Hornett and Fredericks, 2005).

Insert Table 8 from Appendix I here

6.3 Shareholder activists and performance improvement

The study next investigates whether CEO narcissism has any significant association with the post-intervention performance improvement. It is plausible to suggest that the negativity associated with the personality trait narcissism would have an adverse impact on the post-activism value creation for a target company. In order to test the hypothesis that there is a link between a CEO's level of narcissism and post-activism performance improvement we calculate the buy and hold abnormal return for the event firms following the activism events. We calculate 18-month BHAR and 24-month BHAR following Barber and Lyon (1997).[13] Panel A of Table 9 reports the results. We report both equal-weighted and value-weighted BHARs. The 18-month BHAR for firms managed by highly narcissistic CEOs is significantly lower (p-value < 0.05) than the 18-month BHAR associated with the portfolio of less narcissistic CEOs. The high (low) narcissism target CEOs are those with NS in the top (bottom) 20 (80) per cent of the CEOs in our sample according to their narcissism scores. The 24-month BHAR associated with the highly narcissistic CEO group is also lower compared to the BHAR for the less narcissistic CEO portfolio (the difference, however, is statistically insignificant).

The other performance criteria are ROA and RET. Our analysis is based on two matched counterfactuals. We calculate the ROA and RET for two years before the activism event, the year of activism event, and two years following the activism event. We do the same for peer firms. In the next step, we calculate the difference associated with each target-peer pair for ROA and RET. We do that for every year between t-2 and t+2. In Table 9, the mean values are reported. Panel B shows the

[13] Available from: https://wrds-web.wharton.upenn.edu/wrds/ds/wrdseventstudy/longrun/index.cfm?navId=355

comparison between target firms' and matching firms sorted based on industry, MB, and SIZE. The results in Panel C are associated with a sample of matching firms selected based on industry, MB, SIZE, and ROA (please see earlier discussions on matching firm selection process).

Panel B and Panel C report the mean of the differences for each variable. Reported figures in the last two rows in both panels are based on pre and post year comparisons. The study compares the post-event value (i.e. values in year t+1 and t+2) with the value in the year before activism. We do that separately for firms with narcissistic CEOs (top 20 per cent in terms NS) and firms with non-narcissistic CEOs. In both matching approaches, we can see that narcissistic CEOs are less likely to be associated with post-activism performance improvement. Narcissistic CEOs produced poor results for their firms in the year following the event when measured by RET. Our results (not tabulated) are similar if use industry adjusted returns. The agency theory suggests that the magnitude of improvement in performance should be more prominent for firms suffering more from agency problem. Assuming that a high level of narcissism CEOs is associated with the severity of agency problem within firms, we can expect a negative association between post activism performance improvement and CEO narcissism.

The personality of CEOs' is an important factor to shareholder activists because it is essential for them to engage in conversations with the company management in order to achieve their goals. The CEO is one of the key figures involved in the conversation. Among other things, narcissism can influence an individual's cooperation, trust, motivations, and decision-making (Campbell et al., 2004). High level of narcissism in CEOs is expected to be associated with a high level of disagreement and conflict as narcissistic individuals value themselves highly, and they are less attentive to the needs of others. Therefore, performance improvement will be difficult within target firms with narcissistic CEOs at the top of the executive tree even though there are rooms in those firms for improvements.

Insert Table 9 from Appendix I here

7. Robustness Checks

Although our primary results suggest that CEOs with a high level of narcissism will face more activism events, we cannot cancel out the possibility that there could be alternative interpretations. Our assumption is that specific features in a narcissistic manager influence the decisions of the shareholder activists to target the firm under his or her management. Alternatively, it could be suggested that the relationship is, in fact, opposite, i.e. facing activism events causes the CEO to demonstrate more narcissistic tendencies. Some might say that it is possible, but we argue that the alternative explanation is implausible. Studies in the literature of personality traits suggest that narcissism is a stable characteristic in an individual. As per Miller and Campbell (2010), attributes associated with a narcissistic individual are dispositional and unlikely to be swayed away by circumstances. In our empirical study on CEO narcissism, an overwhelming number of firms faced only one activism event (81 per cent approximately), suggesting it is unlikely that facing an activism event would cause a radical transformation in the CEO's personality traits.

The results associated with the main hypothesis of the study are based on probit regressions. One concern is that the results might be affected by not treating the data set as CEO ID-year panel or a FIRM ID-year panel. In order to assuage this concern, in an alternative specification, the probability of activism is modelled as the outcome of a *random effects probit regression* with *robust standard error clustered by Firm ID* ((i.e. global company key in Compustat) and then CEO ID (co_per_rol in Execucomp). The results (un-tabulated) from this alternative specification are very similar to the original results.

In order to compute a continuous measure of CEO narcissism we used the factor weightings from the factor analysis on three components: CEO picture prominence, relative cash pay, and relative non-cash pay. We checked whether our results changes when we use a simple average of the standardized values of the three components of CEO narcissism measure. We construct a new narcissism measure which is the mean of the standardized values of the three items. The new variable is labelled as NSEW. The corresponding dummy is NSEW80. We re

estimate the probability of an activism event in panel B and Panel C of Table 7 using NSEW and NSEW80 as the key explanatory variable. The new results are reported in Table 10; Probit regressions are based on all available fiscal year observations and three matched counterfactuals. The results based on NSEW and NSEW80 are very similar to results in Table 7.

The study also uses an alternative CEO narcissism measure based on the CEO's signature size, relative cash pay, and relative non-cash pay. According to Zweigenhaft and Marlowe (1973), larger signatures are associated with a greater level of self-esteem in individuals. Zweigenhaft (1977) and Jorgenson (1977) show that this can be used to measure ego and dominance, and these concepts are closely connected to narcissism. Signature size has been popularly used to measure ego. According to Rudman et al. (2007), one nice feature of a signature-size approach is that it is not necessary to ask subjects direct questions regarding narcissism and they do not consciously think that an important thing like ego could be related to something as trivial as the size of a signature. The study by Ham et al. (2018) uses CEO signature size as a narcissism measure.

The first step is collecting CEO signatures from the annual reports of the companies in the sample. It is a very time-consuming process to go to a company's investor relation website to download reports. Moreover, some of the annual reports do not contain CEO signatures; those firms will be excluded from the sample. The next step is reading CEO signatures to calculate the narcissism scores for each CEO. Firms for which CEO signatures are illegible is dropped from the sample. So, after finding the annual reports, the size of the signatures are measured; the study uses a software (ImajeJ) for this purpose. A rectangle is drawn around the signature, which touches the extreme points of the signature. The area of the rectangle is calculated by using the software. We take the log value of the signature size before computing the median of the log value. The correlation coefficient between picture and signature is positive at statistically significant (with a p-value < 0.01) indicating that they are associated with the same construct.

We calculated the standardized signature size, relative cash pay, and relative noncash pay for 741 CEOs.[14] It is found that 98 CEOs out of these 741 CEOs were targeted by activists. We calculate the narcissism scores using the factor loading weights as well using equal weights for three elements. We present the more conservative results in the appendix, i.e. the results based the on the narcissism score calculated using equal weights. The new measure is labelled as NSsig. The new CEO narcissism indicator is labelled as NSsig80. We compare the mean NSsig values associated with the target and non-target CEOs. The mean NSsig associated with the target CEOs (0.119) is significantly higher than the mean value for non-target CEOs (-0.043); the difference is significant at 1 per cent level.[15] The difference remains to be statistically significant when the target CEO mean value is compared with the value associated with an industry, SIZE, and MB matched counterfactual (two sample t test p-value <0.01).

The study re-estimates the primary regression model (Equation 2) in the analysis using the new CEO narcissism measure. We estimate the probability of an activism event using the same set of explanatory variables with all the firm-year observations for which the value for the dependent and independent variables of interest are available. We also run probit regressions with three different sets of peer firms (discussed in earlier sections), where the observations are associated with year t-1 (if year t is the event year). The probit regression results are presented in Table 11. The results are similar to our main results and support the hypothesis that narcissistic CEOs are more likely to be targeted by shareholder activists. We find that our new narcissism measure is significantly (typically at 1 per cent level) and positively associated with the likelihood of activism events. In six out of eight specifications, the coefficients on the signature-based narcissism measure are statistically significant with a p-value < 0.01 (based on robust standard error clustered by gvkey). Regression results based on two other matching firm controls leads us to the same conclusion. The results provide more supports to our

[14] Signature information was not available for 37 CEOs in our original sample.

[15] The p-value is based on a two-sample t test assuming unequal variances.

hypothesis that CEO narcissism is positively associated with the probability that shareholder activists will target the firm.

Insert Table 10 & 11 from Appendix I here

8. Experimental Evidence on Narcissism and Punishment

8.1 Experimental set-up and motivation

In order to support the main results of our study, we use experimental data collected by running sessions at a renowned university based in the UK. The CEO narcissism measure used in this study is an indirect measure of narcissism. Although the measure was validated in prior research work such as Chatterjee and Hambrick (2007), it does not have the strength of more direct measures such as the Narcissistic Personality Inventory developed by Raskin and Terry (1988). The experimental study gives us the opportunity to verify whether a qualitatively similar effect of narcissism can be found on the principal-agent relationship. The experimental set-up also allows applying renowned personality tools such as the NPI-40 or NPI-10 to measure the level of narcissism in subjects directly. The discussion in the following sections can be thought of as a robustness check but using a different data set generated by a different methodology.

The experimental study is used to support our main empirical finding that narcissistic CEOs are more likely to be targeted by activist shareholders. The experiment is about a three-person micro-organization or group with a principal-agent set-up. In every group, there is an agent and two principals. The agent makes decisions on behalf of the group. The decision involves choosing between a risky and safe option. The experiment lasts for 24 periods and the leader (i.e. the agent) representing a group of three individuals, including himself/herself, makes decisions in every period. The experimental sessions were conducted in the year 2017. Overall, 285 subjects were recruited at the CRIBS laboratory of the University of Nottingham, using ORSEE (Greiner, 2015). We ran 16 sessions in order to collect the data. The experiment lasted approximately 55 minutes. Participants earned £10.20 on average.

There are four types of treatments in our experiment *(i)* two treatments where the leader can be punished *(ii)* two treatments where the leader can be rewarded *(iii)* two treatments where the leader can be punished and rewarded *(iv)* two treatments where the leader can neither be punished nor rewarded. Our analysis is based on type *(i)* and type *(iii),* i.e. treatments where the leader can be punished. One of the principals in a group (involving reward or punishment) plays a more active role and can affect the payoff of the decision-maker (also called the leader) in our micro-organization.[16] We replicated a principal-agent set-up where the agent (the leader) represents the interests of the principal (two members in the group). The principal taking the responsibility of evaluating the leader is called the active member.

In four out of the eight treatments, the leader and other members of the group initially receive a variable amount; three of these four treatments allow the active member to reward or punish the leader. The initial payoff is determined on the basis of the leader's decision.[17] In four other treatments, the leader receives a flat fee at the beginning in every period, but the members receive a variable amount; three of these four treatments allow the active member to reward or punish the leader. The payoff of the leader in six treatments involving rewards or/and punishment can be different from the initial payoff (flat or variable) depending on the active principal's action. The leader receives a prespecified amount if rewarded and loses a prespecified amount if punished. Rewards, as well as punishments, are costly, and the cost is borne equally by the active and passive members of the group. Both principals incur a pre-determined cost if the leader is punished or rewarded. It can be noticed that the treatment variables are the nature of the contract that determines subjects' payoff and the role of other members in a group.

The description of the game is given below:

[16] The member assumes the role of the monitor and the leader is accountable for his decisions.

[17] The end payoff of the leader and the members of the group could be different from the initial variable payoff depending on the reward punishment decision taken by the active member.

Stage 1: Leader makes a decision (choosing between a risky and a safer option) and other members of the group indicate their preferences regarding the leader's choice.

Stage 2: The result is shown to the leader and the leader is asked for his/her level of happiness regarding the decision.

Stage 3: Leader's decision is shown to the members of the group (not the result yet) and the members are asked to indicate their level of happiness (scale: 0 to 9) regarding the leader's choice.

Stage 4: The result is shown to members.

The payoff function of the participants varies with treatments. In some treatments (six treatments), active members can punish or/and reward the leader at this stage.

Stage 5: Active members can punish or/and reward the leader at this stage. Punishment as well as reward are costly and the costs are borne equally by the principals.

Stage 6: Payoffs are shown to everyone and participants proceed to the next period.

In four out of eight treatments in the experiment, one member of the group (who plays an active role) can punish the leader after the outcome of the leader's decision is known. In our experimental set-up, punishment is a hypothetical parallel to an activism event. Our discussion is focused on four treatments involving punishment. The main interest here is the behaviour of the active member, especially the way she makes punishment decisions. The rest of the treatments are excluded from the discussions. In two of the four treatments that we exclude, leaders are not accountable for their choices, and in two other treatments, the accountability tool is a reward rather than punishment. We suggest that punishment, unlike reward, imposed by the active member of the group, is like an intervention or initiation of an activism event and hence relevant to our analysis.

In the experiment, we are essentially working with a three-person organization where the leader plays a role that bears a resemblance to the role a CEO plays. The inclusion of an active member is motivated by the presence of activists' shareholders in markets and the influence they have been exerting in recent years. Active members in this set-up are assuming the role of monitors, i.e. they have the responsibility to rein in the actions of the leader. In line with our primary hypothesis that narcissistic CEOs are more likely to be targeted by activists, we conjecture that narcissistic leaders are more likely to be punished by the active member. Similar to the initiation of an activism event, imposing punishment in our experimental set-up is a form of intervention. Moreover, like real-world activism events, punishment in our set-up is costly. The active member bears part of the cost.

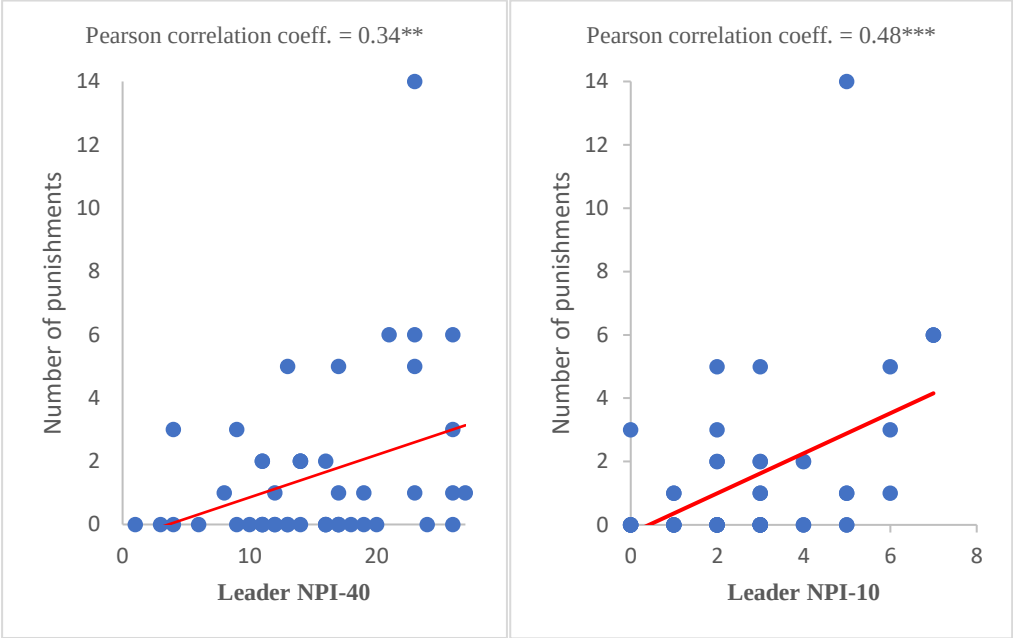
8.2 Experimental results

The study explores the punishment behaviour of the active member to find any connection between the leader's level of narcissism and the propensity of receiving punishments. We use NPI-40 and the NPI-10 as the narcissism measures to verify the link between punishment and narcissism. We predict that narcissistic leaders are more likely to be punished by the active member of the group. There are 48 groups (and hence 48 leaders) leaders in treatments involving punishment, so we calculated the total number of punishments received and narcissism score for each leader.[18] The mean (median) total number of punishments is 1.56 (0.00). As expected, we find a positive and significant association between the agent's level of narcissism and the number of times he was punished during the experiment. The phenomenon is graphically presented in Figure 2. The Pearson correlation coefficient between the number of punishments received and narcissism score is calculated using NPI-40 as well as NPI-10 as the narcissism measures. In both instances, the rho is significant and positive, suggesting that highly narcissistic leaders are more likely to be punished by the active member of their group. The

[18] The total number of punishments is the number of times the active member punishes the leader in her group during the experiment.

Pearson correlation coefficient between NPI-10 score of the leader and the number of times he was punished during the experiment is 0.48 (p-value < 0.001). Similarly, the rho is 0.34 (p-value < 0.05) when the comparison is based on NPI-40 score of the leader. The Pearson correlation results also indicate a strong and positive association between leaders' narcissism level and punishment propensity.[19]

Figure 2: Pearson correlation coefficient between leaders' narcissism score and the number of punishments received by the leader. *** significant at 1%; ** significant at 5%; * significant at 10%. The number of subjects is 48 in both graphs. The number of dots in the graph on right side is smaller as we have the same pair-wise value for more observations.



Panel C of Table 12 presents the relevant experimental results. We notice that narcissistic leaders are more likely to be punished by the active member. We compared the NPI-40 value of the leaders with the original sample of 384 subjects. There were 48 groups of three members, and hence 48 leaders participated in four treatments we analysed.[20] Ten out of these 48 leaders belong to the top 20 per cent of the subject pool as per their NPI-40 score. It is reasonable to assume that these ten leaders are highly narcissistic. We found that eight out of the ten highly

[19] The corresponding Spearman rho(s) are very similar and indicates a positive association between narcissism and punishment (untabulated results).

[20] Treatments where an active member can punish the leader of the group.

narcissistic leaders were punished at least once by the active member of the group during the experiment. In contrast, only fifteen out of 38 leaders in the less-narcissistic leader group were punished at least once by the active. The proportion of leaders punished at least once in the highly narcissistic group of leaders (0.80) is statistically significantly different from the proportion (0.39) for the less-narcissistic group (two-tailed p-value < 0.05 based on a test of proportions). Moreover, the mean number of punishments associated with ten leaders in the high-narcissism group is 3.70, which again significantly differs from the mean associated with the less-narcissistic-leader group (two-tailed p-value < 0.05; based on a t-test assuming unequal variance). We observe the same pattern and same level of significance in the mean punishment number difference when we use the top 33.33 per cent and bottom 66.66 per cent classifications (Panel C of Table 12). The mean number of times a narcissistic leader faced punishment is statistically different and higher than the mean value associated with their less-narcissistic counterparts.

It is also observed that narcissistic leaders receive punishment sooner. As discussed before, the experiment lasts for 24 periods. Table 12 shows that a narcissistic leader survives 6.44 periods on average before he/she receives the first punishment. In contrast, a non-narcissistic leader typically survives 9.93 periods before punishment is imposed for the first time.

Insert Table 12 from Appendix I here

As a robustness check, we used the ten-item narcissism measure, i.e. the NPI-10 to explore the link between narcissism and punishment. The results are reported in Table 12 (last two segments). We followed the same process and found very similar results. The proportion of leaders punished at least once in the highly narcissistic group (based on NPI-10) of leaders is 1.00, which is significantly different from the proportion for the less-narcissistic group (two-sided p-value < 0.01 based on a test of proportions). The mean number of punishments associated with ten leaders in the high-narcissism group is 4.50, which is significantly different from the mean associated with the less-narcissistic-leader group (two-sided p-value < 0.05 based on a t-test assuming unequal variance). We also found

that narcissistic leaders are punished earlier. We observe the same pattern and same level of significance in the mean punishment number difference when we use the top 33.33 per cent and bottom 66.66 per cent classifications using the NPI-10.

Since the results presented in Table 12 do not take into account the panel data structure of our experimental data, we conduct robustness check related to the main result that narcissistic leaders are more likely to be punished by the active members. We run random-effects probit regressions where the dependent variable is binary taking the value 1 if the leader is punished in a period and 0 otherwise. The results are reported in Table 13. There were 48 leaders in four treatments related to this analysis (treatments involving punishment). Each leader makes 24 decisions which give us a total of 1152 observations.[21] The experimental dataset is treated as a panel dataset where panel variables are *unique subjects ID* and *period*. The key explanatory variable is either leader's narcissism score based on the NPI-40 or NPI-10 or a dummy that indicates whether the leader belongs to the high narcissism group or low narcissism group. The analysis includes dummy variables associated with different treatments and some leader specific demographic and personality variables as controls.

Given that we are only interested in punishment decisions, our analysis is limited to observations associated four treatments where the active member in the group can punish the leader. The level of significance (p-values) is based on robust standard errors clustered by *unique subjects ID*. As can be seen in Table 13, narcissistic leaders are more likely to be punished by active members of the group. For each model, we report probit coefficients and their p-values based on robust standard error. In Model 1 to 4, the leader's narcissism level is captured by an indicator variable. The coefficients associated with indicator variables are positive and statistically significant (p-values are typically less than 0.001), suggesting that narcissistic leaders are more likely to be punished by the active member. In specification 5 and 6, leaders' narcissism scores are based on the NPI-40 and the

[21] Every period, the leader makes a decision, and the decision is subsequently evaluated by the active member of the group (assuming the role of a monitor). An active member can punish the leader after knowing the outcome of the leader's decision.

NPI-10, respectively. The results are consistent with the previous observation that leaders with a high level of narcissism are more likely to face punishment; the p-value associated with NPI-40 is less than 0.05, and the p-value associated with NPI-10 is less than 0.001.

Insert Table 13 from Appendix I here

9. Conclusion

We study CEO narcissism in the context of a compelling institutional force in the form of shareholder activism. The study investigates how investors view CEO narcissism and how their perception of this personality trait shapes their actions. Prior studies suggest that CEOs with high levels of narcissism are more likely to be associated with unethical behaviour in order to accomplish their goals and fulfil their relentless need for appreciation and esteem. A high level of narcissism may help an individual to reach the top of the corporate ladder (i.e. become a CEO) and accomplish great things for their firm. However, if not controlled, a very high dose of narcissism can be detrimental to the interest of the organization. By documenting a positive association between CEOs' narcissism and the propensity of being targeted by shareholder activists, we endorse the notion that narcissism is perceived as a negative individual difference by outside shareholders.

The managerial narcissism literature is still in its infancy, and only so few studies shed light on CEO narcissism concerning various financial variables and outcomes. The results of our study have implications for academics and investors. Our study is distinct from prior studies on CEO narcissism in two crucial aspects. First, we extend research on executive personalities by providing evidence on how CEOs' narcissistic tendencies are viewed by a very influential group, i.e. activists, who play vital roles behind many important corporate outcomes. Studies such as Chatterjee and Hambrick (2007), Olsen et al. (2013), Aktas et al. (2016) and Ham et al. (2018) are some examples where the focus was on how CEO narcissism affects different firm-level variables. No study to our knowledge has directly attempted to observe whether market forces are consciously putting any efforts into

controlling actions of highly narcissistic CEOs. Our contribution is significant as we focus on arguably one of the most influential investor groups (Goranova and Ryan, 2013), activists. While prior studies have documented significant effects of executive personalities on various corporate outcomes, the link between the investor-manager relationship and CEOs' personality traits has been an unexplored avenue. Second, unlike most studies on CEO narcissism, our study used experimental data to complement the main results. Using the experimental data, we show that there is a qualitative similarity of the influence of narcissism in agency situations. In our experimental set-up, narcissistic agents received more punishment, just like narcissistic CEOs who faced activism events more frequently than less narcissistic CEOs. We also show that institutional blockholders are less likely to be associated with firms with narcissistic CEOs at the helm. While we do not find any connection between CEO narcissism and short-term market reaction around activism news, we observe that post activism performance improvement is significantly (marginally) lower for firms with narcissistic CEOs, compared to firms with non-narcissistic CEOs.

Our study is subject to certain limitations. We recognize that our findings support statistically significant associations between CEO narcissism and shareholder activism due to the archival nature of our dataset. Moreover, we use an unobtrusive measure of CEO narcissism, which although supported by past studies, does not have the depth and accuracy that validated psychological tools, for instance, the Narcissistic Personality Inventory (Raskin and Terry, 1988), can deliver. One of the items included to the narcissism proxy, CEO picture, is likely to be influenced by factors such as the CEO's physical attributes, past life experience, and education. In order to alleviate this concern, the study controls for CEO gender and CEO age in the primary multivariate analysis, and then it is assumed that the rest of the sources of variation follow a random distribution. We also employ an alternative measure of CEO narcissism and arrive at the same results. The other measure uses CEOs' signature size instead of picture prominence rankings in the three-item proxy of CEO narcissism. Moreover, we use experimental evidence to support our primary findings. In a laboratory setting, we

observe that narcissistic leaders are more likely to be punished by their group members. The study is restricted to the most recent period (i.e. 2006-2015) due to difficulties associated with hand collection of data on CEOs' picture prominence in the annual reports. The investor relation websites of a significant portion of the S&P 1500 companies do not have annual reports for fiscal years beyond the most recent years. It is not clear whether CEO narcissism would have the same impact on activists' decision to target a firm in the pre-financial crisis era. Many studies on investor activism, however, suggests that investor activism was much less prominent before the recent financial crisis.

Narcissistic individuals are self-focused and driven by a heightened need for high esteem and recognition. It is likely that narcissistic CEOs regularly engage in bold and highly controversial activities in order to get frequent praise and admiration. This pursuit may put them under the spotlight (which is what they want), but it comes at a price. Apparently, actions fuelled by narcissistic tendencies increase the chance of becoming a target of activists. The findings lead to the following conclusions. First, it can be suggested that market forces, such as shareholder activists do pay attention to CEOs' personality traits before making their moves. Secondly, it seems that CEO narcissism is generally seen as a negative trait by the investors, as shown by the positive link between CEO narcissism and the possibility of activism events. Finally, the results highlight the importance of incorporating the CEO's personality traits while studying various issues associated with the investor-management relationship. We also argue that since the objective of passive shareholder (most of the large shareholders are pursuing a passive strategy) is different from activists, they are likely to behave differently. Our findings may assist boards of directors to understand that the presence of a narcissistic CEO can attract attention from activists, which can destabilize the environment of the company. Board of directors can make use of our results to identify CEOs with a high level of narcissism and can avoid hiring them in the first place. If a narcissistic CEO is already at the helm of the firm, investors should implement heightened monitoring over his potentially harmful activities.

Future studies on activism can look into the behavioural characteristics of key personalities associated with activist entities. While our study mainly explores a psychological trait of the CEO, i.e. the agent in the set-up, behavioural characteristics of the activists may have a significant impact on the outcome of activism events. Activists' Behavioural biases and personality traits can significantly influence the negotiations process and eventually, the outcome of the event. More researches are required in order to reveal the magnitude and nature of such connections. In order to conduct empirical research focusing on principals' psychological characteristics, data on key figures in activists' entities, such as hedge fund manager or CEO, are required. Those people at the top of the activist organization profoundly influence the philosophical stand of an activist entity. Hence, valuable insights can be gained by studying the psychological aspects of key personalities (e.g. fund managers) behind activism events. Another potential research route is to examine the link between managerial personality traits and blockholder heterogeneity. Our study shows a significant link between institutional blockholding and CEO narcissism, but we still do not know how other blockholder groups (non-institutional) view CEO psychology. Future studies can attempt to fill this research gap.

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10. Appendix I

Appendix 1.1: Variable Descriptions

Variables	Descriptions
NS	Narcissism score of a CEO, calculated as the weighted sum of the standardized values of a CEO's picture prominence in the annual report, the CEO's relative cash pay, and the CEO's relative non-cash pay. The weight is from a factor analysis where all the three indicators are loaded on a single factor.
NS80	A dummy variable that takes a value of 1 if a CEO's narcissistic score *NS* is in the 80th percentile and 0 otherwise.
NSEW	An alternative measure of CEO, calculated as the mean of the standardized values of a CEO's picture prominence in the annual report, the CEO's relative cash pay, and the CEO's relative non-cash pay. The weight the same for each component. from a factor analysis where all the three indicators are loaded on a single factor.
NSEW80	A dummy variable that takes a value of 1 if a CEO's narcissistic score *NSEW* is in the 80th percentile and 0 otherwise.
NSsig	An alternative measure of CEO, calculated as the mean of the standardized values of a CEO's signature size (log value) in the annual report, the CEO's relative cash pay, and the CEO's relative non-cash pay. The weights are the same for each component. In order to measure CEO signature, we use ImageJ software. After measuring the size, we take the standardized value of the median of the log of the signature size.
NSsig80	A dummy variable that takes a value of 1 if a CEO's narcissistic score NSEW is in the 80th percentile and 0 otherwise.
NPI-40	The variable NPI-40 is the narcissism score of the subjects computed following the 40-item Narcissistic Personality Inventory or NPI-40, constructed and validated by Raskin and Terry (1988). NPI-40 values range from 0 to 40 (discrete values). The variable NPI-10 is consisting of ten questions from the NPI-40 that, according to Ames et al. (2006), are mostly associated with narcissism. The variable NPI-6 is based on six questions that are most closely linked to overconfidence and there acts as a proxy for **overconfidence** levels in subjects.
NPI-10	The variable NPI-10 consists of ten questions from the NPI-40 that, according to Ames et al. (2006), are mostly associated with narcissism. NPI-10 is a condensed version of the NPI-40, and the values can range between 0 and 10.
NPI-6	The variable NPI-6 is based on six questions from the NPI-40 that are most closely linked to overconfidence—the variable acts as a proxy for overconfidence levels in subjects.
Activism	A dummy variable that takes a value of 1 if the firm is targeted by at least one activist in a given year and 0 otherwise.

SIZE	Log of market capitalization., which is share price (PRCC_F) multiplied by number of shares outstanding (CSHO).
MB	We follow previous studies in the literature to calculate the MB ratio. Market to book ratio where market equity is the product of share price at the end of a fiscal and the number of shares outstanding. In order to compute *Book equity*, the sum of preferred stock and balance sheet deferred taxes and investment tax credit is deducted from stockholder's equity. Book value of equity calculated as total shareholders' equity (SEQ), plus deferred taxes and investment tax credit (TXDITC), minus the book value of preferred stock (PSTK), in millions of US dollars, as of fiscal year end (Compustat) Market Equity (BE) Market Capitalization calculated as share price (PRCC_F) multiplied by number of shares outstanding (CSHO), in millions of US dollars, as of fiscal year end (Compustat)
LEV	This ratio is obtained by dividing the sum of long-term debts (DLTT in Compustat) and short-term debt (DLC in Compustat) by lagged total assets (AT in Compustat) in the prior year.
RD	Research and development expense (XRD in Compustat) to assets ratio is obtained by dividing Research and development expense by total assets in the prior year. If data item XRD is missing, it is set to zero.
ROA	Ratio of earnings before interest expense, taxes, depreciation and amortization (EBITDA) to lagged assets (AT) (Compustat)
RET	Annual fiscal year stock return calculated as [PRCC_F(t)/ AJEX(t) + DVPSX_F (t)/AJEX(t)]/[PRCC_F(t -1)/AJEX_F(t - 1)] – 1 (Compustat)
INST	Total Institutional ownership as a percent of Shares Outstanding in the prior year.
BLOCK	Number of institutional blockholders with stakes greater than five percent in the prior year.
CONFIDENCE	A dummy variable that takes a value of 1 if *Confidence* measure is equal to or greater than 67% in at least two years, and 0 otherwise, where: $$Confidence = \frac{Average\ Value\ Per\ Vested\ Option}{Average\ Strike\ Price}$$ $$Average\ Value\ Per\ Vested\ Option = \frac{Value\ of\ vested\ unexcercised\ options}{Number\ of\ vested\ unexcercised\ options}$$ $$Average\ Strike\ Price = Stock\ price - Average\ Value\ Per\ Vested\ Option$$
CHAIR	A dummy variable that takes a value of 1 if the CEO is also the chairman of the board in a given year and 0 otherwise. This is a lagged variable.
AGE	Age of a CEO.
MALE	A dummy variable that takes a value of 1 if the CEO's gender is male and 0 otherwise.

HHI	The Herfindahl-Hirschman Index value in the prior year, which is used as a measure of market concentration. The variable indicates how big the company is with respect to the industry.
INSTBOWN	Log value of shares owned by institutional blockholders of the firm in the prior year. A blockholder is a shareholder that holds more than five percent of a company's shares.
BOARD	Number of directors in the board of a firm in the prior year. The data is collected from BOARDEX.
LRG5OWN	Log value of shares owned by five largest shareholders of the firm in the prior year.

Table 1

Panel A: Panel reports summery statistics related to a survey associated with some experimental sessions. The variable Picture rating in Panel A is based on responses from participants who completed a survey at the end of experimental sessions conducted at the University of Nottingham. In a survey-question, a subject was asked to choose one of the five pictures with varied level of prominence, which is coded as 1 to 5 (where 5 is the highest prominence rating). The variable NPI-40 is the narcissism score of the subjects computed following the 40-item Narcissistic Personality Inventory or NPI-40 (Raskin and Terry, 1988). Values for NPI-40 range from 0 to 40 (integer values). The variable NPI-10 is an abridged version of NPI-40 and is consisting of ten questions from the NPI-40 that, according to Ham et al. (2018), are mostly associated with narcissism. The variable NPI-6 is based on six questions that are most closely linked to overconfidence and there acts as a proxy for overconfidence levels in subjects. **Panel B**: The Pearson correlation coefficients among variables described in Panel A are presented (p-value is in the parenthesis).

Panel A: Picture rating and Narcissism level from the experiments

Variables	N	Mean	Median	SD	Min	Max
Picture rating	384	3.187	4	1.278	1	5
NPI-40	384	15.179	15.5	6.227	0	34
NPI-10	384	3.086	3	2.020	0	8
NPI-6	384	2.174	2	1.534	0	6

Panel B: Pearson Correlation coefficient; N= 384

	Picture rating
NPI-40	0.102
	(0.046)
NPI-10	0.101
	(0.048)
NPI-6	0.043
	(0.399)

Table 2

Panel A: Panel A of the table reports summary statistics of the components of the narcissism measures used in the analysis. Results are based on median values (for each CEO) of the components. In CEO Relative cash pay, cash pay is the sum of salary (SALARY in ExecuComp) and bonus (BONUS in ExecuComp). In CEO Relative non-cash pay, non-cash pay is calculated by deducting the sum of salary and bonus from the total compensation (TDC1 in ExecuComp). CEO Picture is based on CEOs' picture prominence in annual reports (coded as 1,2,3,4, and 5). CEO's relative cash pay is defined as the ratio of the CEO's salary and bonus to that of the second highest (highest if the CEO is not the highest-paid executive) paid executive at the CEO's firm. CEO Relative non-cash pay is the ratio of the CEO's total compensation less cash compensation to that of the second highest (highest if the CEO is not the highest-paid executive) paid executive at the CEO's firm.

Panel B: Panel B of the table reports the Pearson correlation coefficients of the components of the narcissism measures used in the analysis: NS, NS80, and CONFIDENCE (see section A1 in the appendix for variable descriptions). Here NS is the continuous measure narcissism in CEOs and NS80 is a binary measure which takes the 1 if the CEO belongs to the top 20 per cent of the CEOs according to the narcissism scale. The variable CONFIDENCE is the option-based measure of CEO overconfidence. Results are based on median values (for each CEO) of the components and the variables. *, **, *** Denote statistical significance at the 10 per cent, 5 per cent, and 1 per cent levels, respectively.

Panel C: The panel presents statistics on CEOs' narcissism score denoted as NS and NSEW. NS as well as NSEW are continuous variables and are calculated based on information on CEOs' picture prominence on the annual report, relative cash pay and relative non-cash pay. NS is calculated by incorporating the factor weightings from the principal-components analysis of three narcissism measure components. The variable NSEW, on the other hand, is calculated based on equal weights of the standardized values. Panel B presents the comparison between CEO narcissism scores of target and non-target firms in the sample. A comparison between the target firm CEO sample and a non-target matching firm CEO sample is also presented. The non-target matching firms are selected based on industry, MB, and SIZE. NS80 and NSEW80 are dummy variables (see section A1 in the appendix for variable descriptions). ***, **, and * represent the level of significance (based on two-tailed p-values from a two-sample t-test assuming unequal variances) at the 1%, 5%, and 10% level, respectively associated with mean differences.

Panel D: Panel reports the frequency associated with the annual reports collected from company websites to construct the CEO narcissism proxy. The study collects annual reports (non 10-k format) from company websites.

Panel A

	Mean	Median	SD	Min	P20	P40	P60	P80	Max	N
CEO Picture	2.846	3.500	1.389	1	1	2	4	4	5	778
CEO Relative cash pay	1.610	1.617	0.411	0.579	1.267	1.530	1.597	1.900	2.900	778
CEO Relative non-cash pay	2.278	2.251	1.009	0.035	1.465	1.982	2.416	2.967	6.072	778

Panel B: Pearson correlation coefficient with significance; N =778

	CEO Picture	CEO Relative cash pay	CEO Relative non-cash pay	NS	NS80
CEO Relative cash pay	0.160***				
CEO Relative non-cash pay	0.219***	0.566***			
CONFIDENCE	-0.043	0.072**	0.037	0.031	0.004

Panel C

	Target CEOs				**Non-Target CEOs**				**Non-Target Matched (Industry, SIZE, and MB)CEOs**			
	N	Mean	Median	SD	N	Mean	Median	SD	N	Mean	Median	SD
NS	102	0.158	0.129	0.953	676	-0.073**	-0.056	0.856	114	-0.120**	-0.086	0.808
NSEW	102	0.094	0.111	0.584	676	-0.039**	-0.012	0.534	114	-0.060**	-0.034	0.506
NS80	102	0.294	0.000	0.458	676	0.185**	0.000	0.389	114	0.158**	0.000	0.366
NS80EW	102	0.284	0.000	0.453	676	0.186**	0.000	0.390	114	0.158**	0.000	0.366

Panel D

Number of annual reports	Number of CEO	Percentage
1	111	14.27
2	87	11.18
3	129	16.58
4	114	14.65
5	87	11.18
6	76	9.77
7 or more	174	22.37
Total	778	

Table 3

The table presents the descriptive statistics associated with the key variables in this study. Variable definitions can be found in Appendix 1.1. Table reports mean median, and standard deviation for each variable. All the continuous variables are winsorized at 1% and 99% level.

Variable Name	N	Mean	Median	SD
Activism	4766	0.024	0.000	0.153
NS	778	-0.026	-0.043	0.872
NS80	778	0.199	0.000	0.400
NSEW	778	-0.022	-0.001	0.542
NSEW80	778	0.199	0.000	0.400
NSsig	741	-0.024	0.001	0.498
NSsig80	741	0.200	0.000	0.400
SIZE	4766	7.932	7.738	1.518
MB	4766	3.058	2.265	2.878
LEV	4766	0.199	0.187	0.165
ROA	4766	0.172	0.157	0.096
RET	4766	0.162	0.133	0.402
RD	4766	0.026	0.000	0.043
AGE	4232	56.016	56.000	6.686
CHAIR	4232	0.535	1.000	0.499
CONFIDENCE	4232	0.539	1.000	0.499
MALE	4232	0.976	1.000	0.152
INST	4232	0.786	0.802	0.164
BLOCK	4232	2.668	3.000	1.607
HHI	4216	0.047	0.041	0.025
LRG5OWN	4216	17.033	16.833	1.168
INSTBOWN	3929	16.508	16.405	1.059

Table 4

Panel A of the table shows the event and campaign frequency by year. Panel B tabulates events by type of activist. Events are separated by hedge fund, non-hedge fund institutional activists, corporations, and others. A hedge fund is defined according to company information collected from Bloomberg company profile and through LexisNexis search. Panel B reports Event frequency by activists and by targets. Panel D reports activists' demands. We follow Greenwood and Schore (2009) to define the campaign objective. A campaign is defined as value-seeking if activists' demand includes Seek alternatives and/or Strategic direction; *Board Control* if demands include Boar Representation, Board Control, and Shareholder rights; Restructuring if demands include spinoff, force sale, hostile acquisitions. An activist can have multiple demands in the same event. Panel E of the table shows frequencies associated with different types of campaign outcomes

Panel A: Number of Activism C*ampaigns/Events by Year*

	Events	Percentage	Campaigns	Percentage
2006	2	1.65%	5	3.62%
2007	1	0.83%	1	0.72%
2008	7	5.79%	9	6.52%
2009	6	4.96%	14	10.14%
2010	10	8.26%	10	7.25%
2011	14	11.57%	14	10.14%
2012	17	14.05%	17	12.32%
2013	18	14.88%	19	13.77%
2014	23	19.01%	24	17.39%
2015	23	19.01%	25	18.12%
Total	**121**	**100.00%**	**138**	**100.00%**

Panel B: Number of Activism Events by Type of Activists

Types of activists	Number of events	Percentage	Number of activists	Percentage
Hedge fund	68	56.20%	33	48.53%
Non-hedge fund institutions	34	28.10%	19	27.94%
Corporations	11	9.09%	11	16.18%
Others	8	6.61%	5	7.35%
Total	**121**	**100%**	**68**	**100%**

Panel C: Event Frequency by Firms and by Activists

Frequency	Number of firms	Percentage	Frequency	Number of activists	Percentage
1 event	82	81.19%	1 event	48	70.59%
2 events	18	17.82%	2 events	8	11.76%
3 events	1	0.99%	3 events	1	1.47%
			4 events	7	10.29%
			5 or more events	4	5.88%
Total	**101**	**100.00%**	**Total**	**68**	**100.00%**

Panel D

Activist's demand	Number of events	Percentage
Value seeking	66	54.55%
Board control	90	74.38%
Restructuring	30	24.79%

Panel E

Campaign Outcome	Number of events	Percentage
Dissident Part. Victory	11	9.09%
Dissident Victory	17	14.05%
Management Victory	21	17.36%
Pending	15	12.4%
Settled	27	22.31%
Unresolved	15	12.4%
Withdrawn	15	12.4%
Total	121	100%

Table 5

The table reports descriptive statistics for the firm-specific and CEO specific variables employed in our primary multivariate analysis. The table reports values separately for firms that faced activism events and for firms that did not face any activism event during 2006-2015. First of all, we compare all the firm-year values associated with the target firms with the firm-year values of non-target firms in the full sample. Second, we compare the target firm years with firm-year observations associated with a set of peer firms. All continuous variables are winsorized at the 1 per cent and 99 per cent levels. All variables are as defined in Appendix 1.1. *, **, *** denote statistical significance (two-tailed) at the 10 per cent, 5 per cent, and 1 per cent levels. P-values are based on t-test with unequal variance assumption for corresponding mean differences and MWU test for median differences, indicating a statistically significant difference between the values associated with the underlying variable of firms that experienced activism events and firms that did not face activism campaign. Panel A reports values separately for firm years with target firm CEOs and matching firm CEOs. For each target firm-year, one matching firm is selected based on the following equation. We find a peer firm from the same industry group (based on Fama-French 30 industry classification) for which the value of the following equation is the closest to the value associated with the target firm (the value for the target firm is 0) in the prior year in relation to the activism-event year.

$$\sqrt{(SIZE\ scaled\ rank\ target - SIZE\ scaled\ rank\ peer)^2 + (MB\ scaled\ rank\ target - MB\ scaled\ rank\ peer)^2}$$

	Target Firms				**Non-Target Firms**				**Non-Target Matched Firms (Industry, SIZE, & MB)**			
	N	Mean	Median	SD	N	Mean	Median	SD	N	Mean	Median	SD
SIZE	678	8.131	8.067	1.534	4088	7.899***	7.701***	1.513	703	7.976*	7.921	1.462
MB	678	2.518	1.920	2.201	4088	3.148***	2.321***	2.966	703	2.490	2.064	1.913
LEV	678	0.216	0.205	0.153	4088	0.196***	0.183***	0.167	703	0.208	0.194	0.164
ROA	678	0.150	0.137	0.084	4088	0.176***	0.161***	0.097	703	0.170***	0.154***	0.096
RET	678	0.139	0.111	0.408	4088	0.165	0.135*	0.401	703	0.159	0.129	0.397
RD	678	0.027	0.000	0.047	4088	0.025	0.000	0.043	703	0.014***	0***	0.030
AGE	619	56.329	56	6.806	3613	55.963	56	6.665	613	55.667*	56	6.275
CHAIR	619	0.580	1	0.494	3613	0.528**	1**	0.499	613	0.544	1	0.498
CONFIDENCE	619	0.548	1	0.498	3613	0.538	1	0.499	613	0.484**	0**	0.500
MALE	619	0.956	1	0.204	3613	0.980**	1**	0.141	613	0.995***	1***	0.070
INST	619	0.806	0.821	0.156	3613	0.783***	0.798***	0.165	613	0.782***	0.789***	0.165
BLOCK	619	2.646	2	1.672	3613	2.672	3	1.595	613	2.529	2	1.558

Table 6

Table reports descriptive statistics for the key firm-specific variables employed in our tests. The table compares the values in the year before the activism year (i.e. year t-1 if activism year is t) for target firms and three sets of matching firms used as controls in our multivariate analysis. Some target firms (CEOs as well) faced events in multiple years, so for each yearly observation, we find a matching firm. *, **, *** denote statistical significance (two-tailed) at the 10 per cent, 5 per cent, and 1 per cent levels. The significance level is based on t-test with unequal variance assumption for corresponding mean differences.

Panel A reports descriptive statistics for the firm-specific variables employed in our tests. Panel A compares the values in the year prior to the activism year for target firms and matching firms. We find a peer firm from the same industry group (based on Fama-French 30 industry classification) for which the value of the following equation is the closest to the value associated with the target firm (the value for the target firm is 0) in the prior year in relation to the activism-event year.

$$\sqrt{(SIZE\ scaled\ rank\ target - SIZE\ scaled\ rank\ peer)^2 + (MB\ scaled\ rank\ target - MB\ scaled\ rank\ peer)^2}$$

Panel B Reports values separately for firm years with target firm CEOs and matching firm CEOs. For each target firm-year, one matching firm is selected based on the equation below. We find a peer firm from the same industry group (based on Fama-French 30 industry classification) for which the value of the following equation is the closest to zero (the value for the target firm is 0) in the prior-year concerning the activism-event year.

$$\sqrt{\begin{matrix}(SIZE\ scaled\ rank\ target - SIZE\ scaled\ rank\ peer)^2 + (MB\ scaled\ rank\ target - MB\ scaled\ rank\ peer)^2 \\ +(ROA\ scaled\ rank\ target - ROA\ scaled\ rank\ peer)^2\end{matrix}}$$

Panel C: Following Brav et al. (2008) and Barber and Lyon (1996), we formed another matching firm portfolio. For each target company fiscal year we select a company from the same two-digit industry (SIC from the Compustat) whose ROA in the year t-2 (relative to the event year where event year is t) is not beyond 90% or 110% of the target firm. If we do not get a firm in the same two-digit industry, the assumption is relaxed and find a firm from the one-digit industry. If we still do not get a firm with the 10*10 condition, we exclude that firm (event year) from the analysis. There were 9 cases like this. So, there are 105 matching firms (to be matched with 105 firm years where an activism event took place) in the 10*10 portfolio.

	Target Firms (year t-1 values in relation to the activism year)				Matching Firms (year t-1 values in relation to the activism year)			
	N	Mean	SD	Median	N	Mean	SD	Median
Panel A	MB, Size, & Industry matched portfolio							
NS	102	0 .158	0.953	0.129	114	-0.120**	0.808	-0.086
ROA	114	0.138	0.077	0.129	114	0.153	0.086	0.141
MB	114	2.486	2.259	1.890	114	2.344	1.566	1.979
LEV	114	0.232	0.149	0.224	114	0.205	0.167	0.200
SIZE	114	8.239	1.704	8.203	114	8.067	1.486	7.901
RET	114	0.067	0.326	0.044	114	**0.205*** **	0.402	0.175
RD	114	0.024	0.044	0.000	114	0.016	0.035	0.000

	Target Firms (year t-1 values in relation to the activism year)				Matching Firms (year t-1 values in relation to the activism year)			
	N	Mean	SD	Median	N	Mean	SD	Median
Panel B				MB, Size, ROA matched portfolio				
NS	102	0 .158	0.953	0.129	114	**-0.110****	0.799	-0.091
ROA	114	0.138	0.077	0.129	114	0.143	0.056	0.144
MB	114	2.486	2.259	1.890	114	2.477	1.747	2.113
LEV	114	0.232	0.149	0.224	114	0.213	0.159	0.209
SIZE	114	8.239	1.704	8.203	114	8.087	1.498	7.884
RET	114	0.067	0.326	0.044	114	**0.162****	0.383	0.176
RD	114	0.024	0.044	0.000	114	0.018	0.034	0.000
Panel C				10*10 matched portfolio				
NS	96	0.179	0.940	0.144	105	**-0.243*****	0.714	-0.218
ROA	105	0.141	0.064	0.131	105	**0.157***	0.071	0.147
MB	105	2.570	2.329	1.989	105	2.638	2.206	1.988
LEV	105	0.230	0.143	0.222	105	0.209	0.170	0.193
SIZE	105	8.286	1.713	8.229	105	8.147	1.629	7.837
RET	105	0.082	0.325	0.049	105	0.131	0.314	0.106
RD	105	0.022	0.039	0.000	105	0.026	0.043	0.000

Table 7

In Table 6 the probability of an activism event is modelled as the outcome of a probit regression. The analysis involves activism events associated with sample CEOs for which we could determine narcissism scores during the period 2006-2015.

In **Panel A,** the probability of activism events is modelled as the outcome of a probit regression. The coefficients associated with the explanatory variables are reported along with their p-values. In the regression, we have one observation for each CEO. The dependent variable is set to 1 if the CEO is targeted by an activist during the period 2006-2015 and 0 otherwise. The explanatory variables are CEOs' level of narcissism and another time-invariant variable in the form of MALE, which has value 1 if the CEO is a male and 0 if the CEO is a female. In specification 1 to 4, CEO narcissism scores (as captured by the variable NS) are **continuous** values. In Model 5 to 8, CEO narcissism score is an indicator variable (NS80). The variable NS80 takes the value 1 if the CEO is among the top 20% of the sample in terms of narcissism score and 0 otherwise. In specification 1 and 5, the number of CEOs who received at least 1 activism event is 102, and the number of CEOs who did not receive any activism event is 676. In the rest of the specifications, observations include CEOs from samples of matching firms as controls. In specification 2 and 6, the regression uses a sample of matching firms based on industry (Fama-French 30 industry classification), SIZE, and MB. The method we followed in selecting a peer firm for each fiscal year, where an activism event took place is discussed in one of the previous sections. The number of fiscal years where at least one activism event occurred is 114, and therefore, we selected 114 firms (and 114 CEOs) to construct a set of peer firms. Specification 3 and 7, are based on a SIZE, MB, ROA, and Industry matched counterfactual and Specification 4 and 8 are based on 10*10 portfolio matched firms. P-values, reflecting robust standard errors clustered by firm ID (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NS80 is statistically significant.

Panel B of Table 6 reports the effects of covariates on the probability of being targeted by activists. The dependent variable is set to 1 for fiscal years, where the CEO is targeted by an activist and 0 otherwise. For each model, we report probit coefficients and their p-values based on robust standard error clustered by firm identifiers (i.e. gvkey). In specification 1 to 4, CEO narcissism scores (as captured by the variable NS) are continuous values. In Model 5 to 8, CEO narcissism score is an indicator variable (NS80). P-values, reflecting robust standard errors clustered by *firm ID* (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

Variable definitions are listed in Appendix 1.1. Column 1,2,5, and 6 reports results based on all firm-year observations for which data on every covariate is available. *Year fixed effects* and *industry fixed effects* are included by using Fama-French 30 industry portfolio for these specifications. Column 3,4,7, and 8 are based on target firm observations and observations from a sample of matching firms in the year before the activism event. Since matching firms are selected from the same industry and years are also matched, industry fixed effects and year fixed effects are not included in these specifications. In Column 3,4,7, and 8, the regression uses a sample of matching firms based on industry (Fama-French 30), SIZE, and MB as the control. The method we followed in selecting a peer firm for each fiscal year, where an activism event took place is discussed in one of the previous sections. The number of fiscal years where at least one activism event occurred is 114, and therefore, we selected 114 firms (and 114 CEOs) to construct a set of peer firms. We also report the predicted probability of an activism event in the last two rows. The last row reports the predicted probability when NS80 = 1 and the second last row reports the predicted probability when NS80=0. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NS80 is statistically significant.

Panel C reports results based on two more sets of peer firms. The probability of an activism event is modelled as the outcome of a probit regression. In specification 1 to 4, CEO narcissism scores (as captured by the variable NS) are continuous values. In Model 5 to 8, CEO narcissism score is an indicator variable (NS80). Column 1,2,5, and 6 reports results that are based on 10*10 portfolio matched

control firms. Column 3,4,7, and 8 are based on an industry (Fama-French 30) SIZE, MB, ROA, matched counterfactual. P-values, reflecting robust standard errors clustered by *firm ID* (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively. Since matching firms are selected from the same industry and years are also matched, industry fixed effects and year fixed effects are not included in these specifications. We also report the predicted probability of an activism event in the last two rows. The last row reports the predicted probability when NS80 = 1 and the second last row reports the predicted probability when NS80=0. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NS80 is statistically significant.

Panel A

Prob. of activism	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
NS	0.159**	0.223**	0.219**	0.388***				
	(0.019)	(0.019)	(0.025)	(0.000)				
NS80					0.338**	0.513**	0.554***	1.032***
					(0.011)	(0.014)	(0.008)	(0.000)
MALE	-0.238	-0.618	-0.639	-1.055*	-0.269	-0.715	-0.718	-1.133*
	(0.390)	(0.236)	(0.212)	(0.085)	(0.331)	(0.160)	(0.157)	(0.070)
CONST	-0.897***	0.526	0.545	0.990	-0.940***	0.510	0.506	0.869
	(0.001)	(0.303)	(0.279)	(0.102)	(0.001)	(0.310)	(0.313)	(0.160)
N	778	216	216	201	778	216	216	201
Pseudo R^2	0.011	0.023	0.022	0.058	0.012	0.026	0.029	0.077
Industry & Year Fixed Effect	NO	NO	NO	NO	NO	NO	NO	NO
Predicted probability when NS80 =0					0.115	0.428	0.425	0.410
Predicted probability when NS80 =1					0.194**	0.630**	0.643***	0.789***

Panel B

Probability of activism	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
NS	0.111**	0.116**	0.292***	0.264**				
	(0.026)	(0.044)	(0.002)	(0.021)				
NS80					0.235**	0.276***	0.640***	0.771***
					(0.016)	(0.008)	(0.003)	(0.002)
SIZE	0.063**	0.099**	0.048	0.110	0.061**	0.100**	0.060	0.135*
	(0.037)	(0.010)	(0.427)	(0.135)	(0.044)	(0.010)	(0.320)	(0.078)
MB	-0.038*	-0.055*	0.036	0.008	-0.038*	-0.056*	0.039	0.026
	(0.083)	(0.071)	(0.429)	(0.880)	(0.090)	(0.081)	(0.396)	(0.665)
LEV	0.215	0.064	0.774	0.409	0.227	0.074	0.744	0.267
	(0.417)	(0.812)	(0.199)	(0.542)	(0.398)	(0.785)	(0.221)	(0.690)
ROA	-2.078***	-2.192***	-0.910	-1.132	-2.052***	-2.172***	-1.059	-1.438
	(0.001)	(0.000)	(0.414)	(0.376)	(0.001)	(0.000)	(0.330)	(0.265)
RET	-0.459***	-0.301**	-0.790***	-0.672***	-0.460***	-0.304**	-0.862***	-0.794***
	(0.001)	(0.029)	(0.001)	(0.010)	(0.001)	(0.027)	(0.000)	(0.004)
RD	0.538	0.460	3.500	3.957	0.466	0.462	2.899	3.000
	(0.652)	(0.724)	(0.182)	(0.193)	(0.695)	(0.720)	(0.265)	(0.320)
AGE		0.008		0.030**		0.008		0.0279*
		(0.265)		(0.049)		(0.263)		(0.067)
CHAIR		-0.022		-0.148		-0.035		-0.233
		(0.820)		(0.516)		(0.726)		(0.313)
CONFIDENCE		0.005		0.144		0.008		0.173
		(0.962)		(0.483)		(0.936)		(0.404)
MALE		-0.344		-1.041*		-0.375*		-1.203*
		(0.111)		(0.092)		(0.093)		(0.050)
INST		0.163		0.511		0.208		0.569
		(0.648)		(0.511)		(0.551)		(0.461)
BLOCK		0.022		0.062		0.027		0.080
		(0.594)		(0.436)		(0.517)		(0.320)
CONST	-2.541***	-2.958***	-0.489	-2.068	-2.584***	-3.036***	-0.677	-2.127
	(0.000)	(0.000)	(0.331)	(0.159)	(0.000)	(0.000)	(0.189)	(0.151)
N	4766	4232	228	198	4766	4232	228	198
Pseudo R^2	0.098	0.103	0.074	0.098	0.098	0.105	0.076	0.115
Industry & Year Fixed Effect	YES	YES	NO	NO	YES	YES	NO	NO
Predicted probability when NS80 =0					0.013	0.012	0.443	0.442
Predicted probability when NS80 =1					0.023**	0.024***	0.690***	0.734***

Panel C

Probability of activism	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
NS	0.402***	0.420***	0.286***	0.251**				
	(0.000)	(0.001)	(0.004)	(0.023)				
NS80					1.034***	1.149***	0.640***	0.688***
					(0.000)	(0.000)	(0.004)	(0.005)
SIZE	0.063	0.095	0.056	0.134*	0.069	0.108	0.065	0.147**
	(0.290)	(0.220)	(0.350)	(0.056)	(0.241)	(0.165)	(0.283)	(0.040)
MB	0.043	-0.015	-0.009	-0.055	0.037	-0.018	-0.008	-0.044
	(0.348)	(0.805)	(0.850)	(0.414)	(0.426)	(0.790)	(0.871)	(0.539)
LEV	-0.030	-0.017	0.804	0.663	0.096	0.176	0.764	0.596
	(0.966)	(0.981)	(0.216)	(0.337)	(0.892)	(0.809)	(0.246)	(0.391)
ROA	-2.527	-1.884	0.280	1.605	-2.339	-1.683	0.184	1.588
	(0.115)	(0.302)	(0.854)	(0.373)	(0.127)	(0.344)	(0.903)	(0.372)
RET	-0.402	-0.274	-0.570**	-0.292	-0.499*	-0.390	-0.636***	-0.384
	(0.167)	(0.392)	(0.017)	(0.279)	(0.095)	(0.241)	(0.009)	(0.167)
RD	-1.419	-0.050	4.341	4.714	-2.497	-1.318	3.643	3.995
	(0.583)	(0.985)	(0.111)	(0.115)	(0.338)	(0.634)	(0.170)	(0.179)
AGE		-0.006		0.028*		-0.007		0.026
		(0.713)		(0.083)		(0.665)		(0.104)
CHAIR		-0.256		-0.087		-0.317		-0.133
		(0.248)		(0.685)		(0.159)		(0.538)
CONFIDENCE		0.137		0.183		0.106		0.199
		(0.520)		(0.354)		(0.625)		(0.319)
MALE		-0.978*		-1.016*		-1.157*		-1.131*
		(0.088)		(0.090)		(0.058)		(0.058)
INST		0.275		0.279		0.472		0.363
		(0.732)		(0.717)		(0.558)		(0.631)
BLOCK		-0.003		0.089		0.013		0.097
		(0.973)		(0.251)		(0.880)		(0.206)
CONST	-0.155	0.746	-0.691	-2.492*	-0.404	0.484	-0.848	-2.576*
	(0.758)	(0.608)	(0.192)	(0.099)	(0.432)	(0.752)	(0.120)	(0.093)
N	210	191	228	202	210	191	228	202
Pseudo R^2	0.067	0.084	0.052	0.080	0.085	0.114	0.055	0.093
Industry & Year Fixed Effect	NO	NO	NO	NO	NO	NO	NO	NO
Predicted probability when NS80 =0					0.429	0.414	0.445	0.440
Predicted probability when NS80 =1					0.804***	0.824***	0.692***	0.704***

Table 8

Table 7 reports panel data random-effects regressions results where the dependent variables are INSTBOWN and LFIVEOWN. The panel variables are year and firm-ID (gvkey). The dependent variables are informative about large institutional shareholding in a firm (see Appendix 1.1 for the variable definition). The key explanatory variable is the CEO narcissism proxies discussed in earlier sections. We use both the continuous measure of CEO narcissism (NS) and a dummy variable (NS80) that takes the value one if the CEO's narcissism score is among the top 20 per cent scores in the sample. Model 1 and 3 have INSTBOWN as the dependent variable and Model 2 and 4 have LRG5OWN as the dependent variable. In every specification, year fixed effects and industry fixed effects are included. All the continuous variables are winsorized at 1% and 99% levels. P-values in the parentheses reflect robust standard errors clustered by firm ID (gvkey). ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

	(1)	**(2)**	**(3)**	**(4)**
Dependent variable	INSTBOWN	LFIVEOWN	INSTBOWN	LFIVEOWN
NS	-0.062**	-0.050**		
	(0.023)	(0.028)		
NS80			-0.109**	-0.074**
			(0.028)	(0.043)
SIZE	0.486***	0.462***	0.486***	0.462***
	(0.000)	(0.000)	(0.000)	(0.000)
MB	-0.007	-0.009**	-0.007	-0.009**
	(0.193)	(0.030)	(0.199)	(0.030)
LEV	0.453***	0.320***	0.450***	0.320***
	(0.000)	(0.000)	(0.000)	(0.000)
ROA	-1.059***	-0.746***	-1.055***	-0.745***
	(0.000)	(0.000)	(0.000)	(0.000)
RET	-0.233***	-0.162***	-0.234***	-0.163***
	(0.000)	(0.000)	(0.000)	(0.000)
RD	1.585***	1.239***	1.576***	1.232***
	(0.005)	(0.002)	(0.005)	(0.002)
AGE	-0.003	0.001	-0.003	0.001
	(0.377)	(0.567)	(0.390)	(0.575)
CHAIR	0.037	0.007	0.038	0.007
	(0.295)	(0.792)	(0.286)	(0.792)
CONFIDENCE	-0.075**	-0.072***	-0.074**	-0.069***
	(0.030)	(0.005)	(0.031)	(0.006)
MALE	-0.103	-0.001	-0.097	0.004
	(0.235)	(0.981)	(0.274)	(0.940)
BOARD	0.012	0.005	0.011	0.004
	(0.223)	(0.418)	(0.266)	(0.471)
HHI	6.731***	3.582***	6.739***	3.586***
	(0.000)	(0.000)	(0.000)	(0.000)
INST	1.193***	0.430***	1.178***	0.425***
	(0.000)	(0.000)	(0.000)	(0.000)
CONST	11.72***	12.88***	11.76***	12.90***
	(0.000)	(0.000)	(0.000)	(0.000)
N	3929	4216	3929	4216
Wald chi-square	1361.65***	1116.63***	1363.27***	1118.57***
Year Fixed effects & Industry Fixed effects	YES	YES	YES	YES

Table 9

Panel A of the table reports the buy and hold abnormal return (BHAR) for event different firm portfolios. Event firms are devided into two groups based on the CEO narcissism score dummy, i.e. NS80. The high narcissism CEO group has target firms whose CEOs were among the top twenty percent of the CEOs in our sample of 778 CEOs. The low narcissism CEO group, on the other hand, has target firms whose CEOs were among the bottom eighty per cent of the CEOs in our sample of 778 CEOs. Panel A reports BHAR assuming annual rebalance. Both equal weighted and value weighted BHARs are reported. We 18- and 24-month BHAR for both portfolios. ***, **, and * represent the level of significance (based on two-tailed p-values from a two-sample t-test assuming unequal variances) at the 1%, 5%, and 10% level, respectively, and indicate whether the mean BHARs are significantly different form each other.

Panel B reports the results from the post-event performance analysis following a matching firm portfolio approach. Matching firm portfolio is based on Industry (Fama-French-30), SIZE, MB. The details regarding control firm selection can be found in previous sections. We first calculate the difference for each variable between the target firm and corresponding peer firm in each of the fiscal-year from t-2 to t+2. Then we calculate the mean value of the differences for each item. The definitions for ROA and RET is in Appendix 1.1. The table shows the mean of differences between the target firm and peer firm ROA and RET for each year between year t-2 and t+2 (first two rows with values). In the last two rows the mean of difference between prior year difference and post year(s) difference are reported. ***, **, and * represent the level of significance (based on two-tailed p-values from a two-sample t-test assuming unequal variances) at the 1%, 5%, and 10% level, respectively, and indicate whether the mean values associated with low narcissism CEO group are significantly different form the mean values associated with the high narcissism CEO group.

Panel C reports the results associated with the post-event performance analysis following a matching firm portfolio approach following the procedure. Matching firm portfolio is now based on Industry (Fama-French-30), SIZE, MB, and ROA. ***, **, and * represent the level of significance (based on two-tailed p-values from a two-sample t-test assuming unequal variances) at the 1%, 5%, and 10% level, respectively, and indicate whether the mean values associated with low narcissism CEO group are significantly different form the mean values associated with the high narcissism CEO group.

Panel A: performance analysis based on BHAR

	High narcissism CEO NS80 = 1		**Low narcissism CEO NS80=0**	
	Equal-weighted annual rebalance	Value-weighted annual rebalance	Equal-weighted annual rebalance	Value-weighted annual rebalance
BHAR 18-month	-0.082	-0.073	0.106**	0.114**
BHAR 24-month	-0.009	-0.001	0.081	0.100

Panel B: Performance analysis based on Industry, MB, and SIZE matched portfolio

	High narcissism CEO NS80 = 1					-	Low narcissism CEO NS80=0				
	t-2	**t-1**	**t**	**t+1**	**t+2**		**t-2**	**t-1**	**t**	**t+1**	**t+2**
ROA	-0.055	-0.052	-0.076	-0.067	-0.052		-0.005*	-0.008	-0.015**	-0.021*	-0.001
RET	-0.098	-0.068	-0.175	-0.095	0.156		-0.001	-0.176	0.037*	0.054*	-0.025*
	(t+1) – (t-1)	**(t+2) – (t-1)**					**(t+1) – (t-1)**	**(t+2) – (t-1)**			
RET	-0.027	0.244					0.223*	0.131			
ROA	-0.015	-0.001					-0.014	0.004			

Panel C: Performance analysis based on Industry, MB, SIZE, ROA matched portfolio

	High narcissism CEO NS80 = 1					Low narcissism CEO NS80=0				
	t-2	**t-1**	**t**	**t+1**	**t+2**	**t-2**	**t-1**	**t**	**t+1**	**t+2**
ROA	-0.027	-0.017	-0.036	-0.023	-0.016	-0.006	-0.009	-0.015	-0.017	-0.005
RET	-0.023	0.004	-0.130	-0.161	0.133	0.008	-0.142	0.016	0.011	-0.016
	(t+1) – (t-1)	**(t+2) – (t-1)**				**(t+1) – (t-1)**	**(t+2) – (t-1)**			
RET	-0.165	0.146				0.148**	0.104			
ROA	-0.006	0.001				-0.008	0.003			

Table 10

Panel A of Table 9 reports the effects of covariates on the probability of being targeted by activists. The dependent variable is set to 1 for fiscal years, where the CEO is targeted by an activist and 0 otherwise. For each model, we report probit coefficients and their p-values based on robust standard error clustered by firm identifiers (i.e. gvkey). In specification 1 to 4, CEO narcissism scores (as captured by the variable NSEW) are continuous values. In Model 5 to 8, CEO narcissism score is an indicator variable (NSEW80). P-values, reflecting robust standard errors clustered by *firm ID* (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

Variable definitions are listed in Appendix 1.1. Column 1,2,5, and 6 reports results based on all firm-year observations for which data on every covariate is available. *Year fixed effects* and *industry fixed effects* are included by using Fama-French 30 industry portfolio for these specifications. Column 3,4,7, and 8 are based on target firm observations and observations from a sample of matching firms in the year before the activism event. Since matching firms are selected from the same industry and years are also matched, industry fixed effects and year fixed effects are not included in these specifications. In Column 3,4,7, and 8, the regression uses a sample of matching firms based on industry (Fama-French 30), SIZE, and MB as the control. The method we followed in selecting a peer firm for each fiscal year, where an activism event took place is discussed in one of the previous sections. The number of fiscal years where at least one activism event occurred is 114, and therefore, we selected 114 firms (and 114 CEOs) to construct a set of peer firms. We also report the predicted probability of an activism event in the last two rows. The last row reports the predicted probability when NSEW80 = 1 and the second last row reports the predicted probability when NSEW80=0. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NSEW80 is statistically significant.

Panel B reports results based on two more sets of peer firms. The probability of an activism event is modelled as the outcome of a probit regression. In specification 1 to 4, CEO narcissism scores (as captured by the variable NSEW) are continuous values. In Model 5 to 8, CEO narcissism score is an indicator variable (NSEW80). Column 1,2,5, and 6 reports results that are based on 10*10 portfolio matched control firms. Column 3,4,7, and 8 are based on an industry (Fama-French 30) SIZE, MB, ROA, matched counterfactual. P-values, reflecting robust standard errors clustered by *firm ID* (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively. Since matching firms are selected from the same industry and years are also matched, industry fixed effects and year fixed effects are not included in these specifications. We also report the predicted probability of an activism event in the last two rows. The last row reports the predicted probability when NSEW80 = 1 and the second last row reports the predicted probability when NSEW80=0. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NSEW80 is statistically significant.

Panel A

Probability of activism	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
NSEW	0.159*	0.168*	0.446***	0.381**				
	(0.053)	(0.072)	(0.005)	(0.044)				
NSEW80					0.228**	0.270***	0.598***	0.709***
					(0.020)	(0.009)	(0.006)	(0.004)
SIZE	0.061**	0.098**	0.038	0.101	0.061**	0.100**	0.056	0.125*
	(0.041)	(0.011)	(0.529)	(0.169)	(0.044)	(0.010)	(0.349)	(0.097)
MB	-0.038*	-0.055*	0.038	0.010	-0.038*	-0.056*	0.037	0.022
	(0.083)	(0.070)	(0.412)	(0.861)	(0.088)	(0.079)	(0.415)	(0.716)
LEV	0.215	0.059	0.819	0.451	0.232	0.080	0.772	0.338
	(0.419)	(0.827)	(0.173)	(0.498)	(0.385)	(0.767)	(0.204)	(0.614)

ROA	-2.068***	-2.179***	-0.976	-1.180	-2.053***	-2.178***	-1.021	-1.376
	(0.001)	(0.000)	(0.371)	(0.353)	(0.001)	(0.000)	(0.345)	(0.286)
RET	-0.455***	-0.298**	-0.792***	-0.666**	-0.460***	-0.305**	-0.842***	-0.766***
	(0.001)	(0.031)	(0.001)	(0.011)	(0.001)	(0.027)	(0.000)	(0.005)
RD	0.526	0.463	3.454	3.913	0.491	0.499	2.934	3.201
	(0.660)	(0.723)	(0.189)	(0.199)	(0.678)	(0.698)	(0.259)	(0.288)
AGE		0.008		0.030**		0.008		0.029*
		(0.252)		(0.049)		(0.245)		(0.056)
CHAIR		-0.017		-0.126		-0.030		-0.208
		(0.863)		(0.578)		(0.762)		(0.362)
CONFIDENCE		0.008		0.136		0.007		0.168
		(0.933)		(0.507)		(0.940)		(0.417)
MALE		-0.340		-1.062*		-0.368*		-1.205**
		(0.114)		(0.083)		(0.098)		(0.049)
INST		0.190		0.547		0.224		0.584
		(0.592)		(0.480)		(0.518)		(0.447)
BLOCK		0.022		0.063		0.027		0.075
		(0.597)		(0.429)		(0.520)		(0.349)
CONST	-2.517***	-2.970***	-0.414	-2.017	-2.573***	-3.055***	-0.647	-2.141
	(0.000)	(0.000)	(0.412)	(0.169)	(0.000)	(0.000)	(0.207)	(0.147)
N	4766	4232	228	198	4766	4232	228	198
Pseudo R^2	0.097	0.102	0.072	0.094	0.098	0.105	0.073	0.110
Industry & Year Fixed Effect	YES	YES	NO	NO	YES	YES	NO	NO
Predicted probability when NSEW80 =0					0.013	0.012	0.447	0.449
Predicted probability when NSEW80 =1					0.023**	0.024***	0.679***	0.719***

Panel B

Probability of activism	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NSEW	0.608***	0.644***	0.448***	0.372**				
	(0.001)	(0.001)	(0.005)	(0.039)				
NSEW80					0.927***	1.022***	0.571***	0.611**
					(0.000)	(0.000)	(0.009)	(0.011)
SIZE	0.053	0.084	0.046	0.126*	0.064	0.102	0.062	0.143**
	(0.369)	(0.273)	(0.445)	(0.074)	(0.277)	(0.185)	(0.302)	(0.044)
MB	0.044	-0.010	-0.006	-0.052	0.036	-0.019	-0.010	-0.047
	(0.332)	(0.864)	(0.893)	(0.437)	(0.439)	(0.767)	(0.844)	(0.506)
LEV	-0.022	-0.034	0.827	0.666	0.105	0.200	0.782	0.643
	(0.975)	(0.963)	(0.203)	(0.336)	(0.882)	(0.782)	(0.236)	(0.355)
ROA	-2.619*	-2.084	0.182	1.508	-2.426	-1.748	0.229	1.680
	(0.099)	(0.251)	(0.904)	(0.400)	(0.112)	(0.322)	(0.879)	(0.345)
RET	-0.386	-0.268	-0.568**	-0.292	-0.473	-0.354	-0.621**	-0.365
	(0.183)	(0.399)	(0.017)	(0.279)	(0.110)	(0.280)	(0.011)	(0.188)
RD	-1.329	-0.031	4.262	4.570	-2.573	-1.399	3.687	4.139
	(0.605)	(0.991)	(0.119)	(0.129)	(0.320)	(0.610)	(0.164)	(0.164)
AGE		-0.007		0.027*		-0.007		0.027*
		(0.661)		(0.091)		(0.654)		(0.088)
CHAIR		-0.249		-0.074		-0.290		-0.118
		(0.260)		(0.728)		(0.192)		(0.584)
CONFIDENCE		0.140		0.182		0.095		0.195
		(0.508)		(0.357)		(0.657)		(0.324)
MALE		-0.937		-0.999*		-1.149*		-1.125*
		(0.104)		(0.097)		(0.058)		(0.060)
INST		0.380		0.280		0.362		0.327
		(0.634)		(0.715)		(0.657)		(0.665)
BLOCK		-0.008		0.091		0.021		0.100
		(0.923)		(0.243)		(0.808)		(0.191)
CONST	-0.073	0.803	-0.605	-2.393	-0.331	0.618	-0.820	-2.611*
	(0.884)	(0.581)	(0.252)	(0.114)	(0.515)	(0.683)	(0.131)	(0.087)
N	210	191	228	202	210	191	228	202
Pseudo R^2	0.062	0.079	0.051	0.077	0.074	0.100	0.049	0.086
Industry & Year Fixed Effect	NO	NO	NO	NO	NO	NO	NO	NO
Predicted probability when NSEW80 =0					0.435	0.423	0.451	0.447
Predicted probability when NSEW80 =1					0.777***	0.796***	0.673***	0.684***

Table 11

Panel A of Table 6 reports the effects of covariates on the probability of being targeted by activists. The dependent variable is set to 1 for fiscal years, where the CEO is targeted by an activist and 0 otherwise. For each model, we report probit coefficients and their p-values based on robust standard error clustered by firm identifiers (i.e. gvkey). In specification 1 to 4, CEO narcissism scores (as captured by the variable NSsig) are continuous values. In Model 5 to 8, CEO narcissism score is an indicator variable (NSsig80). P-values, reflecting robust standard errors clustered by *firm ID* (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

Variable definitions are listed in Appendix 1.1. Column 1,2,5, and 6 reports results based on all firm-year observations for which data on every covariate is available. *Year fixed effects* and *industry fixed effects* are included by using Fama-French 30 industry portfolio for these specifications. Column 3,4,7, and 8 are based on target firm observations and observations from a sample of matching firms in the year before the activism event. Since matching firms are selected from the same industry and years are also matched, industry fixed effects and year fixed effects are not included in these specifications. In Column 3,4,7, and 8, the regression uses a sample of matching firms based on industry (Fama-French 30), SIZE, and MB as the control. The method we followed in selecting a peer firm for each fiscal year, where an activism event took place is discussed in one of the previous sections. The number of fiscal years where at least one activism event occurred is 114, and therefore, we selected 114 firms (and 114 CEOs) to construct a set of peer firms. We also report the predicted probability of an activism event in the last two rows. The last row reports the predicted probability when NSsig80 = 1 and the second last row reports the predicted probability when NSsig80=0. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NSsig80 is statistically significant.

Panel B reports results based on two more sets of peer firms. The probability of an activism event is modelled as the outcome of a probit regression. In specification 1 to 4, CEO narcissism scores (as captured by the variable NSsig) are continuous values. In Model 5 to 8, CEO narcissism score is an indicator variable (NSsig80). Column 1,2,5, and 6 reports results that are based on 10*10 portfolio matched control firms. Column 3,4,7, and 8 are based on an industry (Fama-French 30) SIZE, MB, ROA, matched counterfactual. P-values, reflecting robust standard errors clustered by *firm ID* (gvkey), are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively. Since matching firms are selected from the same industry and years are also matched, industry fixed effects and year fixed effects are not included in these specifications. We also report the predicted probability of an activism event in the last two rows. The last row reports the predicted probability when NSsig80 = 1 and the second last row reports the predicted probability when NSsig80=0. The significance level associated with the predicted probabilities in the last row indicates whether the difference in the coefficients for different levels of NSsig80 is statistically significant.

Panel A

Prob. of activism	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
NSsig	0.207**	0.213**	0.565***	0.518**				
	(0.013)	(0.015)	(0.003)	(0.015)				
NSsig80					0.251***	0.273***	0.778***	0.821***
					(0.006)	(0.008)	(0.001)	(0.002)
SIZE	0.074**	0.122***	0.081	0.159**	0.076**	0.123***	0.092	0.178**
	(0.017)	(0.003)	(0.179)	(0.031)	(0.015)	(0.003)	(0.134)	(0.020)
MB	-0.041*	-0.063*	0.000	-0.024	-0.041*	-0.062*	0.012	-0.018
	(0.078)	(0.070)	(0.994)	(0.717)	(0.086)	(0.076)	(0.807)	(0.790)
LEV	0.294	0.156	0.873	0.543	0.295	0.154	0.930	0.685
	(0.272)	(0.558)	(0.202)	(0.481)	(0.276)	(0.564)	(0.175)	(0.363)

ROA	-2.088***	-2.167***	-0.620	-1.263	-2.046***	-2.085***	-0.796	-1.148
	(0.002)	(0.001)	(0.628)	(0.419)	(0.002)	(0.001)	(0.534)	(0.473)
RET	-0.475***	-0.300**	-0.741***	-0.578**	-0.478***	-0.307**	-0.702***	-0.559**
	(0.001)	(0.038)	(0.002)	(0.044)	(0.001)	(0.034)	(0.003)	(0.049)
RD	0.805	0.978	4.529	6.344*	0.777	0.928	4.352	6.147*
	(0.502)	(0.470)	(0.108)	(0.055)	(0.518)	(0.485)	(0.139)	(0.078)
AGE		0.010		0.038**		0.011		0.042***
		(0.155)		(0.019)		(0.124)		(0.009)
CHAIR		0.001		-0.015		0.000		-0.001
		(0.992)		(0.949)		(0.996)		(0.995)
CONFIDENCE		0.015		0.078		0.017		0.059
		(0.876)		(0.714)		(0.856)		(0.788)
MALE		-0.429*		-1.347**		-0.419*		-1.333**
		(0.066)		(0.039)		(0.060)		(0.027)
INST		0.089		0.982		0.113		0.855
		(0.807)		(0.217)		(0.752)		(0.282)
BLOCK		0.038		0.066		0.037		0.078
		(0.380)		(0.439)		(0.387)		(0.344)
CONST	-2.735***	-3.348***	-0.771	-3.056**	-2.820***	-3.512***	-1.046*	-3.586**
	(0.000)	(0.000)	(0.134)	(0.043)	(0.000)	(0.000)	(0.051)	(0.015)
N	4564	4048	218	188	4564	4048	218	188
Pseudo R^2	0.104	0.114	0.082	0.125	0.105	0.116	0.093	0.146
Industry & Year Fixed Effect	YES	YES	NO	NO	YES	YES	NO	NO
Predicted probability when NSsig80 =0					0.012	0.012	0.432	0.441
Predicted probability when NSsig80 =1					0.023***	0.023***	0.727***	0.750***

Panel B

Prob. of activism	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
NSsig	0.585***	0.631***	0.414**	0.342*				
	(0.002)	(0.002)	(0.024)	(0.087)				
NSsig80					0.812***	0.817***	0.563***	0.587**
					(0.000)	(0.001)	(0.010)	(0.013)
SIZE	0.094	0.145*	0.083	0.172**	0.101*	0.150*	0.088	0.180**
	(0.119)	(0.064)	(0.172)	(0.015)	(0.100)	(0.055)	(0.158)	(0.012)
MB	0.012	-0.039	-0.041	-0.103	0.021	-0.036	-0.036	-0.102
	(0.801)	(0.537)	(0.425)	(0.185)	(0.666)	(0.583)	(0.496)	(0.201)
LEV	0.247	0.275	0.906	1.031	0.230	0.401	0.955	1.154
	(0.726)	(0.714)	(0.180)	(0.150)	(0.746)	(0.585)	(0.162)	(0.101)
ROA	-2.667*	-2.079	0.780	2.444	-2.562	-1.775	0.763	2.799
	(0.096)	(0.262)	(0.615)	(0.193)	(0.105)	(0.328)	(0.628)	(0.141)
RET	-0.385	-0.227	-0.460*	-0.121	-0.460	-0.278	-0.441*	-0.099
	(0.193)	(0.495)	(0.062)	(0.674)	(0.126)	(0.406)	(0.073)	(0.724)
RD	-0.416	1.129	5.770**	8.254**	-0.909	0.930	6.032**	8.839**
	(0.879)	(0.704)	(0.047)	(0.012)	(0.748)	(0.763)	(0.048)	(0.011)
AGE		0.003		0.042**		0.008		0.045***
		(0.856)		(0.012)		(0.631)		(0.006)
CHAIR		-0.170		0.023		-0.105		0.039
		(0.451)		(0.917)		(0.643)		(0.857)
CONFIDENCE		0.082		0.127		0.017		0.101
		(0.714)		(0.544)		(0.940)		(0.633)
MALE		-1.375**		-1.156*		-1.241**		-1.047
		(0.020)		(0.070)		(0.034)		(0.103)
INST		0.400		0.570		0.543		0.521
		(0.618)		(0.471)		(0.490)		(0.511)
BLOCK		0.035		0.091		0.028		0.089
		(0.684)		(0.268)		(0.750)		(0.269)
CONST	-0.417	-0.047	-0.972*	-3.905**	-0.672	-0.843	-1.160**	-4.462***
	(0.411)	(0.975)	(0.076)	(0.011)	(0.204)	(0.563)	(0.044)	(0.004)
N	201	182	217	191	201	182	217	191
Pseudo R^2	0.060	0.085	0.049	0.102	0.072	0.096	0.056	0.116
Industry & Year Fixed Effect	NO	NO	NO	NO	NO	NO	NO	NO
Predicted probability when NSsig80 =0					0.426	0.424	0.448	0.446
Predicted probability when NSsig80 =1					0.734***	0.734***	0.667***	0.674**

Table 12

The table reports some descriptive statistics related to a survey and results from an experimental study. The variable NPI-40 is the narcissism score of the subjects computed based on the 40-item Narcissistic Personality Inventory or NPI-40 (Raskin and Terry, 1988). The value for NPI-40 ranges from 0 to 40 (integer values). The variable NPI-10 is consisting of ten questions from the NPI-40 that, according to Ames et al. (2006), are mostly associated with narcissism. The value for NPI-10 range from 0 to 10 (integer values). There were 48 leaders in four treatments analysed in the study. Panel A shows the number of leaders by quintile of narcissism score (based on the original sample of 384 participants). Panel B present experimental results with a focus on participants' narcissism scores. In column (3) of Panel B, the test statistics is based on a two-sample test of proportion, testing whether the same proportion of leaders from two samples (narcissistic and non-narcissistic) received punishment at least once. Test statistics in other columns, Column 4 to 8, are based on a t-test assuming unequal variances, testing whether the mean values associated with two samples (narcissistic and non-narcissistic) are statistically the same. In panel B, ***, **, and * represent the level of significance (two-tailed) at the 1%, 5%, and 10% level respectively; indicating whether the mean value or proportion associated with the group under consideration is significantly different from the mean value associated with the counterpart group.

Panel A

Sample with treatments involving punishment

	No. of leaders	Percentage
Bottom 20% NPI-40	9	18.75
2	11	22.92
3	13	27.08
4	4	8.33
Top20%NPI-40	11	22.92
Total	**48**	100

Sample with treatments involving punishment

	No. of leaders	Percentage
Bottom 20% NPI-10	12	25.00
2	10	20.83
3	15	31.25
4	5	10.42
Top 20% NPI-10	6	12.50
Total	**48**	100

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Leader groups: High narcissism vs low narcissism	Total number of leaders in treatments with punishment	Number of leaders in treatments with punishment who were punished at least once	Proportion of leaders punished at least once	Mean match before punishment is imposed for the first time	Mean match after punishment is imposed for the first time	Number of times on average a leader is punished by the active member	Number of matches before the next mismatch after punishment	Number of average periods before the punishment is imposed for the first time
				NPI-40				
Top 20% NPI-40	10	8	0.80	0.39	0.62	3.70	1.41	6.25
Bottom 80% NPI-40	38	15	0.39**	0.47	0.63	1.00**	1.87	9.80
Top 33.33% NPI-40	14	10	0.71	0.41	0.62	3.14	1.27	6.60
Bottom 66.66% NPI-40	34	13	0.38**	0.47	0.63	0.91**	2.16	10.08
				NPI-10				
Top 20% NPI-10	6	6	1.00	0.48	0.64	4.50	1.37	7.17
Bottom 80% NPI-10	42	17	0.40***	0.43	0.62	1.143***	1.76	9.06
Top 33.33% NPI-10	11	9	0.82	0.40	0.60	3.91	1.26	6.44
Bottom 66.66% NPI-10	37	14	0.38**	0.47	0.64	0.86**	2.16*	9.93

Table 13

The table presents random-effects probit regressions results based on a panel dataset. The panel variables are *unique subject ID* and *period*. Table reports the effects of covariates on the probability of being punished by active members of the group. The dependent variable is set to 1 for periods, where the leader is punished by the active member and 0 otherwise. For each model, we report probit coefficients and their p-values based on robust standard error clustered by unique subject identifiers. In Model 1 to 4, leader's narcissism score is an indicator variable. TOP20NPI-40 (TOP20NPI-10) takes the value 1 if the leader belongs to the top 20 per cent of the subject pool according to her narcissism ranking based on the NPI-40 (NPI-10); TOP33NPI-40 (TOP33NPI-10) takes the value 1 if the leader belongs to the top 33.33 per cent of the subject pool according to her narcissism ranking based on the NPI-40 (NPI-10). In specification 5 and 6, leaders' narcissism scores are based on the NPI-40 and the NPI-10 respectively (integer values). In every specification, we include treatment dummies: *punish*, *rewpun*, and *rewpunFixed*. Control variables include *Leader'sGender* (value = 1 if the leader is a female and 0 otherwise); *Leader'sCRT* measures leader's cognitive ability measured by the three-item cognitive reflection test or CRT (Frederick, 2005); *Leader'sRiskaversion* measures risk aversion based on values reported by subjects (1 to 11), using survey inputs: very risk averse (1) to being very risk-seeking (11). P-values, reflecting robust standard errors clustered by *unique subject ID*, are in the parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

Prob. of punishment	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
TOP20NPI-40	0.905***					
	(0.001)					
TOP20NPI-10		1.231***				
		(0.000)				
TOP33NPI-40			0.750***			
			(0.002)			
TOP33NPI-10				0.916***		
				(0.000)		
NPI-40					0.0507**	
					(0.030)	
NPI-10						0.201***
						(0.000)
punish	-0.933**	-0.779**	-1.005**	-0.874**	-0.950**	-0.721**
	(0.018)	(0.013)	(0.012)	(0.018)	(0.030)	(0.042)
rewpun	-0.267	-0.570*	-0.463	-0.413	-0.562	-0.474
	(0.503)	(0.081)	(0.174)	(0.201)	(0.142)	(0.140)
rewpunFixed	-0.308	-0.301	-0.310	-0.170	-0.320	-0.304
	(0.249)	(0.245)	(0.252)	(0.521)	(0.286)	(0.272)
Leader'sAge	-0.063	-0.045	-0.050	-0.038	-0.050	-0.045
	(0.141)	(0.157)	(0.181)	(0.245)	(0.269)	(0.192)
Leader'sGender	-0.369	-0.429*	-0.232	-0.257	-0.270	-0.201
	(0.157)	(0.084)	(0.351)	(0.289)	(0.341)	(0.425)
Leader'sRiskaversion	0.045	0.157**	0.060	0.117**	0.045	0.100*
	(0.372)	(0.037)	(0.271)	(0.016)	(0.455)	(0.084)
Leader'sCRT	0.156	0.151	0.177	0.199*	0.197	0.174
	(0.213)	(0.209)	(0.145)	(0.085)	(0.130)	(0.172)
Intercept	-0.620	-1.754**	-1.072	-1.846**	-1.547	-1.903**
	(0.511)	(0.013)	(0.175)	(0.014)	(0.135)	(0.016)
N	1152	1152	1152	1152	1152	1152
Number of Subject Clusters	48	48	48	48	48	48

Paper 2

Decision Making Under Accountability

Abstract

We study accountability in a principal-agent set-up where a leader makes risky decisions on behalf of a three-person group. The experiment has 24 periods, and the leader makes a decision in every period. The decision involves choosing between a risk and a safer option. We study six accountability treatments (in addition to two no-accountability treatments) where a member from each group (an active member) can punish or/and reward the leader. Punishment and rewards are costly. Leaders exhibit more risk-seeking behaviour when the principal is unable to punish. We find that recipients are the happiest when the leader chooses the same option they (recipients) prefer. We also show that the punishment-only environment leads to the lowest amount of misalignment between leaders' choices and recipients' preferences. It is observed that punishment as an accountability mechanism is cheaper as the principal needed to use this less frequently. Unlike punishment decisions, reward decisions found to be biased toward the outcome of leaders' decision. Finally, we observe that although punishment decisions by principals are in line with a hybrid accountability system, rewards decisions are in line with an outcome accountability system.

Keywords: Accountability, Responsibility, Risk attitude, Reward, Punishment, Accountability systems

1. Introduction

Individuals often have to make decisions involving a varied level of risks in their daily life. Some examples are purchasing a house, changing jobs, and investing family funds. Typically, it is not only the individual who makes decisions are affected by the outcomes resulting from these decisions but other individuals too. The decision to purchase a new house is likely to affect everyone in the household. Similarly, in firms, managers act on behalf of shareholders, and investment managers invest on behalf of their clients. In the latter case (if not the former), the decision-maker is typically accountable for his choices. These

situations in which the choice of one person affects other individuals are a widespread phenomenon and given the influence of these decisions, it is essential to investigate the influence of accountability on choices under risk. In the literature, accountability is defined as the prospect that a decision-maker might need to validate his decisions to someone (Lerner and Tetlock, 1999). This term is closely related to responsibility and has prevailed as a debiasing tool. The pressure due to accountability is a pervasive aspect of social systems: essentially everybody has to answer to somebody for something. Accountability triggers deeper thinking processes and therefore, in general, improves the decisions (Vieider, 2011). Studying accountability mechanisms is interesting as a varied level of accountability is usually associated with choices in agency situations. Moreover, since accountability should force agents to think more prudently about the choices at hand, a researcher may create invaluable knowledge regarding the biases associated with the decision-making process.

Accountability is a necessary social contingency that shapes the way human beings behave and makes decisions ((Tetlock, 1985; 1992). Accountability forces individuals to become concerned about their social status and how others perceive them. Therefore, individuals are expected to behave differently when they expect their choices to be evaluated by an entity which may or may not be affected by the decisions. The presence of a salient audience who evaluate the decisions of the actor and then determine whether the actor would be punished or rewarded is often seen as a precondition for accountability (Hall and Ferris, 2011). Although evaluation is an integral part of the process, studies suggest that the actual evaluation does not need to take place to ensure accountability. The accountable individual, nevertheless, must be under the impression that there is a possibility that someone would evaluate his decision.

According to De Dreu and Van Knippenberg (2005), accountability goes beyond mere evaluation and involves consequences that the accountable person must face. The consequences are usually in the form of lost rewards or punishments. The notion of consequence related to the evaluation process separates “accountability” from “responsibility”. Although some studies used these two

constructs interchangeably, accountability is not the same notion as responsibility. Responsibility is usually seen as a subcomponent of accountability (Hall et al., 2017). We can think of accountability situations such as being the CEO of a public limited company and making decisions on behalf of the shareholder. Making decisions on behalf of the family members, on the other hand, is an example of decision making under the condition of responsibility. The phenomenon of accountability is more related to agency situations. Understanding accountability, therefore, is essential as any insight gained can be used to better comprehend the interplay between the agents and principals in an organizational set-up. In a principal-agent set-up, the agent can be held accountable for her decisions in various ways. Different accountability mechanisms such as rewards and punishments can be used by the principals to prod the behaviour of agents in the right direction. Our study attempts to understand the effectiveness of different accountability mechanisms on agents' decisions.

The experimental study examines situations, where a decision-maker is responsible for somebody else's outcomes, i.e., the decision-maker decides on behalf of a group. We have two treatments in the experiment in which the decision-maker assumes the role of the leader. In these two situations, the leader makes decisions on behalf of the group but does not face any consequences for the decisions, i.e., the leader is not held accountable for the decisions. The study also adds six situations (treatments) where the leader can be held responsible for the decisions by the recipient. So, in these situations, decision-makers are not only responsible but also accountable for their decisions. The leader representing a group of three individuals (including herself) makes binary choices between two alternatives. The investment choice tasks are conducted for eight treatments. The decision-maker is asked to pick repeatedly between two alternatives: risky and safer/less risky. The design of the experiments is motivated by Holt and Laury (2002) and is in line with Pahlke et al. (2015) and Bolton and Ockenfels (2010). The primary treatment variables are the nature of the contract specified for other members (i.e., principals) of the group. In baseline treatments (i.e. two responsibility treatments without accountability), the role of the members of the

group is passive. In other treatments, one of the members plays an active role. The active member has the power to influence the payoff of the leader after he or she observes the outcome of leader's decision. In six treatments, the active member can punish or (and) reward the leader after the outcome of the decision is known. Punishment and rewards are costly. We have two treatments involving both reward and punishment.

Our study is not the first attempt to inquire about responsibility, accountability, and decisions under risk. Overall, studies show that subjects become more risk averse on average under responsibility, i.e. when they make decisions for others. Bolton et al. (2015) show that subjects with a low level of risk aversion become more cautious when they are responsible for the pay-out of another participant.[22] Experimental investigations on the link between risk attitude and responsibility in agency settings find mixed results. The studies by Chakravarty et al. (2010) and Polman (2012) find that responsibility can reduce risk aversion. Both works study binary lottery choices. According to Eriksen and Kvaloy (2010) and Füllbrunn and Luhan (2014), on the other hand, responsibility can subdue risk-seeking behaviour.[23]

The purpose of the study is three-fold. We first investigate the influence of different accountability mechanism on the behaviour of the leader. We also check how the recipients in a group use different accountability tools and the effectiveness of different accountability mechanism in producing social benefits. Finally, the study touches upon the issue of accountability system preferences and contributes to the debate surrounding the superiority of two accountability systems: process accountability and outcome accountability.

We report the following results. We find that Incorporation of accountability in the form of reward makes leaders less risk averse. We show that for reward only

[22] Bolton et al. (2015), nevertheless, do not observe any shift towards riskier picks among more risk averse decision-makers under responsibility.

[23] Both studies use the investment task following Gneezy and Potters (1997), where agents make choices on behalf of groups of different sizes. Similarly, Reynolds et al. (2009) find that there is a positive association between responsibility and the degree of risk aversion.

scenarios, subjects are unaffected by responsibility and accountability and even become slightly more risk-seeking. In situations involving punishment as the accountability mechanism, conversely, leaders make either risk-neutral or risk averse decisions. In general, risk aversion does not seem to intensify when leaders are accountable for the decisions they make compared to no-accountability situations. This is true even in punishment-only treatments. The study also seeks to get a deeper insight into the behaviour of recipients in the setting, i.e. members who are affected by leaders' choices. We show that the recipients are never risk-seeking regarding the choices. It was found that active members are more likely to use reward than punishment as the tool to prod the behaviour of the leader in the desired direction. The same pattern is observed in passive members' reward and punishment suggestions. We suggest that altruism is the reason behind a disproportionately higher frequency of rewards. Our most interesting finding is that both active and passive members care more about whether the leader chooses the same prospect they preferred than anything else. Both active and passive members are pleased when the leader choose the same option they preferred. This is true for every treatment (p-values < 0.001). Although accountability through punishment does not force agents to make significantly more risk averse decisions, it produces more social benefits in the sense that the costs are lower, and the benefits are slightly higher. We observed outcome bias in principals' reward decisions, i.e. principals often rewarded decisions that they did not agree with after a favourable outcome. Punishment decisions, on the other hand, found to be generally less prone to outcome bias. We find that altruistically motivated inefficient reward decisions can encourage agents to take less risk averse decisions in environments where reward is the only accountability tool available to the principal. Therefore, we do not observe excessive risk-taking by agents when they face a threat of punishment. Finally, punishment decisions found to be in line with a hybrid accountability system. Reward decisions, on the other hand, seem to be in line with an outcome accountability system. The paper is related to the works in the literature of decision making for others in general (Bolton and Ockenfels, 2010; Pahlke et al., 2015); accountability (Lerner and Tetlock, 1999; Pahlke et al., 2012 Pollmann et al., 2014); accountability systems (Tetlock et al., 2013; Patil et al., 2017).

The following section presents a review of relevant literature. Part 3 presents the experimental design and the discussion on treatments. Part 4 develops testable hypotheses. Part 5 presents and analyses the results. Part 6 presents discussions on generalizability of the results and Part 7 concludes.

2. Literature Review

The economic circumstances in which decision-makers choices affect other individuals' outcomes, as well as her own, establish a common category of phenomena. It is often observed that in financial-agency contracts incentive for an agent overlaps with that of principal, meaning both are affected by the outcomes of choices made by the agent. For example, in corporations, top managers are rewarded with company stocks and the investment manager rewarded based on the profit he makes. Managers in public companies act as shareholders' agents and the decision they take effect the wealth levels of shareholders. These different circumstances have mostly been demonstrated with a single objective function seeing all choices like decisions that do not affect others (when these decisions do affect others). Several studies in recent years shaded lights on this topic. For example, Pahlke et al. (2015) compare decisions under responsibility with individual decisions, i.e. they compare decisions where the decision-maker is the only person to be affected with decisions where others' financial position is also affected.

Accountability is a phenomenon with several dimensions and has been attracting scholarly interests since ancient times. As per Plato "without accountability for our actions, we would all behave unjustly". Accountability is a vital element not only of an organisation but also in societies where organisations operate (Hall et al., 2003). In a society without accountability, individuals would not be concerned about the consequences of their actions (Mitchell et al., 1998). Accountability also makes it easier for individuals to coordinate within an organisation (Frink and Klimoski, 1998). Accountability is understood to be a meso-level (Frink et al., 2008; Lerner and Tetlock, 1999) as well as a multi-level

concept by researchers and is related to different types organisational studies (Frink and Klimoski, 1998; Lerner and Tetlock, 1999). Our study mainly pays attention to accountability at the micro-level, which has received considerable attention in the literature of organisational studies and experimental economics.

An important issue concerning delegated decisions is whether there is a misalignment between the risk preferences of the decision-maker and parties affected by those decisions. The findings from experimental studies on risk-taking for others are mixed. According to Bolton and Ockenfels (2010), decision-makers, in general, have a tendency to be more risk averse when others are also affected by what they decide. Nevertheless, they may lean more towards a riskier option when the safer option involves unfavourable inequality. While Bohnet et al. (2004) show that inequality resulting from a risky choice does not affect risk-taking. In Eriksen and Kvaløy (2010), decision-makers acted cautiously when they were acting as agents for another entity, i.e. making decisions on behalf of others. The study compared behaviour in a stag hunt game in which a dictator makes the decision on behalf of the group and behaviour in the game in which an individual makes the decision only for himself. Chakravarty et al. (2011) study the attitude toward risk of subjects making decisions for others in a simple experimental set-up. The focus of their study was on a comparison between subjective preferences in situations when only the decision-maker makes decisions for himself and situations when the decision-makers make decisions for others. They find that subjects are much less risk averse when they are making decisions for others compared to the decisions they make for themselves. Subjects were generally risk averse when only they are affected by the decisions they make and appear close to risk-neutral when they are making decisions for others.

The works in the area of microeconomic agency literature have generally highlighted an approach that is based on evaluating an agent's performance based on the results he or she produced, which is established by linking the compensation to outcomes. In doing so, it is often overlooked that outcomes can be solely determined by pure luck and not the precision of the decision-making process. Several studies in the literature of psychology, nonetheless, suggest that rewarding

individuals solely based on the performance (i.e. the observed outcomes) can be suboptimal for situations where luck plays an important role (in addition to skill) (See Mauboussin, 2010; Tetlock et al., 2011). Some scholarly works in the literature of economics also suggest that managers or executives are frequently rewarded based on luck driven outcomes.

In our accountability treatments, we allow the recipients to know the decision process, as well as the outcomes of the decision before the payoff of the agent (i.e. the leader of the group), can be determined. Lerner and Tetlock (1999) argued that executives should be held accountable for the decision-making process and not for the outcomes that are mainly driven by luck. In our study, the active member who can punish or reward the leader knows not only the outcome of the decision but also the choices made by the leader. This discussion motivates one of the hypotheses we propose. We suspect that recipients in a group would value decisions that are in line with their preferences and not just the ones that produced the best results. We proposed to analyse variables such as the level of happiness associated with the choices made by the leader to shed light on this issue.

The level of risk aversion in individuals can change with circumstances. Stoner (1961) termed the phenomenon of becoming less risk averse (after starting at a lower level of risk-taking) owing to exogenous circumstances as risk shift. The opposite is termed as cautious shift, i.e. becoming more risk averse owning to exogenous shocks. The seminal work of Holt and Laury (2002) show that individuals are generally risk averse, and the level of aversion goes up with an increase in the stake. Andersson et al. (2014) examine whether the presence of potential loss has an of effect on risk-taking for others. The experimental study showed that the magnitude of risk individuals take while making decisions for themselves is similar to the level of risk they take when the decisions are taken on behalf of others in the absence of losses. On the other hand, when a loss is a possibility, decision-makers demonstrated less risk aversion while making decisions for others. It was shown that the increase in risk-taking results from a reduction in loss aversion for decisions on behalf of others (see Andersson et al., 2014).

In the literature of decision making for others, more attention is paid to the behaviour of the decision-maker. The behaviour of the parties affected by the decision has largely been ignored is this strand of literature. One of the motivations behind our experimental design is the shirking problem or more formally known as the agency problem in organisations involving joint team production, which has been the focus of many academic works in the field of economics. For instance, according to Alchian and Demsetz (1972), although there are many benefits of team production, it comes at a cost as it is hard to match compensations to each person's contribution. The problem of shirking is unusually severe in corporations. The seminal work of Berle and Means (1932) focuses on the agency problem that arises because the management of a corporation does not own 100 per cent of the firm, i.e. ownership is separated from control. Shareholders can use different mechanisms to deal with this issue. In the literature of finance, the mechanism generally known as "voice" is often associated with a specific shareholder group in the form of activist shareholders. This shareholder group are more involved with the process of disciplining the management. The work of Alchian and Demsetz (1972) suggests "modifications" are necessary to deal with the shirking problem that is inherent in modern corporations. Shareholder activism can be one of those "modifications". A direct intervention can be hostile or non-hostile and can take many different forms: proposing a strategic change via a public shareholder proposal or a private letter to management or voting against directors at an annual meeting (Cronqvist and Fahlenbrach, 2008). We argue experimental designs concerning decision making under accountability should pay attention to the fact that in the principal-agent set-up principals typically have varied level of involvement; some principals are more involved with the monitoring process than others. Hence, we allow our recipients (other members of the group) to assume an active as well as a passive role.

Performance evaluation is an important organizational issue and occupies an important place in the accountability literature. Performance evaluation systems can be divided into two types: process accountability and outcome accountability (Patil et al., 2017). As per Holmstrom and Milgrom (1991), the person making

decisions in a process accountability regime needs to explain how he generated the result. So, in this system means rather than the ends are crucial in evaluating the decision-maker. On the other hand, outcome accountability focuses on the end product, i.e. the result of the decision. Hence, it can be suggested that under the process accountability system, a decision would be considered as a right decision if it complies with the specified rules even if the outcome of the decision is unfavourable or below expectation.

Researchers also identified a third accountability regime in the form of hybrid accountability system (Chang et al., 2017). A hybrid accountability system allows both the outcome of the decision and the process that is followed to make the decision to be incorporated in the evaluation process. The debate surrounding process versus outcome accountability has enriched the accountability literature by identifying the pros and cons of both approaches. These two systems follow different approaches when it comes to specify two things: whom the accountable persons answer to and for what (Frink and Klimoski, 2004). The later gets much attention in research works championing one of these two accountability systems. Although the literature on process versus outcome accountability is already rich and growing, there has been little research in experimental economics focusing on the preferences of the audience in terms of choosing an accountability system. Most of the works attempting to understand this issue run a horse race among the accountability regimes. Our study, on the other hand, proposes to check whether the evaluator is more inclined to choose just one of the systems or a combination of both systems (i.e. a hybrid system of performance monitoring).

3. Experimental Design

3.1 Data

We ran 16 experimental sessions in total. A typical session had 18 participants, and they were divided into groups of three individuals. In one session, we had 15 participants. The role of a participant is randomly determined. Overall, 285 subjects were recruited at the CRIBS laboratory of the University of Nottingham,

using ORSEE (Greiner, 2015). The experiment lasted approximately 55 minutes. Some treatments took more time compared to other treatments. For example, treatments without any active member in a group, i.e. treatments where the decision-maker is not accountable, required fewer minutes compared to treatments involving an active member in a group. Participants earned £10.20 on average. The experiment was conducted on computers using zTree (Fischbacher, 2007). The fraction of female subjects in the experiment was 53.05 per cent, and the mean (median) age of the sample was 23.44 (22.00) years.

3.2 Game design

Tasks: The leader of the group (i.e., the decision-maker) chooses between a safer prospect and riskier prospect in every period. There were 24 periods in total. The safer prospect is characterized by either a certain amount of money or prospect with a low level of variation in comparison with a riskier prospect. In twenty-one, out of the total of twenty-four periods, the safer option is a certain amount, and in three periods, the safer option is a prospect with a lower variation. In case of the risky prospect in every period and the safer prospect in those three periods, there is a 50 per cent probability of getting one of two outcomes. One of the possible outcomes is more desirable in the sense that it results in a bigger payoff. The prospects are of eight different types (see Chart 1) including positive-only amounts, negative-only amounts and a mixture of both. The decisions are made sequentially, which means that subjects will not have an opportunity to go back to a previous decision to amend it. Everything discussed here is clearly explained in the instructions.

Prospects: The choices (24 in total) for the group-leaders were constructed systematically in the following way. Three different stake levels that are denoted by X, where X= € {2, 6, and 10} (see Chart 1) are used. For these three stake levels, subjects would choose between the following eight prospect pairs:

Gain: Subjects can choose between a certain amount and a prospect yielding either twice that amount with a probability of 0.5 or 0.

Lottery: A risky prospect is now the same as the base case, but a safe payment is substituted for a prospect with a low level of variance (the expected value in this case is the same).

Loss: This replicates the basic prospect pairs (prospect *Gain*), but here the amounts are negative instead of positive.

Mixed: Here, a certain amount in the basic prospect pairs is subtracted from all outcomes, as a consequence prospects are offered with an expected value of 0.

Mean preserving: the base case is modified by increasing and decreasing the alternatives of the risky option by 0.5.

Positive shift: All the amounts are increased by 50 per cent of the safe payment.

Up: In comparison with base example, the risky prospect kept at the same level but the safe payment is increased by 25 per cent. This is done to check level of risk aversion

Down: Similar to the previous case except that now the safe payment is decreased by 25 per cent.

Chart 1: Prospects

Prospects	Safe prospects		Risky prospects	
Gain (base)	X		2X	0
Lottery	0.5X	1.5X	2X	0
Loss	-X		-2X	0
Mixed	0		-X	X
Positive shift	1.5 X		2.5X	0.5X
Mean preserving	X		2.5X	-0.5X
UP	1.25X		0	2X
Down	0.75X		0	2X

3.3 Treatments

The experimental study examines decisions for situations in which a leader makes decisions on behalf of a group. The leader of the group is responsible for repeatedly choosing between a risky option and a safer option. The decision-maker knew that she had to decide on behalf of herself and two other subjects sitting in the laboratory, whose identity were not disclosed. Other subjects were told that they were in a passive or active (depending on the nature of the treatment) role. Moreover, they knew that somebody else in the laboratory would make the decisions on their behalf. The matching remained fixed throughout the rounds.

Treatment descriptions are given below:

T1(*plain*): No punishment or reward and the payoff is the same for everyone in a group.

T2 (*plainFixed*): No punishment or reward and the leader receives a fixed salary every period.

T3 (*reward*): An active member of the group can reward the leader. The payoff is initially the same for everyone in a group, but the leader can get more if rewarded.

T4 (*punish*): An active member of the group can punish the leader. The payoff is initially the same for everyone in a group, but the leader can get less if punished.

T5 (*rewpun*): The leader can be rewarded or punished by an active member of the group. The payoff is initially the same for everyone in a group, but the leader can get more (less) if rewarded (punished).

T6 (*rewpunFixed*): The leader receives a fixed amount at the beginning of every period and can be rewarded or punished by an active member of the group. A reward adds a pre-specified amount to the leader's payoff in a period. One the other hand, punishment causes the leader to lose the same amount from the fixed amount.

T7 (*rewardFixed*): The leader receives a fixed amount at the beginning of every period and can be rewarded by an active member of the group. A reward adds a pre-specified amount to the leader's payoff in a period.

T8 (*punishFixed*): The leader receives a fixed amount at the beginning of every period and can be punished by an active member of the group. Punishment causes the leader to lose a pre-specified amount from the fixed amount.

Treatment dimensions are summarized in Chart 2; treatments names are written in italic and bold. There are eight treatments in total. The treatment variables are the nature of the contract that determines the payoff and the role of other members of a group. The total of 285 subjects is divided into 95 groups. Principals periodic payoff in our experiment depends on leaders' decision. In the accountability

treatments, however, the principal assuming the role of the monitor can incur a cost for the group if she chooses to reward or punish the leader. If a reward is sanctioned, the leader gets 1.20 Experimental Currency Units (ECU henceforth), and if punishment is imposed, the leader loses ECU 1.20. The cost associated with the accountability mechanism is borne equally by the active and the passive member of the group in six accountability treatments. Each member would lose ECU 0.60 if the active member decided to reward or punish.

Chart 2: Dimensions across treatments are summarised in this chart**.** In the first two rows the treatment names are listed in italic. There are six accountability treatments and two no-accountability treatments. An accountability treatment involves reward or punishment, i.e. in an accountability treatment the active member can punish or reward the leader. There are two treatments (listed in the last column) where the active member can reward as well as punish the leader. A reward (punishment) adds (deducts) a pre-specified amount, i.e. ECU 1.20, to (from) the leader's payoff in a period.

Fixed payoff for the leader	***plainFixed***	***rewardFixed***	***punishFixed***	***rewpunFixed***
Variable payoff for the leader	***plain***	***reward***	***punish***	***rewpun***
Leader is accountable	NO	YES	YES	YES
Active member can reward	NO	YES	NO	YES
Active member can punish	NO	NO	YES	YES

In four out of the eight treatments the leader of the group initially receives a fixed amount (treatments are listed in the first row of Chart 2). Members in these treatments receive a variable payoff which depends on the leader's decision. In three of those four treatments with an initial flat amount for the leader, the end payoff in a given period can be different form the initial payoff depending on whether or not the active principal decides to reward or punish. These three treatments are accountability treatments and the other is not. There are four more treatments where the leader and other members initially receive the same payoff in every period (treatments are listed in the second row of Chart 2). The payoff is dependent on the leader's decision. The end payoff of the leader and the member of the group could be different from the initial payoff depending on the reward punishment decision taken by the active member in three accountability treatments.

In summary, six out of the total of eight treatments (T3 to T8) involve the presence of an evaluator. We call these treatments accountability treatments. The evaluator plays an active role, i.e. he/she is given the power to influence the payoff

of the leader. In the rest of the treatments, the decision-maker is not held accountable for the decision, i.e. his/her payoff cannot be influenced by other members of the group.

3.4 The game: stages

The leader makes a decision (choosing between a risky and a safer option), and other members of the group indicate their preferences regarding the leader's choice.

The result is shown to leader and the leader is asked for his/her level of happiness regarding the decision.

Leader's decision is shown to the members of the group (not the result yet), and the members are asked to indicate their level of happiness (scale: 0 to 9) regarding the leader's choice.

The result is shown to members.

In some treatments (T3 to T8), active members can punish or/and reward the leader at this stage.

Payoffs are shown to everyone.

Participants proceed to the next period.

It can be noted that in T5 and T6 the contracts specified for the representative of the principals (i.e. the active member of the group) in a group has more flexibility. On the other hand, in T1 and T2, both members of the group are playing a passive role. In the first stage of each period, the leader makes decisions on behalf of the group (themselves and at the same time, other group members). As explained before, the decision involves choosing between a risky option and a safer option. The order of the lotteries is the same for every treatment. Nevertheless, the order is initially randomised, i.e. when used for the first session. After constructing the decisions, we randomly determined the order of 21 decisions involving a safe prospect with a sure payoff. And then we randomised 3 other decisions involving a safe prospect without sure payoff. Other members of the group will be provided

with information on the decision of their leaders. Members are also asked to indicate their level of happiness regarding the leader's decision and their preferences regarding the leader's choices. This particular stage of the design is related to H3 as it allows us to check whether there is any discrepancy between the leader's choices and the ex-ante choices of other members.

After stating their level of happiness/satisfaction, members of the group will be shown the result associated with their leader's decision. In T3 to T8, the active member can punish or reward the leader and therefore, can influence the payoff of the leader after observing the result. If the leader chooses the risky option (or the safe option in the last three periods), the active member needs to wait until the uncertainty resolved to make reward or punishment decision. Punishment and reward are costly. Both active and passive members of the group will incur a cost equal to ECU 0.60 if the active member chooses to punish or reward the leader. An active member here is deciding on behalf of both. The passive member of the group (in T3 to T8) can suggest whether the leader should be punished or rewarded. Passive member's suggestion does not influence the payoff of the leader.

4. Hypotheses

The findings in experimental studies on decision making for others do not lead to a clear-cut conclusion. Some studies reported that decision-makers tend to become risk averse when they make decisions for others, and some other studies suggest the opposite (and some suggest no effects of responsibility). The study argues that the existence of a mechanism that the principal (the active member of the group) can use to prod the behaviour of the decision-maker should have a material impact on decision-maker's behaviour. Our study differs from those studies where decisions made in individual contexts are compared with decisions under responsibility, i.e., decisions made on behalf of others. Studies such as Bolton and Ockenfels (2010) and Pahlke et al. (2015) are based on that premise. We only consider situations where a leader makes decisions on behalf of the group. The primary interest of this study is the comparison among circumstances where

the parties affected by the decision have access to some contracts. We aim to investigate whether there is any relationship between the nature of the accountability mechanism that the affected party (other members in the group) has and the leader's decision-making behaviour.

The study incorporates accountability by adding conditions that allow one member of a group to hold the leader accountable for the decision. The decision-maker is evaluated under various conditions after the outcome associated with the decision is known. We allow the principal with the responsibility of evaluating the leader to affect (to some extent) leader's payoff in every period to make sure that the threat is viable. Moreover, the cost associated with using the accountability tool, i.e., rewarding or punishing the leader, ensures that the task is taken seriously by the active member of the group in corresponding accountability treatments. In Pahlke et al. (2012), accountability is incorporated by allowing the recipients (i.e., the principal) to ask the decision-maker (i.e., the agent) to justify their decisions. The agent, for example, explains the reasons behind the choices he or she made in front of the principal. The agent, in this arrangement, does not face any monetary consequences. Pollmann et al. (2014) used reward rather than punishment as the accountability mechanism.[24]

A reward-only environment is expected to encourage the decision-makers to take more risk under accountability. We suggest that how the accountability holder uses accountability tools should influence the behaviour of the decision-maker. We predict that active members would use rewards more often compared to the punishment due to altruistic motives. Therefore, there is a higher chance that active members will reward decisions that are not in line with their preferences. If reward and punishment decisions are altruistically motivated, then we should see an outcome bias in the reward decisions which would encourage the leader to take more risk if the principal is unable to punish the leader. It means that the leader

[24] Pollmann et al. (2014) compare the effectiveness of ex-post and ex-ente accountability in situations where agents made investment decisions on behalf of others.

will take less risk averse decisions in treatments where the active member can not punish. We propose the following hypothesis:

H1: Accountability through reward would encourage agents to take more risk on behalf of others.

The incompatibility of goals between the agents and principals is the key discussion point in the Principal-Agent Theory (see Jensen and Meckling, 1976). According to Kreps (1990), if the parties in a principal-agent set-up differ concerning their attitude towards risk, efficiency loss could be the outcome. It was suggested that the problem could be tackled by creating sufficiently flexible contracts for the principal (also see Grossman and Hart, 1983). H1 is motivated by the fact that the principal's ability to affect the payoff of the decision-maker should affect the decision-making behaviour of the leader, especially risk-taking. If it is assumed that efficiency loss is the lowest when the discrepancy between choices of the agent and the principal is at the lowest level, a mechanism like monetary reward or punishment allows the principal to nudge the behaviour of the agent toward a desirable direction. On the other hand, when the principal does not have access to any contracts, he is unable to influence the decision-making behaviour of the agent.

Sustainable progress and intact social stability require collective efforts. While human beings are inherently competitive and, experimental and field studies provide evidence that humans would be willing to cooperate under the right conditions (Sigmund, 2010). Lack of cooperation leads to the misuse of public goods, for example, green resources or community benefits. In pairwise exchanges, reciprocation can induce cooperation (Nowak and Sigmund, 2005). Nevertheless, in a situation where more than two individuals are interacting, punishment rather than reciprocation is expected to sustain cooperation (Fehr and Gächter, 2002; Traulsen et al., 2007). Studies suggest that individuals do not assess outcomes in absolute terms; instead, they look at outcomes in relative terms, i.e., they evaluate them by comparing to reference value. Besides, loss aversion is a regularly found phenomenon in individuals, which means that losses inflict more pain compared to the delight associated with an economically similar amount of gains (Tversky and

Kahneman, 1991). This notion of loss aversion can be helpful to understand how incentives such as rewards and punishment work.

The study also investigates the impact of leaders' choices on members' level of happiness. We argued that a principal might not always be looking for low-risk investment, so looking at the level risk aversion of decision-maker perhaps is not the best way to evaluate the outcomes. Instead, we should judge the decisions by how close they are in line with the principal's interests. We expect that recipients would be happier when the misalignment between a leader's decision and their (active and or passive members in the group) preference is smaller. We propose to analyse this phenomenon under different accountability conditions. We also predict that accountability with punishment will produce socially desirable outcomes.

We consider the following hypotheses:

H2: Subjects are happier when their leader chooses the option they prefer.

H3: Accountability through punishment is more effective in achieving socially more desirable results.

As discussed, performance evaluation systems can be divided into two types: process accountability and outcome accountability. Researchers also identified a third accountability regime in the form of hybrid accountability system (Chang et al., 2017). A hybrid accountability system allows both the outcome of the decision and the process that is followed to make the decision to be incorporated in the evaluation process. The debate surrounding process versus outcome accountability has enriched the accountability literature by identifying the pros and cons of both approaches. These two systems follow different approaches when it comes to specifying two things: whom the accountable persons answer to and for what (Frink and Klimoski, 2004). The later gets much attention in research works championing one of these two accountability systems. Although the literature on process versus outcome accountability is already rich and growing, there has been little research in experimental economics focusing on the preferences of the audience in terms of choosing an accountability system. Most of the works

attempting to understand this issue run a horse race among the accountability regimes. Our study, on the other hand, proposes to check whether the evaluator is more inclined to choose just one of the systems or a combination of both systems (i.e. a hybrid system of performance monitoring).

The proponents of process accountability suggest that this system commits less to sunk costs, improves calibration and judgement, increases the desire to understand the decision context more thoroughly, and makes the decision-maker more attentive and alert (see Simonson and Staw, 1992; Siegel-Jacobs and Yates, 1996; De Dreu et al., 2006; Brtek and Motowidlo, 2002). Scholars preferring process accountability over outcome accountability argue that the quality of decision does not always show up in the outcome, i.e. a good quality decision may lead to suboptimal outcomes, and therefore the decision should not be judged based on the outcome of the decisions. In outcome accountability systems, luck plays a very significant role in performance evaluation, which can put the decision-maker under more stress. This, in turn, can deteriorate the quality of decisions. According to Schweitzer et al. (2004), outcome accountability systems are prone to encourage corner-cutting behaviour since how the job is done would be less of concern in this regime. Since outcome-based performance evaluation systems often reward the lucky and punish the unlucky, it can also prompt backlash within the organisation as employees might perceive the system to be unfair (Baker et al., 1994). Despite all the positives associated with process accountability, the system is often found to be challenging to implement (Tetlock et al., 2013). In addition to that, process accountability might infuse to nonobjectivity into the employee evaluation process in an organisation. To eradicate biases that are associated with outcome metrics, process accountability system may create new biases (Patil et al., 2017). So, it is not a surprise that most of the organisations employ a combination of process and outcome accountability systems (Hochwarter et al., 2007; Tetlock and Mellers, 2011b).

The study does not directly examine the effectiveness of different accountability systems like some of the previous studies nor does it directly ask subjects about their preferences of accountability systems (see Patil et al., 2017). Our study,

however, allows us to check the motivations behind active members' reward and punishment decisions. Moreover, by observing the reward and punishment behaviour of the subjects, we can gain insight into their preferences of the accountability systems. In the experimental set-up, the active member knows whether the leader chose the option he/she (active member) preferred as well as the outcome of the decision before making a reward or punishment decision. If reward decisions or punishment decisions are biased toward the outcome of the decision rather than the quality of the decision than we can say that active members' evaluation process is more in line with an outcome accountability system. Conversely, if the members are basing their reward and punishment decisions solely on the quality of the decisions regardless of the outcome, then it can be suggested that active members are following a process accountability regime. A third possibility is a combination of both systems, i.e. a hybrid accountability system. We suspect that since reward decisions are more likely to be altruistically motivated, these decisions are likely to be less efficient. So, reward decisions are more likely to be associated with an outcome accountability system. Punishment decisions, on the other hand, are expected to be based on both the outcome and quality of decisions. We propose to explore the link between accountability system preference and use of accountability mechanisms.

The following hypothesis is considered:

H4: Unlike punishment decisions, reward decisions are less likely to be aligned with a hybrid accountability system due to outcome bias.

5. Results and Discussions

5.1 Leaders choices and members' preferences

Some general description of decision choices in all treatments is presented at the beginning. Figure 1 reports an overview of the choices made by the leaders and other members of the group. The leaders showed a preference towards safe options in most treatments. The general pattern, however, is not observed in treatment *reward* and *rewardFixed*. These two are the only treatments where

leaders are neither risk-neutral nor risk averse. In all other treatments, the leaders showed an inclination towards the safe option. In *reward*, the mean proportion of risky choices is 0.56, which is significantly higher than 0.50 (p-value <0.05, based on a one-sided binomial test). Here the value 0.50 is the reference point associated with risk neutrality. In the *rewardFixed* treatment, leaders' exhibited risk-seeking behaviour, and the magnitude is even higher (with a value 0.60) compared to the *reward* treatment. We can reject the hypothesis that leaders are risk-neutral for this treatment where the p-value <0.001 (based on a one-sided binomial test). In other accountability treatments, the leaders are considerably more risk averse as shown in Figure 1. The proportion of risky choices in four treatments involving punishment ranges between 0.41 to 0.44, and in every case the value is significantly lower than 0.50 (based on a one-sided binomial test). It appears that a viable presence of the threat of punishment forces leaders to be conservative in their choices.

Insert Figure 1 from Appendix II here

Table 1 presents the p-values representing the significance in differences among leaders' decision behaviour in terms of the level of risk taking in different treatments. The significance levels are based on a two-sided Mann Whitney U-test (MWU test). The results are consistent with the notion that reward only contracts encourage risk-taking behaviour in the context of decision making for others. It appears that this environment (rewards) encourages the leaders to take more risk despite being responsible for other members' payoff, in particular, the *rewardFixed* treatment where leaders took the highest amount of risk. Almost 60 per cent of the choices made by the leader in this treatment is risky. The p-values suggesting whether the frequency of risky choices by the leader is materially different from frequencies in other treatments. Leaders' decision behaviour in *rewardFixed* treatment is significantly different from the behaviour associated with other treatments except for the *reward* treatment. The only p-value for which the difference is insignificant and greater than 0.001 is associated with this pair (i.e. *reward* and *rewardFixed*). It can be noted that approximately 56 per cent of the choices in the *reward* treatment are risky choices and this value is significantly

different from the values associated with other treatments, accountability treatments involving punishment as well as no accountability treatments. The study also explores the impact of time on leaders' risk attitudes (see Table 2). It seems that the attitude toward risk of the leaders did not change (significantly) in the second half of the game (last 12 periods) when compared with the first half of the game.

Insert Table 1 & Table 2 from Appendix II here

We present the comparison between leaders' choices and members' preferences in Figure 1 as well (also see Table 3 for corresponding p-values). We find that in none of the treatments, members of the group (there are two members in each group) demonstrated any definite inclination toward the risky choices, i.e. members were either risk-neutral or risk averse. Members' showed the highest-level risk aversion in the *punish* treatment. The level of risk aversion associated with members' choices for this treatment is significantly lower than other treatments, as shown in Table 3 (p-values are based on MWU test). The analysis reveals that the overall difference is statistically insignificant, with a p-value of 0.6680 (MWU test). When we analyse the treatments, we find that in *reward* and *rewardFixed*, leaders are on average significantly more risk-seeking than the individuals for whom they are making decisions (the MWU test p-values are 0.0705 and 0.006 respectively). The misalignment is most severe in *rewardFixed* treatment. Moreover, the null hypothesis that leaders did not take more risk compared to the level suggested by the active members in their groups can be rejected at 1 per cent for *reward* and *rewardFixed* treatments (untabulated results). We see the opposite scenario in the *pusnishFixed* and *plainFixed* treatment where the leaders are exhibiting a slightly higher degree of risk aversion compared to their recipients. In other treatments, leaders are either more risk averse or equally risk averse compared to the members of the group.

Insert Table 3 from Appendix II here

Figure 2 shows leaders' choices (percentage of risky choices) over the 24 periods of the experiment, and Figure 3 shows the 3-period averages associated with

leaders' choices. Leaders in *reward* and *rewardFixed* treatments consistently took more risk compared to their counterpart leaders in other treatments. The proportion of risky choices made by leaders for those two conditions generally stayed above the proportion of risky choices associated with leaders in other treatments throughout the life of the experiment. This pattern of behaviour is suggestive of the fact that holding decision-makers accountable through rewards increase their risk-seeking behaviour.

Insert Figure 2 & Figure 3 from Appendix II here

Table 4 presents the p-values representing the significance in differences among members' preferences in terms of the level of risk taking in different treatments. The significance levels are based on two-sided MWU test. unlike their leaders, principals (i.e. members) in *reward* and *rewardFixed* did not prefer the risky option more frequently compared to members in other treatments. Table 2 shows that leaders' attitude toward risk did not change (significantly) in the second half of the game (last 12 periods) when compared with the first half of the game. The same pattern, however, was not observed in members' behaviour, i.e. there was a time effect (see Table 5). The members were generally more risk averse in the second half of the game at various level of significance. In 6 out of the total of 8 treatments, this pattern emerged.

Insert Table 4 & Table 5 from Appendix II here

We check the influence of stake level on leaders' risk attitudes. Studies such as Binswanger (1980), Kachelmeier and Shehata (1992), and Holt and Laury (2002) have demonstrated the impact of stake level on risk aversion. As expected, the stake level has a significant influence on leaders' choices. Figure 4 shows decision choices for the risky option across all treatments. The stake effect is very prominent for most of the treatments, with an increase in risk aversion. Leaders picked the safer option much more frequently for the highest stakes (i.e. when X =10). Risk neutrality can be rejected at various significance levels for *plain, punish, rewpun and plainFixed*, signifying that increases in stake levels lead to a greater magnitude of risk aversion. It appears that leaders in *reward* treatment took less risk for high

stake prospects. The stake effect is also observed in the behaviour of active and passive members of the group, as depicted in Figure 5. The average level of risk aversion among members of the group (including the leader) is found to be the same for the high-stake prospects.

Insert Figure 4 & Figure 5 from Appendix II here

Table 6 shows results from random-effects probit regressions. Here the dependent variable is the leader's choice of prospect and is a binary variable (takes the value 1 if the leader chooses the risky option and 0 otherwise). Since each leader makes 24 decisions, errors are clustered by subject ID for every specification in the table. This takes care of the fact that different decisions by the same participant do not create independent observations.

As discussed earlier, there are six treatments where the decision-maker is accountable for his or her decisions, i.e., the final payoff is affected by someone else's (the active member in the group) decision. In two treatments (*plain* and *plainFixed*) leaders are responsible for somebody else's payoffs as well as one's own, but they are not accountable for the choices they make. Regression results in Table 6 also confirm the fact that in the *rewardFixed* treatment and *reward* treatment, leaders were more risk-seeking. The corresponding p-values associated with these two treatment dummies are 0.004 and 0.032, respectively. The result is suggestive of the fact that designing a contract for a decision-maker where accountability is incorporated in the form of reward can diminish the effect of accountability. Being accountable does not necessarily increase risk aversion relative to the decision situation where decision-makers are only responsible for the decision they make.

Insert Table 6 from Appendix II here

Among control variable stake level appears to be significantly associated with leaders' choices. Leaders acted more cautiously when the decision involves higher stakes. Leaders also chose the safe option more frequently when the safe option has a higher expected value (ex-ante). Similarly, they picked the risky option more frequently when the risky option has a higher expected value (ex-ante). We use two

dummy variables in the regression models as control variables in the forms of UP and DOWN. UP (DOWN) is associated with the prospect where the safe (risky) option has a higher expected value. The effect of UP and DOWN on leaders' choice, however, varied across treatments. In some treatments, the relationship is insignificant. In addition to some standard demographic variables such as age and gender, we added different personality characteristics as controls to our analysis. We used the Cognitive Reflection Test or CRT to measure subjects' level of cognitive ability (Frederick, 2005).[25] We also included the subjects' level of risk aversion and narcissism as explanatory variables in the regression specifications.

Table 7 shows results from random-effects probit regressions where the dependent variable is binary and takes the value 1 if the member prefers the risky option and 0 otherwise. Errors are clustered by subject for every specification, considering that different decisions by the same participant do not create independent observations. The dummy variable associated with treatment *reward* found to be significant, but only marginally. There is no strong indication in regression results that members preferred significantly more or less risk in a specific accountability treatment. Once again for Up (Down) prospects, participants are considerably more (less) risk averse. We have seen a similar effect of variables UP and DOWN on leaders' decisions. We observe the stake effect on members' preferences as well, i.e. they are more risk averse when the stakes are high. Members prefer riskier choices if they have a more significant cumulated wealth. None of the demographic and personality characteristics included in our analysis, including gender, narcissism, and CRT, is significant in the regression. Perhaps these variables are more associated with leadership roles.

Insert Table 7 from Appendix II here

[25] The CRT is associated with general measures of intelligence along with essential decision-making aspects in the form of preferences for time and risk (Frederick, 2005; Oechssler et al., 2009) and reasoning levels (Branas-Garza et al., 2012).

5.2 Desirable outcome: Match between leader's choice and member's preference

Pahlke et al. (2012) show the effect of accountability in the form of lower risk aversion, i.e., accountability lowers risk aversion, as in our condition, *reward* and *rewardFixed*. They let agents explain their decisions to the principal in person at the end of the experiment. Accountable agents (i.e. decision-makers) in their study choose riskier options more frequently compared to a condition where agents make decisions without the need to justify them, as in our condition, *plain* and *plainFixed*. Lefebvre and Vieider (2013) show that accountability increases risk aversion. We, nevertheless, do not observe any increase in leaders' level of risk aversion due to accountability compared to situations where leaders make decisions for others but are not accountable (i.e., there is no payoff effect). The absence implies that the effects of accountability can be contingent on subtle specifics of the accountability manipulation.

Chart 3: The variable *match*. For each decision taken by the leader, there are two observations associated with the variable match: one from the perspective of the active member and one from the perspective of the passive member.

Leader's choice	Active member's preference	Passive member's preference	***match***
Safe	Risky		NO
Safe	Safe		YES
Risky	Safe		NO
Risky	Risky		YES
Safe		Risky	NO
Safe		Safe	YES
Risky		Safe	NO
Risky		Risky	YES

The critical issue in the principal-agent relationship is the misalignment of goals that different parties are pursuing and the costs that the principal incurred to alleviate this problem. To induce efforts from the decision-makers (who are acting on behalf of others), incentives are often combined with responsibility. An underlying problem associated with this practice is that it is generally challenging to design compensation packages that correctly align the decision-maker's interests with those of the principals (i.e., other stakeholders who are affected by the

decisions). Many blamed the players in the financial industry for taking a disproportionately high level of risk on behalf of investors following the recent global economic meltdown. According to Rajan (2006), advances and trends in the finance sector before the meltdown —for instance, added layers of financial management and new multi-layered financial products—have aggravated the problem.

We pay attention to this issue and construct different compensation contracts for the decisions-makers to check which one produces the best results. The preliminary analysis shows convex like designs (*reward* and *rewardFixed* in our context) encourage risk-taking. However, we still do not know whether the amount of risk taken is in line with the principals' interest. Excessive risk is often not desirable (as most economic agents are risk averse, Ceteris Paribas), but if the risk taken by the decision-maker is too low, then the principal will not be happy either. In this section of our analysis, we check which accountability manipulation(s) bring the decision-maker's behaviour closer in line with the principal's interests. Lefebvre and Vieider (2013) show that incentive structures like limited liability lead to lower risk aversion than would be optimal from the principal's point of view, which is consequently mitigated by accountability manipulation.

To describe pro-social behaviour, we resort to a variable that is called *match*. A match happens when the leader chooses, and the member (active or passive) of the group prefers the same option (risky or safe). Therefore, for every decision, for example, concerning the member, there are two combinations for a match or mismatch. Chart 3 describes the conditions associated with the variable *match*.

Insert Figure 6 from Appendix II here

Figure 6 presents the frequencies associated with the number of times in proportions (out of 1.00) there were matches between leaders' choices and members' preferences. The study initially (before incorporating the costs to the analysis) checks whether there was any treatment effect on this phenomenon (frequency of matches or mismatches). Figure 6 shows that *punish* and *punishFixed* treatments are the most successful in terms of avoiding mismatches. These two

treatments resulted in the highest numbers of matches between a leader's choice and member's preference. Figure 7 and Figure 8 also reveals the same pattern, i.e. treatment *punish* and *punishFixed* to be associated with more matches. Punish-only environment seems to force leaders' to be risk averse, which is also what other members of the group typically wanted. We present the p-values in the p-value matrix (see Table 8, where p-values are based on the MWU test). We observe that the effect is somewhat reduced in *punishFixed* treatment. The reason is perhaps that a significant component of the leader's payoff in this treatment is fixed. It is important to note that rewards and punishments are costly and therefore we need to incorporate the costs associated these accountability tools to get a more in-depth insight into the effect of different accountability set-ups (we do that in one of the following sections).

Insert Figure 7 & Figure 8 from Appendix II here

Insert Table 8 from Appendix II here

As a robustness check, we conduct a multivariate analysis, which confirmed the results. We present the results in Table 9. We run random-effects probit regressions where the dependent variable is a dummy taking the value one if there is a match and zero otherwise. In model 1 of Table 9, only accountability treatments are included as dummies, and in Model 2, we add other explanatory variables. We find that the coefficient on treatment dummy *punish* is significant and positive in both the models suggesting that the punishment only environment produced a significantly (p-value < 0.01 in both cases) higher percentage of matches. The coefficient on *punishFixed* is significant in Model two, also supporting the conjecture that punishment is more likely to reduce the misalignment between the choices of the agent and principals' preferences. According to Gächter et al. (2008), punishment as a tool is more effective and efficient to induce actions that lead to more social benefits. Our results are in agreement with that conclusion; punishment is more effective in producing socially more desirable outcomes. Among control variables, UP and DOWN found to be statistically significant, indicative of the fact that leaders and members are more likely to choose the same option, risky or safe, when the option has a higher expected value.

In Model 3 and Model 4, we use dummy variables capturing different dimensions of the experimental design. First of all, we use the dummy variable accountability, which takes the value 1 is the observation is associated with an accountability treatment and 0 otherwise. Secondly, we have a binary variable fixed which indicates whether or not the observation is associated with a treatment where the agent initially receives a fixed amount in every period. We also include two dummy variables specifying the nature of the contract and the accountability mechanism: rewardOnly takes the value 1 if the observation is associated with treatment *reward* or *rewardFixed* and punishOnly takes the value 1 if the observation is associated with treatment *punish* or *punishFixed.* Consistent with the results in Model 2, we find that accountability through punishment leads to more matches. The variable punishOnly is statistically significant (p-value < 0.01) in both specifications. We do not observe any significant influence of the dimensions fixed and accountability on the variable *match*. Among control variables, UP and DOWN found to be statistically significant in predicting a match between leaders' choices and members' preferences.

Insert Table 9 from Appendix II here

5.3 Members' happiness: Match between leaders' choices and members' preferences

In every treatment, active and passive members could indicate their level of happiness with a leader's decisions. They were asked to indicate whether they were "very happy" or "very unhappy" (on a Likert scale of 9 to 0 where 9 is very happy) with the choices. This information, however, was not communicated to the leader, and this did not affect the leader's payoff. Leaders were aware of happiness ratings. There is evidence that the choice of safe options is more likely to make a good impression on members of the group. Table 10 presents some non-parametric results associated with members' happiness ratings. The overall mean happiness ranking related to safe choices is significantly higher (p-value = 0.000 based on a two-sample t-test with unequal variances) than the mean associated with risky

choices. The pattern does not change when the subject is an active or a passive member of the group, though active members appear to be more critical in evaluating leaders' decisions. This ranking is not inverted with respect to treatments, i.e., in every treatment, a safe choice is much more likely to be associated with a higher satisfaction ranking. Overall satisfaction rankings vary. It seems that members are even happier when active members can punish leaders. Punishment environment may produce results that are more desirable to the group members. This observation could also be due to the sense of feeling that they are in a democratic environment. The happiness level drops with the complexity of the contract. The happiness level is the lowest for treatments where the active member can punish as well as reward the leader. The study checks whether there was a time effect on members' level of happiness. As reported in Table 11, the time effect on happiness appears weak.

Insert Table 10 & Table 11 from Appendix II here

The study then focuses on the variable *match* and its relation to members' level of happiness. We often do not observe the subjective conditions of mind, but this can be quite significant in personal communications and as a determining factor of economic activity. Several laboratory experimental studies showed that individuals do not always choose to maximize their monetary reward, seemingly being inclined to non-monetary considerations (Konow and Earley, 2008). There is a budding interest in the economics discipline trying to explain the drives for such behaviour. The study attempts to identify any non-monetary influence on group members' level of satisfaction associated with leaders' choices.

Insert Table 12 from Appendix II here

The variable that emerged as being profoundly influential in determining members' level of happiness is match or mismatch between the leader's choice and the corresponding group member's preference regarding the choice (see Chart 3). In the regression models, we use a dummy that takes the value 1 if there was a match and 0 otherwise. Table 12 reports results associated with non-parametric tests concerning this issue. The level of happiness of the members goes up by an

astonishing 95 per cent on average when there is a match compared to the happiness level for a mismatch (i.e. when a leader-member pair choose different options). There is overwhelming evidence that the influence of *match* is much more substantial than the riskiness of choice. The members prefer a safer choice, but the impact is only a 15.55 per cent (on average) increase in their level of happiness. The pattern remains statistically highly significant across all the treatments and true for both active and passive roles. The regression results confirm that this relationship holds after controlling different game-specific and personality-specific variables that may influence members' level of happiness. The p-value associated with this variable is always significant (p-value = 0.000 for all the treatments).

We conduct a multivariate analysis in order to check whether the association between happiness and *match* is robust after controlling for other variables that can influence subjects' state of minds. The results associated with a random-effects ordered logit regression are presented in Table 13.[26] In different specifications, we use the members'/recipients' level of happiness or satisfaction with each choice made by the leader of the group as the dependent variable and run regressions using some independent variables. As before, we cluster errors at the participant level. In all treatments, members of the group observed the decision-maker's choice and are asked to indicate whether their level of happiness with the decision (on a scale 0 to 9). Although this rating was not incentivized, it may nevertheless indicate the degree to which leaders revised their choice to the generally accepted one, or adequately sensed which choice would be taken more favourably. Since satisfaction assessments were not transferred to the leaders and had no payoff implications, members (active and passive members) had no strategic motives to lie about their preferences.

As predicted, the variable that emerged as having the highest level of influence on members' happiness level is *match*, which is statistically highly significant across all specifications (i.e., in every treatment, the p-value is < 0.0001). This variable is economically much more significant compared variables such as lag

[26] The results from random-effects GLS regressions are very similar to the results we presented in Table 13.

cumulated payoff and even the leader's choice. It is found that the variable *Stake*Leader'sChoice* is significant implying that members are less happy when the leader chooses the risky option for high stake prospects, i.e., a safe amount for high stake prospects is generally not perceived as satisfactory. Members are not concerned about the choice made by the leader for the mid and low prospect, as indicated by the fact that the variable Leader's Choice is not significant for any of the specification. Risk-seeking behaviour deems to be more acceptable as long as the decision under consideration does not involve a high-stake prospect.

Insert Table 13 from Appendix II here

5.4 Reward and Punishment as costly incentive tools:

In the accountability treatments (six out of eight treatments), every group has an active member who can punish and (or) reward the leader. Punishment and reward are costly, and the cost is borne equally by both active and passive members of the group (but the active member bears the responsibility to punish or reward).[27] Punishment or reward can be imposed after the result of the decision is known to everyone in the group. Figure 9 summarizes the reward-punishment behaviour of active members in different groups. We observe that active members are more likely to use the reward as a tool to incentivize the leader than punishment. The is an indication that active members use the option to punish more efficiently and more cautiously. We notice that principals used the accountability tool (i.e. reward or punishment) more frequently in situations where agents' payoff is fixed. The number of times active members used either punishment or reward is much higher in treatments where the leader's payoff contract involves a fixed component (i.e., a significant portion of leaders' payoff in this treatment is fixed). It seems that active members used incentive tools more frequently to prod the decision-making behaviour of the leader in the desired direction when leaders are less uncertain about their payoffs in each period. The observation is unsurprising and in line with

[27] In treatment *rewpun* and *rewpunFixed*, the active member can also choose to do nothing; they do not have to punish or reward.

the agency theories. We expect principals to incur more agency costs to rein in agents enjoying the bliss of a partly deterministic environment. In the game, passive members of the group can also indicate whether the leader should be punished or rewarded by the active member. Figure 9 also shows the preferences of passive members regarding the use of accountability mechanisms. The pattern is almost identical to that of active members' choice. They chose the option more frequently "Do nothing", but Passive member's choice does not (as they are playing a passive role) affect the payoff of the leader.

Insert Figure 9 Appendix II here

We suggest that active members rewarded more and punished less due to altruism. The notion that individuals can be altruistic has been used to clarify generous actions in many socio-economic set-ups in the economics literature. Altruism can also explain why people contribute to different types of public goods when zero contribution is optimal (Becker, 1974). It is suggested that reward decisions, unlike punishment decisions, can be motivated by altruism and for that reason, we observe rewards more frequently compare to punishments. In our experimental set-up, reward and punishment decisions are made after the outcome is known. We assume that if there is a match (mismatch) between the leader's choice and member's preference, the decision would be perceived as a high (low) quality decision and vice versa. So, following a mismatch, we do not expect to see the active member to reward the leader. On the other hand, we do not expect to see the leader to be punished following a match. However, altruistically motivated principals may ignore the quality of the decision when the outcome is favourable, which can lead to a disproportionately high number of rewards.

5.5 Is punishment more effective than reward in generating socially more desired outcomes?

Our main finding concerning this issue is that accountability with punishment brings agent's behaviour closer in line with the principal's interests.

Previous analysis suggests that recipients (active and passive members) in a group are happiest when the leader chooses the option they prefer. We argue that from the point of view of the active and passive member, a match is the desirable outcome since there is a strong and positive association between their level of satisfaction and a match. Now the question is whether this is achieved at a significantly higher cost. The literature on punishment and cooperation suggest that punishment is more effective in sustaining cooperation. Studies suggest that the costs of punishment are often justified since it leads to better outcomes. For example, Sigmund (2007) show that despite the costs, punishment can sustain costly cooperation. As per Boyd et al. (2003), the costs associated with punishment drops significantly after cooperation is established since it is rarely required afterwards. Therefore, any pressure that the punisher might be facing would become strengthless at this point and would be outweighed by the benefit that punishment brings to the group.

We present the statistics associated with the frequency of reward and punishment for each treatment (where applicable) in Figure 9. We keep the costs at the same level. So, this shows the cost of disciplining the behaviour of the leader. The cost is much lower in treatments involving punishment. Treatment *punish* produced the lowest cost figure as the active members in this treatment used the option only with a frequency of 2.08 per cent, which is statistically significantly different from the frequencies associated with every other treatment. And yet this treatment produced the highest number of matches between leaders' choices and other members' preferences. It seems that the presence of a stick rather than the use of the stick drives the result. Figure 7 (24 periods) and Figure 8 (three-period average) presents the percentage of matches for different treatments. We observe that punishment only treatments, *punish* and *punishFixed* consistently produced more matches between the leader's choices and recipients' preferences over the life of the game.

The findings are in line with Gächter et al. (2008); in a different set-up, it was shown that the possibility of punishment in public goods game leads to the best outcomes. They observe that punishment not only increases cooperation but also

makes groups and individuals better off in the long run because the costs of punishment become negligible and are outweighed by the increased gains from cooperation. In our case, we argued that the socially beneficial outcome from the recipients' perspective is a match between the choices of the leader and members of the group.

Among the fixed salary treatments (the leader initially receives a fixed salary) *punishFixed* produced the highest *match* figure with 61.63 per cent of matches. This percentage is significantly higher and different from the figure associated with *rewardFixed* treatment (with a p-value of 0.031) where we observed a match in 55.38 per cent cases. We expected the costs incurred by the group members' (active member decides that) in all fixed treatments to be higher (compared to variable salary treatments) as the agency problem, stemming from the fact that a significant portion of the leader's salary is fixed, is more severe in this environment. Active members tried to deal with this issue by using the available tool, regardless of whether it is reward or punishment or the combination of both, more frequently. Nevertheless, we observed the costs to be the lowest in the treatment where punishment is imposed on the leader in the form of a deduction from a fixed amount. The cost is significantly lower (p-value is 0.000) than the cost associated with treatment *rewardFixed*.

5.6 Accountability mechanisms and accountability systems

5.6.1 Difference between Expected payoff and actual payoff as a yardstick in an outcome accountability system

In addition to an alignment or misalignment between leaders' choices and members' preferences, the study analyses the per period payoff resulting from leaders' choices. One purpose is to understand the motives behind the reward and punishment decisions of recipients, especially the active member, in a group in accountability treatments. We calculate the net payoff (NPO henceforth) for the recipients in a group for every period in order to check whether the decision

outcome in a given period is favourable or unfavourable.[28] In our context, NPO is the difference between the actual per period payoff associated with a principal (i.e. members of the group) and the agent (i.e. the decision-maker) and the expected payoff in the period under consideration. In order to calculate expected payoff, we assume that there is a 50 per cent chance that the leader (the decision-maker in the group) will select the risky option and a 50 per cent chance that the leader will select the safer option — we than use the corresponding average values to compute the expected payoffs for the members. Table 14 (Panel A) shows the expected payoff calculation for a member in every period. The payoffs for the active and passive members are always the same, but the leader might end up receiving different payoffs. The regression analysis discussed in this section includes the variable NPO, which is the difference between the actual payoff in a period and the corresponding expected payoff.

The paper also touches upon the issue of the leader's ability to generate economic benefits for the principals under different accountability and non-accountability conditions. Panel B of Table 14 presents the mean NPO associated with the group members in different treatments. Accountability treatments generally leads to significantly more economic benefits for the members of the group compared to the benefits in the non-accountability treatments (p-value is 0.000 based on two-tailed MWU test). The mean NPO (before adjusting for costs) for all the accountability treatments is ECU 0.52 higher than the mean NPO associated with non-accountability treatments.[29] Among the non-accountability treatments, *plain* resulted in the lowest amount of NPO with a value ECU 0.043. We also calculate the standard deviation of members' NPO and calculate the ratio between mean members' NPO and the corresponding treatment standard deviation. As shown by Table 14, the higher ratios are mainly associated with

[28] We focus on net payoff or NPO rather than actual payoff since actual payoff in a given period is heavily influenced by the stake level we use for that period. A favourable outcome in a period may be lower than the unfavourable outcome in another period.

[29] Every time the active member punishes or rewards the leader, active and passive member loses ECU 0.60 each. The variable NPO is the net payoff before adjusting for reward or punishment costs.

accountability treatments. Non-accountability treatments are generally performed poorly in terms of economic value creation for the group member.

Insert Table 14 from Appendix II here

5.6.2 Accountability systems, outcome bias, and reward and punishment decisions

Accountability guidelines usually regulate how a decision is evaluated and for that reason are important features of the society. In a process accountability regime, the decision-maker is judged based on how he/she arrived at the result. In an outcome accountability regime, on the other hand, the decision-maker will be judged mainly based on the results he/she produces (Patil et al., 2014).

If the accountable leader is punished or rewarded solely based on the results he generates for the group, then the audience (to whom the leader is accountable) is following the outcome accountability system. Alternatively, if decision quality is the only thing that affects whether or not the agent is punished or rewarded in accountability treatments, then we have reasons to believe that the chosen system is process accountability. When both approaches are combined, we get a hybrid accountability system.

In order to test H4, we choose the variable *match* as a proxy for decision quality in a process accountability system. A match implies that the decision is perceived as a high-quality decision. In four accountability treatments, *reward, rewpun, rewpunFixed,* and *rewardFixed,* the active member of the group can reward the leader. In *punish, rewpun, rewpunFixed,* and *punishFixed*, the active member can punish the leader of his/her group. In *rewpunFixed* and *rewpun*, the active member can punish as well as reward the leader. Panel C in Table 14 shows that leaders are more likely to receive a punishment when the NPO is negative, and there is a mismatch, and more likely to get a reward when the NPO is positive, and there is a match. Nevertheless, there were an overwhelming proportion of reward decisions that took place even after a mismatch. The total number of punishments is 75, and the total number of rewards is 182. We find that there were only 16 instances when

the active member punishes the leader following a match and 68 instances when the active member rewarded the leader following a mismatch.

These are evidence that the evaluator, i.e. principal, was taking her reward decisions mostly on the basis of decision outcomes. A similar pattern emerges when we analyse the reward and punishment preferences of the passive member. There were only 11 cases when the passive member suggested that the leader should be punished following a match between the leader's choice and passive member's preference and 46 cases when the passive member preferred a reward even after a mismatch.

In our three-person micro organisation, evaluators are not directly asked to evaluate agents' decision based on the outcome or the process of the decision. Both pieces of information are available to them before they can sanction a reward or impose punishment. Our experimental set-up is reminiscent of a hybrid accountability system one might observe in an organisation. The question is whether the evaluator uses both pieces of information to evaluate the agent. When we look at the reward decisions separately, there is a noticeable bias toward the outcome of the decisions made by the leader. The outcome bias arises when individuals evaluate others' decisions and in doing so, put an untenable weight to their results (Baron and Hershey, 1988). This is true for both active and passive members of the group.[30] Punishment decisions, on the other hand, appear to incorporate two aspects of a decision, quality and outcome. It seems as if the active member is operating in a hybrid accountability system while making punishment decisions because they are not falling prey to outcome bias. Punishment decisions also appear to be more efficient as the number of punishments is much lower compared to the number of punishments. It can be suggested that in the absence of punishment, an organization would fall prey to outcome bias when it comes to the evaluation of the management decisions.

[30] Passive member can suggest whether or not the leader should be punished or rewarded, but this does not affect the payoff of the leader. Nor is that suggestion communicated to the active member of the group.

The regression results confirm that active members are following a mixture of both process and outcome accountability templates while imposing punishments and falling prey to an outcome bias while sanctioning rewards. In order to test H4, we run panel data random-effects probit regressions using reward and punishment as dependent variables in the regression models. Table 15 presents the regression results. Since each leader makes twenty-four decisions in twenty-four periods, the panel variables for the regression are unique subject ID and period.

Our main interests are the variables that regulate reward and punishment decisions of the evaluator. We are also interested in passive member's preferences regarding punishment and reward. Regressions are run separately for active and passive members. We run regressions with observations associated with the active members (Model 1 and 2 in Table 15) and observations associated with the passive members (Model 3 and 4 in Table 15). In each treatment except the *reward* treatment, there were 12 groups and therefore 288 decisions. In treatment *reward*, there were 264 decisions. In Model 1 of Table 15, the dependent variable is an indicator variable which takes the value 1 if the active member rewards the leader following the outcome of the decision is known and zero otherwise. In Model 2, the dependent variable takes the value 1 if the active member decides to punish the leader after the decision outcome is known and zero otherwise. Same goes for the other specifications that used observations associated with the passive members. However, instead of the actual reward and punishment, the dependent variables are passive members suggestions regarding whether or not the active member should reward (Model 3) or punish (Model 4).

In every specification in Table 15, decision outcome emerges as a statistically significant variable in predicting reward as well as punishment. We use NPO as a proxy for the quality of decision outcome. The signs associated with the coefficients are consistent with our conjecture. The outcome of the accountable leader's decision is, however, not the only significant variable in predicting the likelihood of punishment. The variable *match* also plays a significant role in the evaluation process. This variable, as predicted, is negatively associated with the likelihood that the leader would be punished. Reward decisions, in contrast, are not

significantly associated with the variable *match*, implying an outcome bias in reward decisions. A similar pattern emerges when we look at the reward and punishment suggestions made by passive principals. We use an interaction term between variables *match* and NPO in every regression specification (see Table 15). The interaction term in Model 1 is negative and significant, suggesting that when the outcome is favourable active members are willing to reward the decision of the leader even if they did not initially agree with the decision. In other specifications (that predict the probability of punishment), the interaction terms are not significant, which again is indicative of the fact that punishment decisions are not biased toward the outcome.

Outcome bias was much less pronounced in subjects with higher cognitive ability. We used the CRT as a measure of cognitive ability. The CRT is associated with general measures of intelligence along with essential decision-making aspects in the form of preferences for time and risk (Frederick, 2005; Oechssler et al., 2009) and reasoning levels (Branas-Garza et al., 2012). The regression results suggest that subjects with the highest CRT score (a score of 3) rewarded less frequently and they are more likely to reward when it could be justified, i.e. when the leader chose the option they preferred. On the other hand, subjects with the lowest CRT score (a score of 0) rewarded more and did so solely on the basis of the outcome of the leaders' choices.[31]

The random-effects probit regression results based on passive member preference data show similar patterns. The outcome of the decision as proxied by NPO and whether the choice is in line with the passive member's preference as proxied by *match* are found to be significantly associated with punishment preferences. As reported in Table 15, NPO is significantly and positively associated with reward preferences of the passive member, but the variable *match* is not. Like active members, passive members too made their punishment suggestions based on profit and quality of a leader's decisions, i.e. punishment suggestions are in line with hybrid accountability system.

[31] Untabulated results based on a two-sided MWU test.

Insert Table 15 from Appendix II here

5.7 Can outcome bias contribute to more risk-seeking behaviour?

Bertrand and Mullainathan (2001) showed a strong association between observable luck and CEOs' compensation level. They found that CEOs are often rewarded for luck rather than performance. Holmstrom (1979) argued that tying luck to agent's compensation can encourage the agent to take more risk. Our experimental results are also in line with this prediction. Subjects in our experiment took less risk averse decisions in treatments where they can only be rewarded without the possibility of punishment. There is a strong indication that lucky decisions are more likely to be rewarded. In four treatments involving rewards, there were 68 decisions in total that were rewarded following a mismatch. Approximately 84 per cent (57 out of 68 decisions) of those were risky choices. Table 16 presents the results. We suggest, since outcome bias is prominent in principal's reward decisions and it is generally a risky decision that can change the mind of the principal (to whom the decision-maker is accountable), reward-only arrangements can encourage risk-seeking behaviour in the agent. As can be seen in Figure 1, leaders chose the risky option more frequently in treatment *reward* and *rewardFixed.* Figure 1 shows that for treatments involving punishment, the tendency is inverted including *rewpun* and *rewpunFixed*, where the active principal can also reward. One might wonder why leaders did not take more risk in other treatments involving rewards, i.e. *rewpun* and *rewpunFixed*. We believe this due to the presence of a punishment threat in those two treatments. Although outcome bias was prevalent in principals' reward decisions, especially in treatment *rewpunFixed*, leaders did not take a significantly higher amount of risk in an environment where the principal can punish as well.

Insert Table 16 from Appendix II here

5.8 Inconsistency between principals' subjective ratings of agents' decision and costly reward decisions

We present some statistics concerning recipients view towards leaders' choices and corresponding outcomes associated with the choices (please see Figure 10 and 11). The analysis in the previous section shows that active and passive members somewhat ignore the quality (from their perspective) of the decision while making reward decisions or reward suggestions. We attributed that tendency to altruism.

A between treatment analysis is conducted to get a deeper understanding of the issue. We compare the average level of recipients' cumulated payoffs (i.e. the amount of money they have at the end of the experiment) in each treatment with the corresponding happiness levels (we scaled the values of happiness by multiplying the value by 10 for demonstration purposes). We also show the comparison between the mean proportion of matches and the level of happiness (scaled by dividing the value by 10). As shown by Figure 10, the association between the level of happiness and the proportion of matches is positive and monotonic. On the other hand, it appears that there is no positive association between average cumulated profit and subjective happiness level of subjects.

We calculated treatment averages for the variables under consideration. Figure 11 presents the trendlines for each combination: match-happiness and cumulated profit-happiness. Table 17 shows the Pearson correlation coefficient between the members' happiness level (treatment average) and *match* (treatment average) to be 0.783 (p-value = 0.022). The relationship between cumulated profit (i.e. the amount of wealth at the end of the experiment) and happiness level, in contrast, is negative with a coefficient of -0.417 (statistically insignificant though). Overall, active and passive members are happy if the right decision (from their perspective) is made regardless of the outcome**.** The results indicate a discrepancy between principals' subjective rating of agents' choices and costly reward decisions. Overall, subjective ratings are consistent with the quality of decisions, but the analysis in the previous sections implies that reward decisions were not. In our experimental set-up, reward and punishment decisions are made after the outcome is known. It

would be interesting to know whether the discrepancy is resolved in situations where principals perform both tasks simultaneously.

Insert Figure 10 and 11 from Appendix II here

Insert Table 17 from Appendix II here

6. Generalizability of Experimental Results

Laboratory environment gives an investigator the perfect opportunity to control for variables that are almost impossible to account for in the outside environment. Experimental data allows the researcher to draw causal inferences with great convenience. The trade-off, however, is a loss of external validity. One might argue that the subjective perception of accountability can have a different meaning in real-life situations. Additionally, institutional variations may likely have generated subject pools that differ in characteristics neither quantified nor controlled for in our work. Academics often suggest the use of field data in order to increase generalizability in experimental studies. For example, different accountability theories can be validated by using professionals operating in relevant fields (Hall and Ferris, 2012). In order to assuage generalizability concerns, the study uses real cash as payments to participants. We carefully designed the instruction so that participants can see the connection between their performance and the experimental payoff. Moreover, we recruit subjects from a renowned university in the UK that belongs to the prestigious Russel Group. A considerable proportion of our subjects were in the third year of their undergraduate study or studying at a higher level. The instructions were read out loud at a comfortable pace. Subjects were also given a chance to ask questions after the instruction to make sure that they start the experiment with a clear understanding of their role inside the lab.

Caution should be taken in making comments on the generalizability of our results. Our experiment deals with simple decision scenarios, much more simplified than potential situations top executives have to face in real-life situations. The stakes that are used in the experiment are much smaller than the ones that CEOs face. Our results for that reason should be embraced as provocative

suggestions, rather than as results policymakers can directly translate into policy recommendations. We do not claim that our experimental results have a very high degree of external validity. Nevertheless, our key findings are qualitatively similar to some important developments concerning agency situations. The consistency ought to alleviate some of the generalizability concerns. We find outcome bias in reward decisions made by the active member (the principal with a more proactive role in the principal-agent set-up). The bias is usually associated with a perceived poor decision that got lucky, i.e. that produced a favourable result. So principals were essentially rewarding luck rather than performance. This pattern is qualitatively similar to what Bertrand and Mullainathan (2001) observed in the empirical data they analysed in the seminal study that links CEO compensation to outside factors. The study reveals a strong association between observable luck and CEOs' compensation level. According to Holmstrom (1979), tying luck to compensation can only encourage risky decisions on the part of the agent. Our experimental results are also in line with this prediction. Agents in our experiment made less risk averse decisions in treatments where they can only be rewarded without the possibility of punishment.

7. Conclusion

Individual decisions have long been the centre of focus in decision theory even though many important financial decisions have an effect on the fate of individuals who are not directly involved in the decision-making process. The study examined risky decisions in a principal-agent set-up where the recipients in a three-person group are affected by decisions made by the leader of the group. The leader assumes the role of the agent. The work makes an original contribution to the literature by shedding light on the effectiveness of different accountability mechanisms and by conducting an in-depth analysis of the behaviour of recipients to whom decision-makers are accountable. We study six accountability treatments (in addition to two no-accountability treatments) where a member from each group (an active member) can punish or/and reward the leader. Punishment and rewards are costly in this experiment. One of the goals of our study is to understand the

impact of different accountability tools on decision-making behaviour, especially risk-taking, of the leader. We investigate whether different accountability mechanism (e.g. punishment) produces stronger (i.e., socially more beneficial outcomes) results. The study attempts to pin down the factors that influence the satisfaction level of the recipients as well. We also ask whether evaluators monitoring the accountable decision-makers prefer specific systems of accountability.

We find that accountable agents are more risk-seeking in treatments where the accountability mechanism involves costly rewards. Leaders made less risk averse decisions on a more frequent basis in reward-only treatments. The tendency is reversed when accountability mechanisms involve a punishment component. We observe that recipients are happier when the leader chooses the option they prefer. The impact of alignment on principals' happiness level is much stronger than the influence of any other variable (including current cumulated profit level and the type of the choice). We also find that costly punishment produces better results for the principals. Punishment-only treatments not only produce the lowest number of misalignments between the leader's choices and recipients' preferences but also it is achieved at the lowest costs as the active member needed to use this accountability tool on a less frequent basis (statistically significant compared to reward-only treatments). Finally, we show that punishment decisions, unlike reward decisions, are more line with a hybrid accountability system. A large proportion of principals' reward decisions were biased toward the outcome rather than the process of the decisions. The pattern was not prevalent in punishment decisions. Punishment decisions appear to be more thoughtful and are based on more information.

Our study extends the current literature on decision making for others along multiple dimensions. First, unlike most experimental works on accountability, much attention is paid to principals' behaviour. The study sheds light on how principals use different accountability mechanisms to rein in accountable agents. We show that the principal's behaviour is also influenced by choice of accountability type. Specific accountability arrangements can lead individuals to

act irrationally, while others can attenuate irrationality. We observe outcome bias in principals' reward decisions but not in punishment decisions. Principals rewarded after observing a favourable payoff in spite of otherwise pessimistic views of agents' decision. Secondly, our study investigates the effectiveness of different types of accountability. We document the importance of choosing the right accountability mechanism in order to ensure that agents' actions are in line with principals' preferences. Our study is related to Pollmann et al. (2014), where they compared the effectiveness of ex-post accountability with ex-ante accountability. We add to the literature by showing that even in situations of ex-post accountability, outcome bias can be mitigated by including a punishment component to the accountability mechanism. Our study is also associated with Gächter et al. (2008). In a different set-up, they documented the effectiveness of punishment in generating socially beneficial collaboration. We too document the use of punishment as an accountability tool in producing more social benefits. Finally, the experimental design allows us to gain some insight into the principal's preferences for different accountability systems and how they use reward and punishment to influence agents' decision choices. The paper adds to the existing literature by shedding lights on the link between accountability tools (i.e. reward and punishment) and accountability systems. Our contribution is distinct from prior studies on accountability systems as those pieces of works mostly focus on the effectiveness of different accountability systems and the influence of different accountability regimes on participant's, especially the agent, behaviour (e.g. Davis et al., 2007; Pitesa and Thau, 2013).

The discussion hints at some interesting routes for conducting future studies on accountability. If principals are falling prey to outcome bias while making reward decisions, then maybe somehow holding them accountable as well will debias their decisions. It is possible that if the active members are also accountable for their choices regarding sanctioning costly rewards, they will think more carefully before making that judgment. Holding the principal accountable can act as a debiasing mechanism. Our experimental design does not allow any communication between the principal and agent. The agent is unaware of the preferences of the principal in

all of our treatments. We predict that establishing a communication channel might improve the efficiency in the agent's choices. Channelling more information can also influence how the principal makes reward and punishment decisions. On the one hand, more communication may lead to a decrease in the frequency of altruistic rewards. Another possibility is an increase of vengeful punishment, which was a rather infrequent event in our dataset. The discrepancy between principals' subjective ratings and reward decisions should also be explored further. It would be interesting to know whether, under different principal-agent set-ups, e.g. treatments allowing more information flow, the discrepancy is reduced. Finally, future studies can focus on the cost of accountability in agency situations. In our experiment, we did not vary the cost of punishment or reward. Researchers can explore this avenue since varying the cost of an accountability tool is expected to have an effect on how frequently a principal will use it. This can also influence the behaviour of an agent making decisions on behalf of others.

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8. Appendix II

Figure 1: Choice frequency of the risky prospect by group leaders for different treatments. Mean proportions (out of 1.00) are reported for each treatment. A comparison between leaders' choice and members' preferences (mean proportion of risky choices out of a total 1.00).

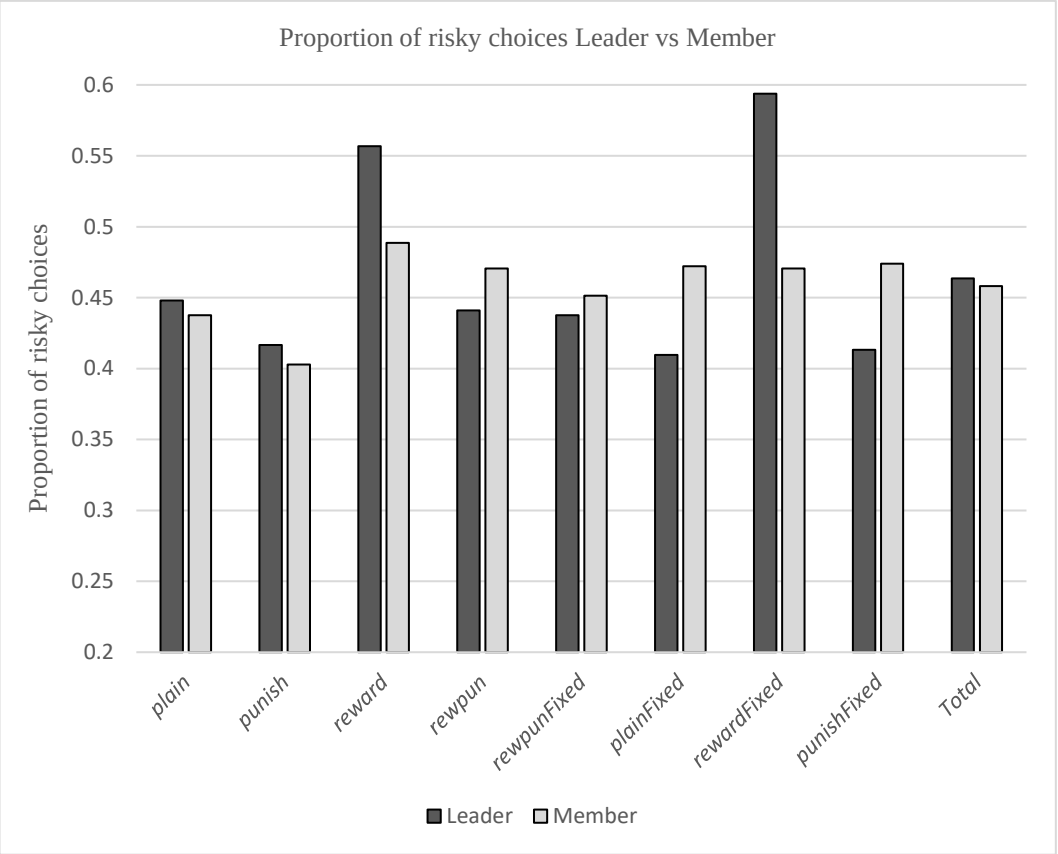


Figure 2: Leader's choice over 24 periods for different treatments.

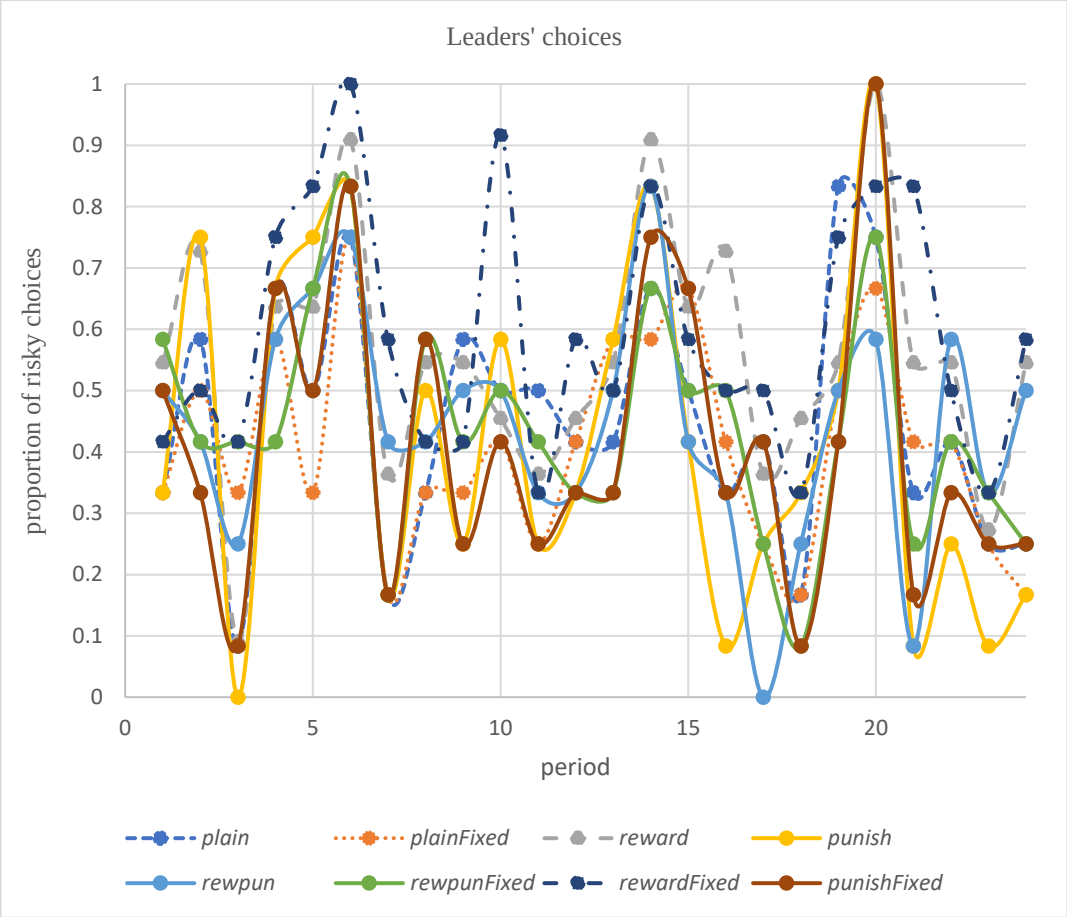


Figure 3: Leader's choices (3-period average).

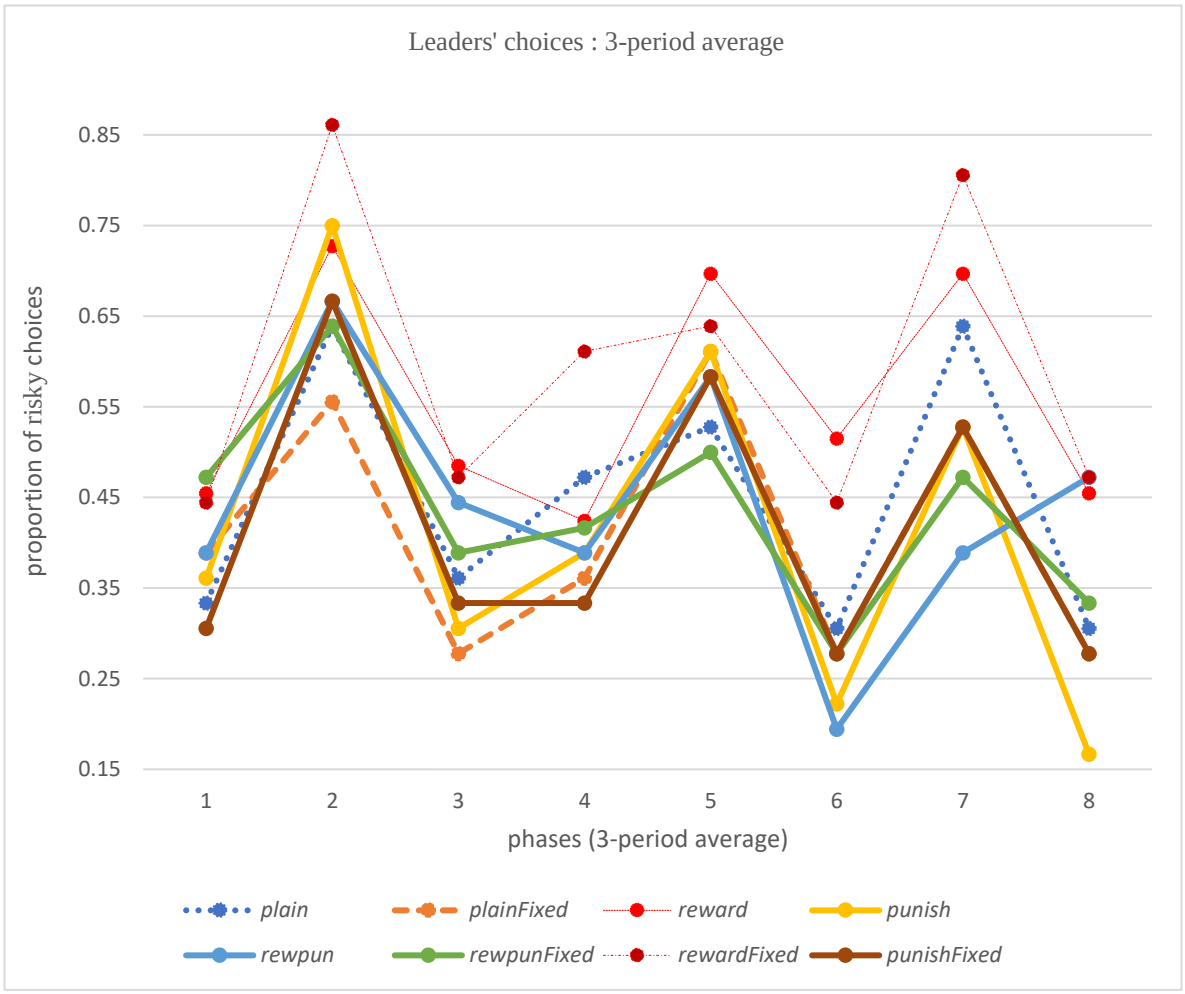


Figure 4: Choice frequency of the risky prospect for high, mid, and low stake levels for different treatments (% of risky choices out of 1.00). High-stake prospects are multiples of 10 (i.e. X=10) and low-stake prospects are multiple of 2 (X=2), the relevant value is 6 for mid-stake prospects.

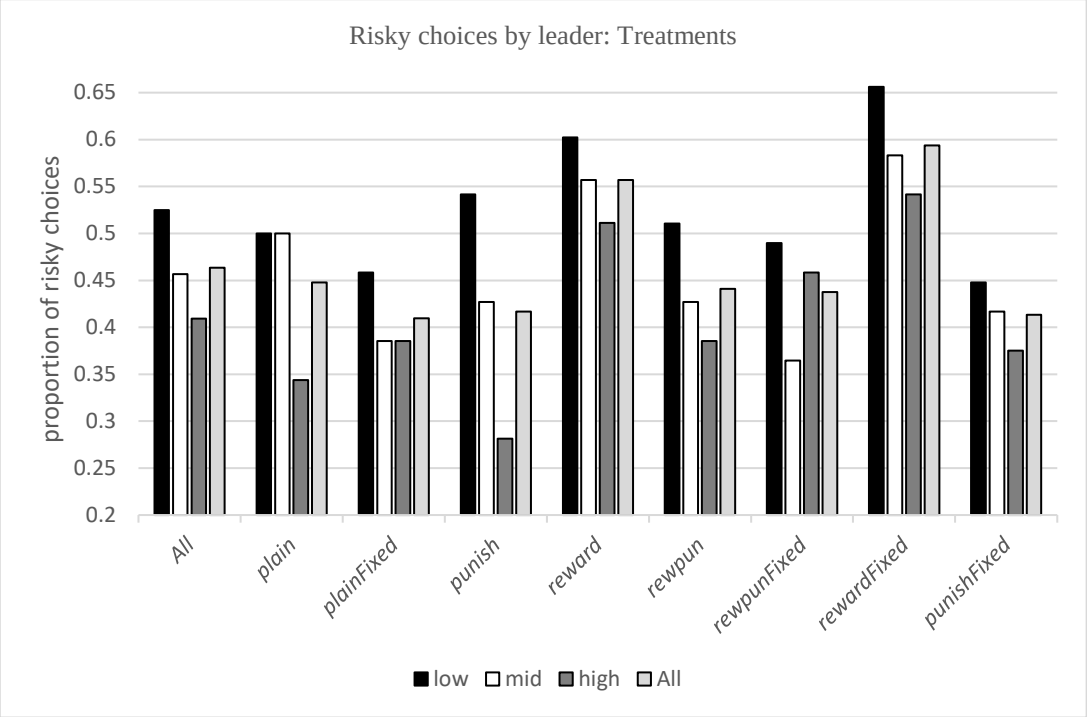


Figure 5: Stake effects across roles: Leader, active and passive members (proportion of risky choices out of 1.00).

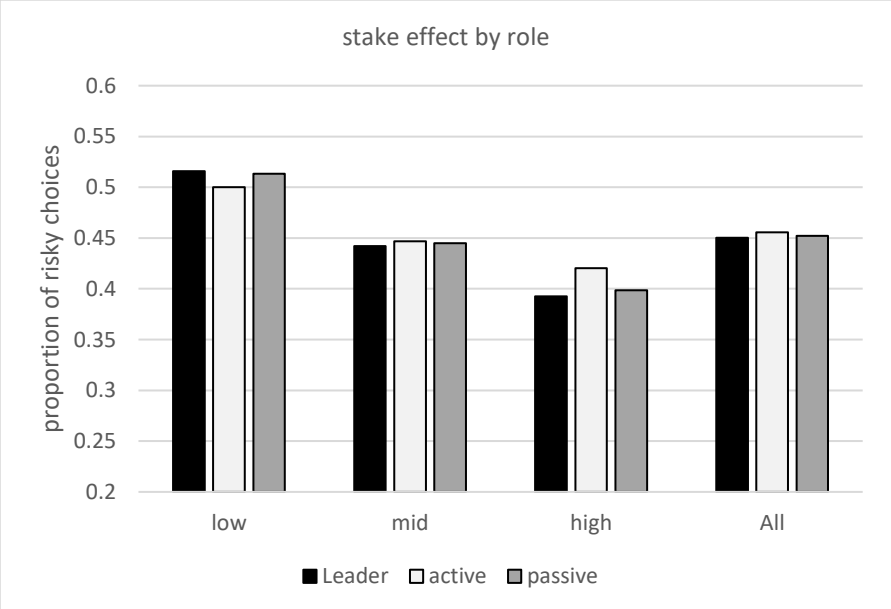


Figure 6: The graph shows the frequencies associated with the number of times in proportions (out of 1.00) there were matches between leaders' choices and members' preferences. A match happens when the leader chooses, and the member (active or passive) of the group prefers the same option (risky or safe).

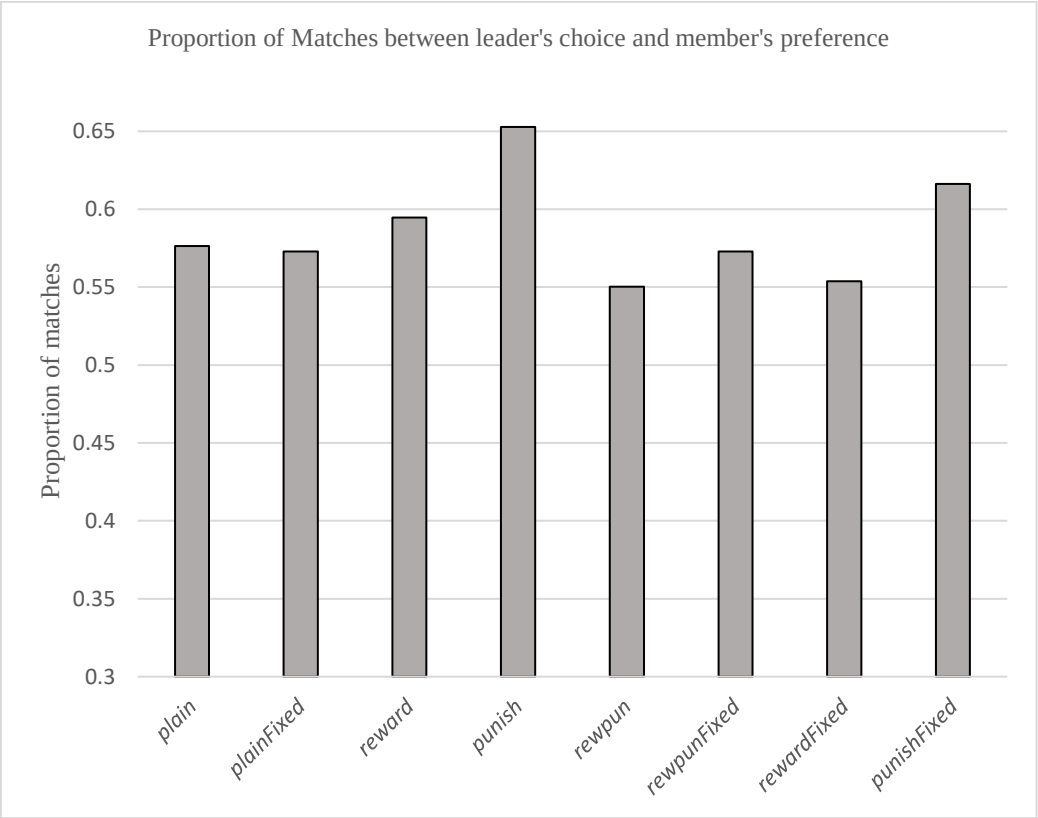


Figure 7: Match over 24 periods. Each line represents a treatment.

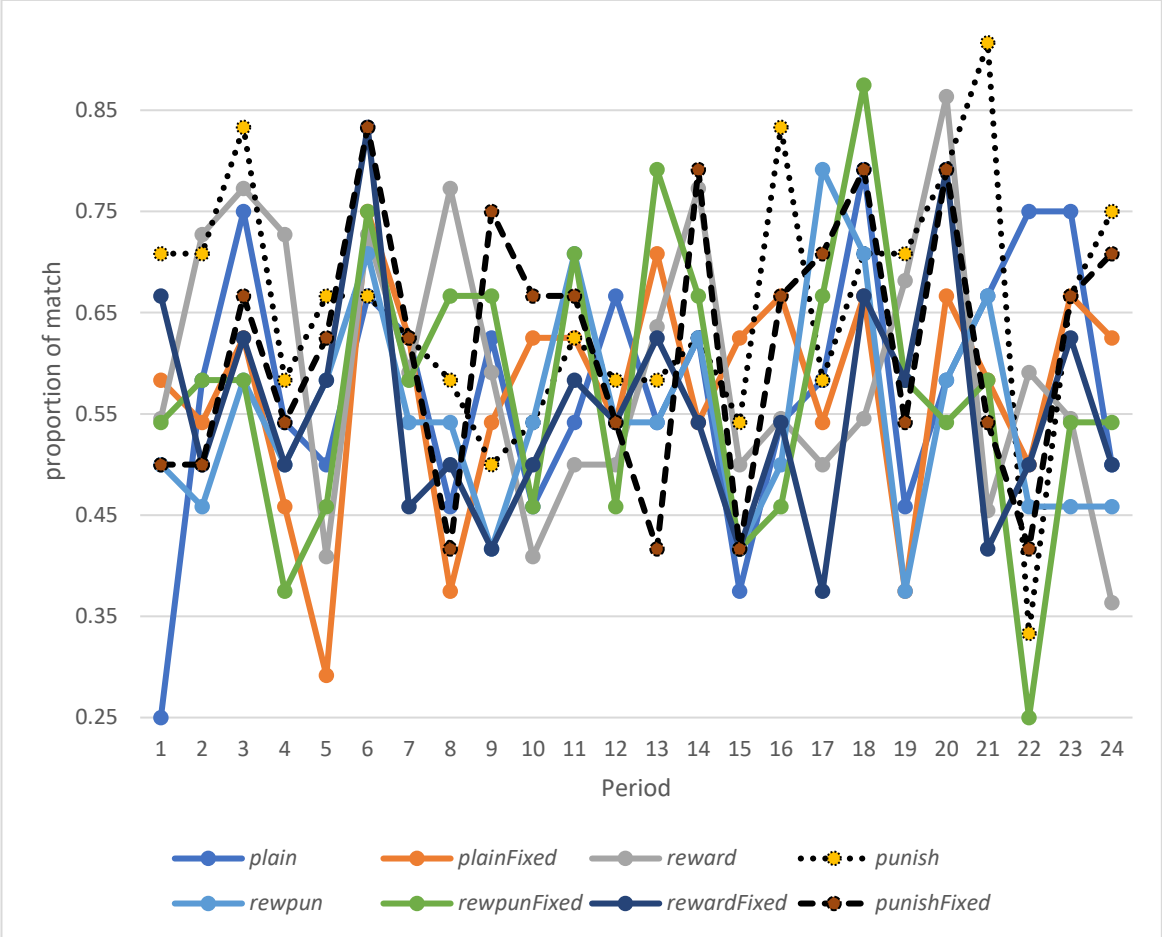


Figure 8: *match* over 8 phases (3 period average values are used). Each line represents a treatment.

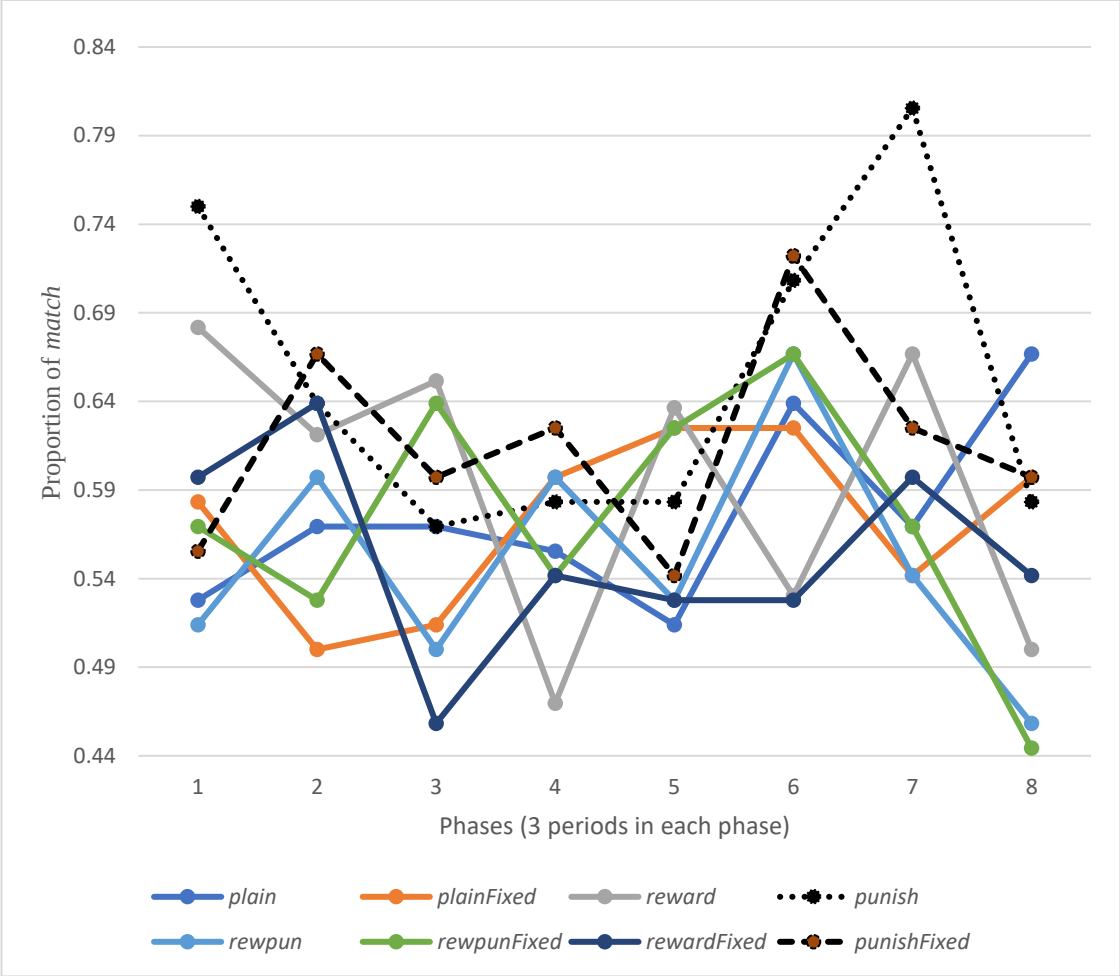


Figure 9: Choice of reward or punishment as a tool to incentivise the leader across different treatments. In the accountability treatments, active members can punish or reward the leader. The figure shows how frequently (out of the total available times, i.e. 1.00) the leaders are punished or rewarded by the active member in each treatment and how frequently the passive member preferred punishment and reward.

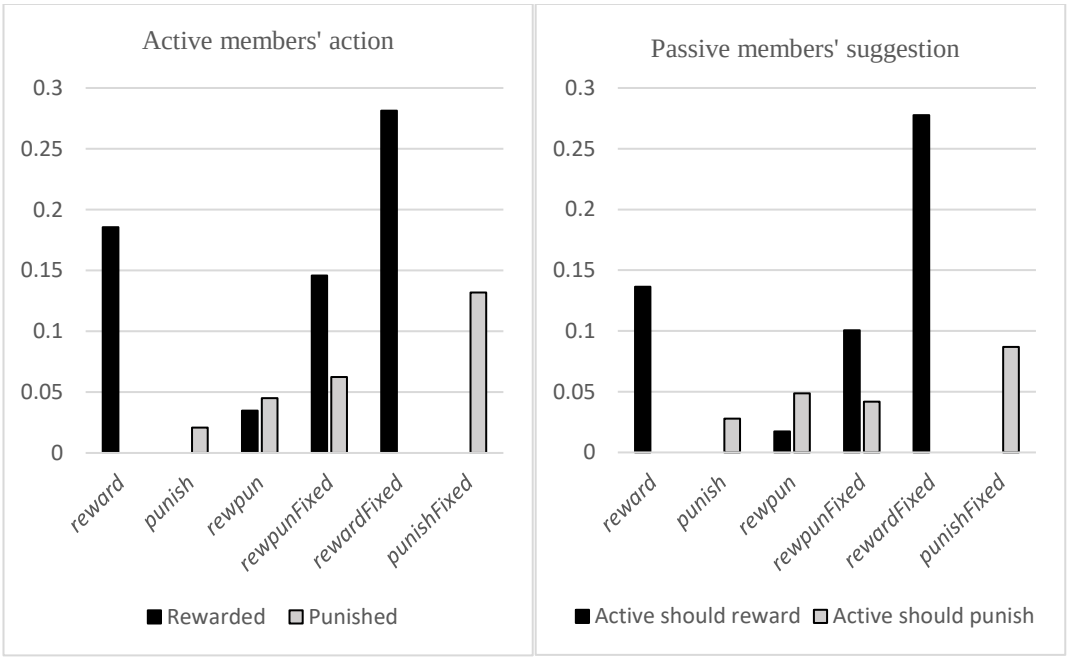


Figure 10: Cumulated payoff vs *match*. Does happiness of the group member rely more on Match? We scaled the values of happiness by dividing the value by 10 for the figure on the left and multiply the value by 10 for the figure on the right for demonstration purposes. Treatment averages are reported.

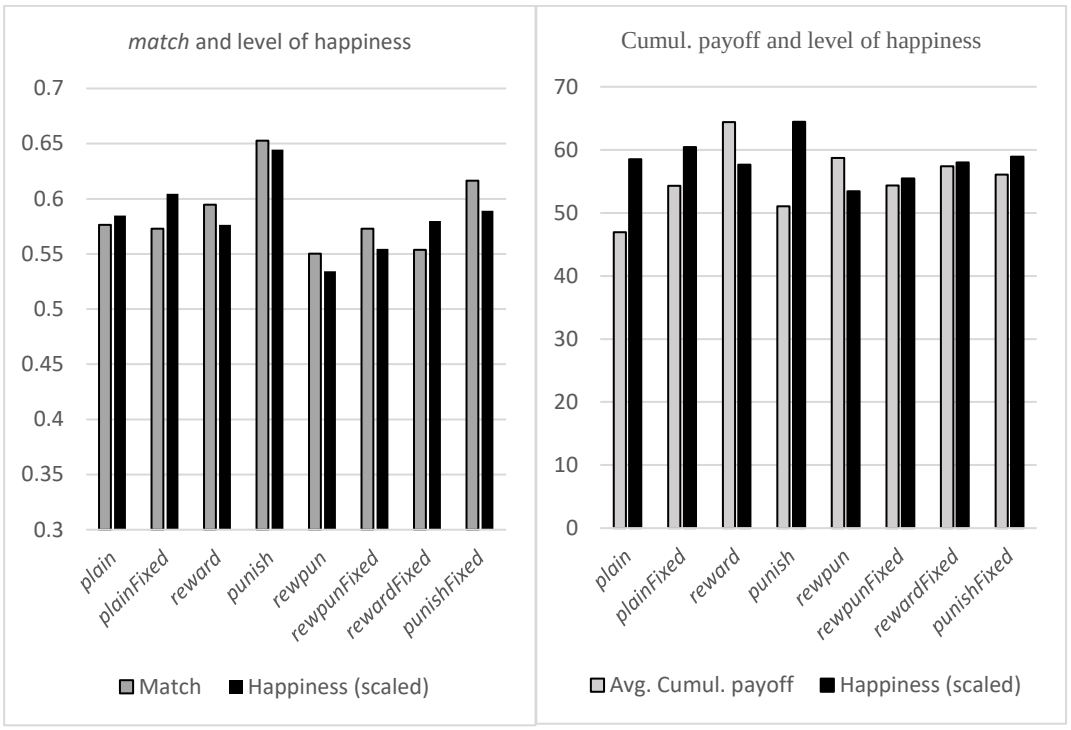


Figure 11: Each dot on the left side indicates the *Average members' happiness – average Match* combination of a treatment; dots on the right indicates the *Average members' happiness – Average cumulated payoff* combination of a treatment.

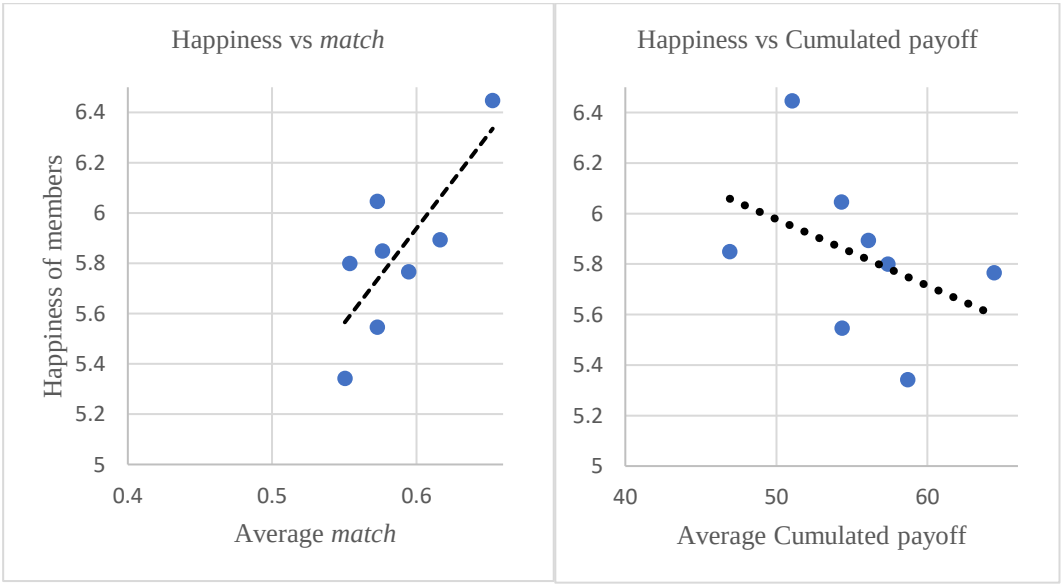


Table 1: P-value matrix**.** P-values are based on a two-tailed MWU test. The variable under consideration is **Leaders' choice**s (of the risky option) among different treatments. A p-value between two treatments indicates whether the distribution of the percentages of risky option chosen by leaders are significantly different.

Two-sided MWU test: p-values Leader Choices

Leaders' choices	*plain*	*plain Fixed*	*punish*	*reward*	*rewPun*	*rewPun Fixed*	*reward Fixed*
plain	.						
plainFixed	0.355	.					
punish	0.450	0.866	.				
reward	**0.011**	**0.001**	**0.001**	**.**			
RewPun	0.867	0.449	0.556	**0.007**	.		
rewPunFixed	0.802	0.500	0.614	**0.005**	0.933	.	
rewardFixed	**0.001**	**0.000**	**0.000**	0.381	**0.000**	**0.000**	.
punishFixed	0.401	0.933	0.933	**0.001**	0.501	0.556	**0.000**

Table 2: Leaders' choices (percentage of risky options): A comparison between first 12 periods vs last 12 periods. Average values are reported. P-values are based on two-sided MWU test.

Leader's Choice in %	1stHalf	2ndHalf	p-value
All	47.19	45.53	0.4250
plain	45.14	44.44	0.9058
plainFixed	39.58	42.36	0.6323
punish	45.14	38.19	0.2328
reward	52.27	59.09	0.2657
RewPun	47.22	40.97	0.2863
rewPunFixed	47.92	39.58	0.1548
rewardFixed	59.72	59.03	0.9049
punishFixed	40.97	41.67	0.9049

Table 3: The reports p-values (from two-sided MWU tests) for the difference between leaders' choice and members' preferences in each treatment.

	plain	*plain Fixed*	*reward*	*punish*	*rewpun*	*rewpun Fixed*	*reward Fixed*	*punish Fixed*	Total
P-value	0.771	0.082	0.071	0.696	0.412	0.699	0.001	0.091	0.668

Table 4: P-value (based on two-sided MWU tests) matrix**.** A p-value between two treatments indicates whether the distribution of the of percentages of risky option preferred by the members are significantly different.

Two-sided MWU test: p-values Members' Preferences

Members' preference	*plain*	*plain Fixed*	*punish*	*reward*	*rewPun*	*rewPun Fixed*	*reward Fixed*
plain	.						
plainFixed	0.237	.					
punish	0.233	**0.018**	.				
reward	**0.089**	0.586	**0.004**	**.**			
rewPun	0.261	0.953	**0.021**	0.547	.		
rewPunFixed	0.635	0.478	**0.096**	0.216	0.516	.	
rewardFixed	0.261	0.953	**0.021**	0.547	1.000	0.516	.
punishFixed	0.214	0.953	**0.015**	0.626	0.906	0.443	0.906

Table 5 Members' preferences for the risky prospect: A comparison between first 12 periods vs last 12 periods. Average values are reported. P-values are based on two-sided MWU test.

Members' preference in %	1stHalf	2ndHalf	p-value
All	49.34	42.28	0.0000
plain	47.22	40.28	0.0933
plainFixed	52.08	42.36	0.0195
punish	44.1	36.46	0.0618
reward	53.03	44.7	0.0557
rewPun	51.74	42.36	0.0243
rewPunFixed	47.22	43.06	0.3154
rewardFixed	48.26	45.83	0.5593
punishFixed	51.39	43.40	0.0552

Table 6: The table shows results from random-effects probit regressions. Here the dependent variable ***Leader'sChoice*** is binary (takes the value 1 if the leader chooses the risky option and 0 otherwise). Panel variables for the regression are a unique subject ID and periods. Errors are clustered by subject ID for every specification, considering that different decisions by the same participant do not create independent observations. The dependent variable in all specifications is a dummy taking the value 1 if the leader chooses the risky prospect and 0 otherwise (i.e., if the safer prospect is selected). We use six dummies for each accountability treatments where the observation takes the value 1 if the observation is associated with the corresponding treatment and 0 otherwise. The variable ***Narcissism*** measures subject's level of narcissism; we use the forty-item Narcissistic Personality Inventory (NPI-40) to calculate a narcissism score for a subject following Raskin and Terry (1988). ***Female*** is a dummy variable which takes the value 1 if the subject is a female and 0 otherwise; ***Riskaversion*** measures risk aversion based on values reported by subjects (1 to 11), using survey inputs: very risk averse (1) to being very risk seeking (11); the variable ***CRT*** measures the level of cognitive ability (scores are 0,1,2, and 3); we used the Cognitive Reflection Test or CRT to measure subjects' level of cognitive ability (Frederick, 2005). The variable ***Stake*** indicates whether the corresponding prospect is a multiple of 10 (high stake = 3), 6 (mid stake=2), and 2 (low stake=1). The variable **UP** (**DOWN**) takes the value 1 when the choice is associated with the prospect Up (Down) and 0 otherwise. The variable ***lagCumulatedPayoff*** is the cumulated payoff from the prior period; ***lagPayoff*** is the payoff from last period. In the pooled model (All) treatment dummies are used. P-values, reflecting robust standard errors clustered by subject ID, are in parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Leader'sChoice	*All*	*plain*	*plainFixed*	*reward*	*punish*	*rewpun*	*rewardFixed*	*punFixed*	*rewpunFixed*
reward	0.471**								
	(0.032)								
punish	0.038								
	(0.857)								
rewpun	0.011								
	(0.953)								
rewardFixed	0.663***								
	(0.004)								
punishFixed	-0.252								
	(0.262)								
rewpunFixed	0.024								
	(0.901)								
Narcissism	-0.016	-0.060*	-0.013	0.008	-0.034*	-0.078***	-0.043	-0.026*	-0.038**
	(0.106)	(0.098)	(0.670)	(0.537)	(0.082)	(0.000)	(0.408)	(0.063)	(0.022)
age	-0.051**	0.010	-0.129	-0.098*	-0.096*	0.001	-0.177*	-0.043	-0.046
	(0.019)	(0.741)	(0.394)	(0.055)	(0.059)	(0.924)	(0.071)	(0.649)	(0.436)
Riskaversion	0.152***	0.346***	0.400**	0.020	-0.291**	0.419***	0.135*	0.123***	-0.034

	(0.000)	(0.000)	(0.023)	(0.720)	(0.016)	(0.000)	(0.065)	(0.000)	(0.547)
CRT	-0.108*	-0.285	-0.552	-0.568***	0.078	0.037	-0.329	-0.070	-0.129
	(0.075)	(0.114)	(0.466)	(0.000)	(0.459)	(0.636)	(0.229)	(0.536)	(0.287)
Female	0.202*	-0.098	0.755	-0.057	-0.559	-0.549***	-0.687	-0.499*	0.598**
	(0.098)	(0.727)	(0.326)	(0.804)	(0.130)	(0.003)	(0.407)	(0.092)	(0.028)
Stake	-0.184***	-0.255*	-0.104	-0.196	-0.393***	-0.256	-0.163	-0.121	-0.040
	(0.000)	(0.065)	(0.446)	(0.197)	(0.010)	(0.104)	(0.215)	(0.114)	(0.607)
UP	-0.798***	-0.979	-0.189	-1.052	-0.606	-1.510*	-0.685**	-0.936**	-0.564
	(0.000)	(0.149)	(0.269)	(0.125)	(0.133)	(0.068)	(0.044)	(0.021)	(0.228)
DOWN	1.349***	1.009**	1.424	1.146***	2.217***	1.123**	1.093***	1.192***	1.121**
	(0.000)	(0.014)	(0.142)	(0.000)	(0.002)	(0.023)	(0.003)	(0.001)	(0.049)
*UP*Female*	0.313	0.568	-0.565*	0.460	-0.189	1.446*	-0.213	0.300	0.403
	(0.116)	(0.437)	(0.063)	(0.531)	(0.728)	(0.058)	(0.581)	(0.480)	(0.505)
*DOWN*Female*	-0.374	-0.164	-0.606	0.000	-1.176	-0.626	0.261	0.658	-0.226
	(0.115)	(0.781)	(0.537)	(0.952)	(0.199)	(0.336)	(0.632)	(0.129)	(0.743)
lagCumulatedPayoff	-0.003***	0.002	-0.004*	0.002	-0.008*	-0.002	-0.003	-0.003	-0.006**
	(0.006)	(0.358)	(0.052)	(0.495)	(0.058)	(0.867)	(0.322)	(0.311)	(0.013)
lagPayoff	-0.010	0.001		-0.006	0.005	-0.0347**	-0.220	0.428*	-0.081
	(0.143)	(0.965)		(0.723)	(0.747)	(0.014)	(0.198)	(0.098)	(0.595)
Intercept	0.581	-1.348	0.777	3.119*	6.009***	-1.457***	6.049*	-1.033	2.137
	(0.352)	(0.240)	(0.832)	(0.068)	(0.004)	(0.000)	(0.078)	(0.697)	(0.103)
N	2185	276	276	241	276	276	276	276	276
Number of Subject clusters	95	12	12	11	12	12	12	12	12

Table 7: The table shows results from random-effects probit regressions. Here the dependent variable ***Member'sChoice*** is binary (takes the value 1 if the member chooses the risky option and 0 otherwise) in a period. Panel variables for the regression are a unique subject ID and periods. Errors are clustered by subject ID for every specification, considering that different decisions by the same participant do not create independent observations. The dependent variable in all specifications is a dummy taking the value 1 if the leader chooses the risky prospect and 0 otherwise (i.e., if the safer prospect is selected). We use six dummies for each accountability treatments where the observation takes the value 1 if the observation is associated with the corresponding treatment and 0 otherwise. The variable ***Narcissism*** measures subject's level of narcissism; we use the forty-item Narcissistic Personality Inventory (NPI-40) to calculate a narcissism score for a subject following Raskin and Terry (1988). ***Female*** is a dummy variable which takes the value 1 if the subject is a female and 0 otherwise; ***Riskaversion*** measures risk aversion based on values reported by subjects (1 to 11), using survey inputs: very risk averse (1) to being very risk seeking (11); the variable ***CRT*** measures the level of cognitive ability (scores are 0,1,2, and 3); we used the Cognitive Reflection Test or CRT to measure subjects' level of cognitive ability (Frederick, 2005). The variable ***Stake*** indicates whether the corresponding prospect is a multiple of 10 (high stake = 3), 6 (mid stake=2), and 2 (low stake=1). The variable **UP** (**DOWN**) takes the value 1 when the choice is associated with the prospect Up (Down) and 0 otherwise. The variable ***lagCumulatedPayoff*** is the cumulated payoff from the prior period; ***lagPayoff*** is the payoff from last period. In the pooled model (All) treatment dummies are used. P-values, reflecting robust standard errors clustered by subject ID, are in parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

Member'sChoice	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
reward	0.306*								
	(0.095)								
punish	-0.157								
	(0.341)								
rewpun	0.078								
	(0.636)								
rewardFixed	0.035								
	(0.830)								
punishFixed	0.010								
	(0.960)								
rewpunFixed	0.032								
	(0.867)								
Narcissism	0.008	0.030	0.008	-0.004	-0.005	0.013	0.014	0.001	0.024
	(0.336)	(0.175)	(0.718)	(0.906)	(0.858)	(0.411)	(0.703)	(0.967)	(0.309)
Age	-0.003	0.012	-0.075	0.012	0.098*	0.080	-0.010	-0.101***	-0.131***
	(0.879)	(0.806)	(0.152)	(0.726)	(0.050)	(0.178)	(0.744)	(0.004)	(0.006)
Riskaversion	0.131***	0.053	0.021	0.160**	0.163*	0.186***	0.211***	0.176**	0.003

	(0.000)	(0.540)	(0.789)	(0.049)	(0.071)	(0.000)	(0.000)	(0.026)	(0.969)
CRT	0.001	-0.050	-0.090	-0.020	0.100	0.082	-0.043	0.086	-0.050
	(0.987)	(0.786)	(0.492)	(0.896)	(0.418)	(0.508)	(0.538)	(0.690)	(0.711)
Female	-0.183*	-0.474	0.067	-0.302	0.004	0.054	-0.151	-0.195	-0.209
	(0.085)	(0.202)	(0.841)	(0.427)	(0.988)	(0.868)	(0.555)	(0.580)	(0.469)
Stake	-0.142***	-0.096	-0.179*	-0.070	-0.257***	-0.161*	-0.194**	-0.110	-0.049
	(0.000)	(0.154)	(0.079)	(0.450)	(0.008)	(0.056)	(0.030)	(0.309)	(0.573)
UP	-1.054***	-0.862**	-0.481	-1.569***	-0.811***	-1.109***	-0.889***	-1.115***	-1.561***
	(0.000)	(0.029)	(0.223)	(0.000)	(0.000)	(0.003)	(0.000)	(0.001)	(0.001)
DOWN	1.376***	1.055***	1.807***	1.436**	1.007***	1.698***	1.584***	1.648***	1.416***
	(0.000)	(0.000)	(0.001)	(0.011)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
*UP*Female*	0.368**	-0.153	-0.331	0.434	0.115	0.644	0.226	0.904**	0.710
	(0.025)	(0.766)	(0.506)	(0.323)	(0.721)	(0.180)	(0.489)	(0.045)	(0.193)
*Down*Female*	-0.221	0.541	-0.301	-0.187	-0.063	-0.818**	-0.654	-0.294	-0.385
	(0.180)	(0.231)	(0.620)	(0.785)	(0.865)	(0.024)	(0.129)	(0.539)	(0.451)
lagCumulatedPayoff	-0.006***	-0.003	-0.010***	-0.009***	-0.005***	-0.007***	-0.003	-0.009***	-0.005*
	(0.000)	(0.253)	(0.000)	(0.000)	(0.009)	(0.001)	(0.136)	(0.000)	(0.082)
lagPayoff	0.003	0.010	0.007	0.005	-0.003	-0.004	0.006	0.001	0.007
	(0.218)	(0.197)	(0.265)	(0.527)	(0.747)	(0.593)	(0.504)	(0.853)	(0.458)
Intercept	-0.463	-0.790	2.246	-0.387	-2.998**	-2.809**	-0.939	1.329	3.045**
	(0.346)	(0.540)	(0.164)	(0.759)	(0.046)	(0.047)	(0.243)	(0.429)	(0.012)
N	4370	552	552	506	552	552	552	552	552
Number of Subject clusters	190	24	24	22	24	24	24	24	24

Table 8: P-value (based on two-sided MWU test) matrix**.** The variable under consideration is ***match*** (the variable value is 1 when there is a match between member's preference and leader's choice) among different treatments**.** A p-value between two treatments indicates whether the distribution of the of percentages of *match* between members' preference and leaders' choices are significantly different.

match	*plain*	*punish*	*reward*	*rewpun*	*rewpun Fixed*	*plain Fixed*	*reward Fixed*
plain	.						
punish	**0.008**	.					
reward	0.538	**0.083**	.				
rewpun	0.802	**0.000**	0.227	.			
rewpunFixed	0.905	**0.005**	0.464	0.501	.		
plainFixed	0.905	**0.005**	0.464	0.614	1.000	.	
rewardFixed	0.440	**0.001**	0.170	0.906	0.514	0.514	.
punishFixed	0.167	0.199	0.463	**0.023**	0.134	0.134	**0.031**

Table 9: The table presents random-effects probit regressions results where the dependent variable is a dummy taking the value 1 if there is a ***match*** and zero otherwise. Panel variables for the regression are unique subject ID and periods. In Model 1, only accountability treatments are included as dummies. In Model 2, we add other explanatory variables. In Model 3 and Model 4, we include dummies capturing the dimensions of the experimental design. The variable ***Fixed*** is a dummy variable taking the value 1 if the observation is associated with a treatment where the leader initially receives a fixed amount in every period and 0 otherwise. The variable ***Accountability*** is a dummy variable taking the value 1 if the observation is associated with a treatment where the leader is accountable and 0 otherwise. We also include two more dummies – ***RewrdOnly*** and ***PunishOnly*** **-** to indicate whether the observation is associated with treatments where the leader can only be rewarded or punished. ***Leader'sGender*** takes the value 1 if the leader is a female. ***Riskaversion*** measures risk aversion based on values reported by subjects (1 to 11), using survey inputs: very risk averse (1) to being very risk-seeking (11); the variable ***Leader'sNarcissism*** measures leader's level of narcissism; we use NPI-40 to calculate a narcissism score for a subject following Raskin and Terry (1988). ***CRT*** the level of cognitive ability (scores are 0,1,2, and 3); we used the Cognitive Reflection Test or CRT to measure subjects' level of cognitive ability (Frederick, 2005). The variable **UP** (**DOWN**) takes the value 1 when the choice is associated with the prospect Up (Down) and 0 otherwise. Errors are clustered at the participant level; p-values, reflecting robust standard errors (clustered by subject ID), are in parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

match	(1)	(2)	(3)	(4)
reward	0.053	0.053		
	(0.536)	(0.529)		
punish	0.207***	0.209***		
	(0.008)	(0.007)		
rewpun	-0.062	-0.080		
	(0.454)	(0.345)		
rewardFixed	-0.054	-0.024		
	(0.499)	(0.779)		
punishFixed	0.108	0.182**		
	(0.116)	(0.028)		
rewpunFixed	-0.005	-0.001		
	(0.943)	(0.991)		
Punishonly			0.191***	0.238***
			(0.002)	(0.000)
rewardonly			0.032	0.051
			(0.634)	(0.437)
Accountability			-0.034	-0.040
			(0.594)	(0.526)
Fixed			-0.038	-0.014
			(0.399)	(0.771)
Stake		0.026		0.026
		(0.252)		(0.252)
Leader'sNarcissism		-0.004		-0.003
		(0.365)		(0.371)
Leader'sRiskaversion		-0.012		-0.013
		(0.302)		(0.272)
Leader'sAge		0.014*		0.013*
		(0.069)		(0.093)
Leader'sCRT		0.020		0.024
		(0.392)		(0.303)
Leader'sGender		-0.014		-0.008
		(0.753)		(0.846)
Riskaversion		-0.004		-0.007

		(0.735)		(0.574)
CRT		0.031		0.033*
		(0.107)		(0.084)
UP		0.323***		0.323***
		(0.000)		(0.000)
Down		0.384***		0.384***
		(0.000)		0.000
Intercept	0.191***	-0.163	0.210***	-0.122
	(0.000)	(0.551)	(0.000)	(0.673)
N	4560	4560	4560	4560
Number of Subject clusters	190	190	190	190

Table 10: Members level of happiness: When **the leader chooses the risky option vs when the leader chooses the safe option.** The level of happiness ranges between 0 and 9. Average values are reported. P-values are based on two-sided MWU test.

	Mean happiness level of members'		
	Leader choice is **Safe**	Leader choice is **Risky**	p-value
Overall	6.225	5.386	0.000
Active member	6.141	5.213	0.000
Passive member	6.310	5.560	0.000
$plain$	6.387	5.186	0.000
$punish$	6.872	5.850	0.000
$reward$	6.406	5.255	0.000
$rewpun$	5.556	5.071	0.045
$rewpunFixed$	5.889	5.103	0.002
$plainFixed$	6.303	5.674	0.008
$rewardFixed$	6.529	5.298	0.000
$punishFixed$	5.973	5.777	0.407

Table 11: Members' level of happiness: first 12 periods vs the last 12 periods. The level of happiness ranges between 0 and 9. Average values are reported. P-values are based on two-sided MWU test.

Member happiness	1st Half or first 12 periods	2nd Half or last 12 periods	p-value
All	5.748	5.924	0.0381
Active member	5.647	5.774	0.3063
Passive member	5.849	6.075	0.0522
$plain$	5.784	5.913	0.4631
$plainFixed$	5.778	6.312	0.0169
$punish$	6.156	6.736	0.0434
$reward$	5.746	5.784	0.7682
$rewpun$	5.388	5.295	0.7925
$rewpunFixed$	5.403	5.687	0.2112
$rewardFixed$	5.857	5.739	0.5965
$punishFixed$	5.872	5.913	0.8550

Table 12: The table shows the impact of a ***match*** between leader's choice and members' preferences on their (members') level of happiness. A *match* happens when the leader chooses and the member (active or passive) of the group prefers the same option (risky or safe). The level of happiness ranges between 0 and 9. Average values are reported. P-values are based on two-sided MWU test.

	Mean happiness level of members'		
	match =1	*match* =0	p-value
Overall	7.308	3.750	0.000
Active member	7.184	3.647	0.000
Passive member	7.432	3.854	0.000
$plain$	7.494	3.610	0.000
$punish$	7.555	4.360	0.000
$reward$	7.013	3.934	0.000
$rewpun$	7.047	3.255	0.000
$rewpunFixed$	7.012	3.577	0.000
$plainFixed$	7.673	3.862	0.000
$rewardFixed$	7.411	3.797	0.000
$punishFixed$	7.214	3.770	0.000

Table 13: The table shows results associated with random-effects ordered logistic regressions. Here the dependent variable is the members'/recipients' happiness/satisfaction level following each choice made by the leader of the group on a number of independent variables. In every period, both members of a group are shown the decision of the leader. Members can indicate how satisfied they are with the decision. Member happiness ratings are collected before the outcome of the decision is known (the outcome of some decisions, especially risky ones, is not immediately available). Member's happiness ratings are coded as discrete values ranging from 0 to 9 where 9 is the highest value. Panel variables for the regression are unique subject ID and periods. As before, errors are clustered at the subject level. Here The variable ***match*** is a dummy which takes the value 1 if there is no discrepancy between the choices made by the leader and the corresponding member of the group. ***Leader'sChoice*** is a dummy taking the value 1 is the leader of the group choses the risky option and 0 if the safer option is chosen; ***lagCumulatedPayoff*** is the cumulated payoff from the prior period; ***Stake*** is a variable indicating whether the corresponding prospect is a multiple of 10 (high stake), 6 (mid stake), and 2 (low stake); ***lagPayoff*** is the payoff to the member in the prior period; ***Leader'sRiskaversion*** measures risk aversion of the leader in the group based on values reported by subjects (1 to 11) using survey inputs: very risk averse (1) to being very risk seeking (11); the variable ***Leader'sNarcissism*** measures leader's level of narcissism; we used the 40-item narcissistic personality inventory or NPI-40 to measure subjects' level of narcissism (Raskin and Terry, 1988). The variable ***Leader'sPsyScore*** measures corresponding subject's leader's level of psychopathy based on the Levenson Self-Report Psychopathy (LSRP) Scale (Levenson and Kiehl, 1995); ***Leader'sCRT*** the level of cognitive ability (scores are 0,1,2, and 3) of the leader; we used the Cognitive Reflection Test or CRT to measure subjects' level of cognitive ability (Frederick, 2005). ***Leader'sGender*** takes the value 1 if the leader is a female and 0

otherwise. In the pooled model (All) treatment dummies are used. P-values, reflecting robust standard errors, are in parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

Members' happiness	(1) All	(2) plain	(3) plainFixed	(4) reward	(5) punish	(6) rewpun	(7) rewardFixed	(8) PunishFixed	(9) rewpunFixed
reward	-0.357								
	(0.284)								
punish	0.304								
	(0.410)								
rewpun	-0.612**								
	(0.043)								
rewardFixed	0.123								
	(0.664)								
punishFixed	0.073								
	(0.832)								
rewpunFixed	-0.253								
	(0.519)								
match	3.138***	3.586***	3.474***	2.597***	2.852***	3.353***	3.634***	2.907***	2.916***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Leader'sChoice	-0.185	-0.479	0.322	0.069	-0.269	0.196	-0.713	-0.061	-0.521
	(0.235)	(0.250)	(0.354)	(0.903)	(0.641)	(0.621)	(0.196)	(0.828)	(0.267)
Stake	0.004	-0.122	0.097	0.231**	0.042	0.072	-0.124	-0.088	-0.033
	(0.933)	(0.245)	(0.529)	(0.023)	(0.748)	(0.670)	(0.462)	(0.429)	(0.755)
*Stake*Leader'sChoice*	-0.187***	0.039	-0.334	-0.462***	-0.222	-0.352	-0.115	-0.125	0.018
	(0.009)	(0.829)	(0.156)	(0.005)	(0.336)	(0.145)	(0.645)	(0.378)	(0.925)
lagCumulatedPayoff	0.003**	0.001	0.010**	0.005	0.004	0.001	-0.001	0.004	0.002
	(0.023)	(0.871)	(0.017)	(0.128)	(0.106)	(0.735)	(0.703)	(0.188)	(0.582)
lagPayoff	-0.001	-0.004	0.002	0.007	-0.004	0.003	-0.011	-0.007	0.010
	(0.864)	(0.655)	(0.779)	(0.483)	(0.739)	(0.791)	(0.222)	(0.482)	(0.410)
Leader'sNarcissim	-0.007	-0.213*	-0.088	0.015	-0.024	(0.000)	-0.003	-0.023	0.004
	(0.649)	(0.068)	(0.264)	(0.599)	(0.668)	(0.993)	(0.942)	(0.576)	(0.950)

Leader'sRiskaversion	-0.081*	0.187	-0.231	-0.030	0.147	0.107	-0.049	-0.126	0.182
	(0.055)	(0.412)	(0.482)	(0.688)	(0.649)	(0.351)	(0.590)	(0.345)	(0.262)
Leader'sAge	0.079***	0.263**	-0.029	0.376***	0.023	0.041	0.003	-0.047	-0.164
	(0.007)	(0.022)	(0.547)	(0.000)	(0.833)	(0.340)	(0.977)	(0.817)	(0.305)
Leader'sCRT	0.153	-0.095	0.069	0.609**	0.034	0.358*	-0.272	-0.087	0.456
	(0.133)	(0.886)	(0.891)	(0.011)	(0.891)	(0.082)	(0.351)	(0.762)	(0.440)
Leader'sGender	0.434*	1.913**	-0.158	0.571	0.011	-0.118	-0.400	0.383	0.759
	(0.056)	(0.022)	(0.805)	(0.238)	(0.990)	(0.813)	(0.609)	(0.425)	(0.539)
Leader'sPsyScore	0.010	0.037	0.047	0.035	-0.017	0.031**	-0.025	0.004	0.040
	(0.306)	(0.310)	(0.188)	(0.316)	(0.715)	(0.023)	(0.285)	(0.838)	(0.476)
Intercept	1.338***	1.688**	0.811***	0.384**	1.255***	0.935	0.518**	0.906***	1.793**
	(0.000)	(0.018)	(0.001)	(0.032)	(0.000)	(0.134)	(0.021)	(0.004)	(0.011)
N	4370	552	552	506	552	552	552	552	552
Number of Subject Clusters	190	24	24	22	24	24	24	24	24

Table 14: Table shows some statistics regarding members' net payoff (NPO), *match* (match and mismatch between active members' choice and leaders' choice), and leaders' choice.

Panel A of the table shows members' expected payoff in each period. In order to calculate the expected payoff, we assume that there is a 50 per cent chance that the leader will select the risky option and a 50 per cent chance that the leader will select the safer option. We than use the corresponding average values to compute the expected payoff for the members.

Panel B presents the mean net payoff, i.e. NPO, and the ratio between the NPO and corresponding standard deviation of the NPO in different treatments. The NPO is the difference between actual payoff and expected payoff to the member in a period. The actual payoff does not include the cost associated with reward or punishment.

Panel C of Table 15 shows some statistics regarding members' net payoff (NPO), *match* (match and mismatch between active members' choice and leaders' choice), and leaders' choice. The NPO is the difference between actual payoff and expected payoff to the member in a period. The actual payoff does not include the cost associated with reward or punishment. A *match* happens when the leader chooses, and the member (active or passive) of the group prefers the same option (risky or safe). Mean variable values associated with the periods the leader is punished are compared with mean variable values associated with the periods the leader is not punished. Corresponding p-values from a two-sided MWU test are also reported. Mean variable values associated with the periods the leader is rewarded are also compared with mean variable values associated with the periods the leader is not rewarded. Corresponding p-values from a two-sided MWU test are reported.

Panel A

Period	Risky Possibility One	Risky Possibility Two	Safer Possibility One	Safer Possibility Two	Expected Payoff [32]
1	25.00	-5.00	10.00	NA	10.00
2	4.00	0.00	2.00	NA	2.00
3	0.00	20.00	12.50	NA	11.25
4	-12.00	0.00	-6.00	NA	-6.00
5	5.00	-1.00	2.00	NA	2.00
6	0.00	4.00	1.50	NA	1.75
7	15.00	3.00	9.00	NA	9.00
8	-20.00	0.00	-10.00	NA	-10.00
9	12.00	0.00	6.00	NA	6.00
10	5.00	1.00	3.00	NA	3.00
11	0.00	4.00	2.50	NA	2.25
12	20.00	0.00	10.00	NA	10.00
13	-2.00	2.00	0.00	NA	0.00
14	0.00	20.00	7.50	NA	8.75
15	-4.00	0.00	-2.00	NA	-2.00
16	25.00	5.00	15.00	NA	15.00
17	-6.00	6.00	0.00	NA	0.00
18	0.00	12.00	7.50	NA	6.75
19	15.00	-3.00	6.00	NA	6.00
20	0.00	12.00	4.50	NA	5.25
21	-10.00	10.00	0.00	NA	0.00
22	12.00	0.00	3.00	9.00	6.00
23	20.00	0.00	15.00	5.00	10.00
24	4.00	0.00	1.00	3.00	2.00

[32] Expected payoff of the members in all treatments and expected payoff of the leader in treatments where the leader does not initially receive a fixed amount.

Panel B

Treatments	NPO	NPO scaled by ST Dev.
plain	0.043	0.009
plainFixed	0.227	0.049
reward	0.733	0.136
punish	0.424	0.094
rewpun	0.548	0.114
rewpunFixed	0.411	0.083
rewardFixed	0.931	0.168
punishFixed	0.830	0.173
Total	0.498	0.100

Panel C

	Mean value associated with the periods the leader is punished	Mean value associated with the periods the leader is not punished	p-value (two-sided MWU)	Mean value associated with the periods the leader is rewarded	Mean value associated with the periods the leader is not rewarded	p-value (two-sided MWU)
NPO	-3.280	0.731	0.000	3.871	0.180	0.000
match	0.213	0.614	0.000	0.626	0.549	0.053
Proportion of risky choices by the Leader	0.600	0.415	0.342	0.500	0.538	0.002

Table 15: The table displays results associated with random-effects probit regressions. The dependent variable in Specification 1 and 2 are the active member's decision to reward and then punish the leader, respectively. The dependent variable in specification 3 and 4 are the passive member' suggestion to reward and then punish the leader, respectively. Panel variables for the regression are unique subject ID and periods. Errors are clustered at the subject level. Column 1 and 2 has results associated with observations related to active members, and Column 3 and 4 have results associated with observations related to passive members in the experiment. Model 1 & 3 includes all treatments involving reward, and Model 2 & 4 includes all treatments involving punishment. In Model 1, the dependent variable takes the value 1 if the active member of the group rewards the leader and 0 otherwise; in Model 2, the dependent variable takes the value 1 if the active member of the group punishes the leader and 0 otherwise. In Model 3, the dependent variable takes the value 1 if the passive prefers the leader to be rewarded and 0 otherwise; in Model 4, the dependent variable takes the value 1 if the passive prefers the leader to be punished and 0 otherwise. The variable ***NPO*** is the difference between actual payoff (before adjusting for reward or punishment cost) and expected payoff of a member in a period. The variable ***match*** is a dummy which takes the value 1 if there is no discrepancy between the choices made by the leader and the corresponding member of the group (the value is 0 otherwise). ***Leader'sChoice*** is a dummy taking the value 1 is the leader of the group choses the risky option and 0 if the safer option is chosen and Stake is a dummy variable taking the value 1 if the choice involves a high stake and 0 otherwise. ***Stake*** is a variable indicating whether the corresponding prospect is a multiple of 10 (high stake), 6 (mid stake), and 2 (low stake); ***Female*** is a dummy variable which takes the value 1 if the subject is a female and 0 otherwise; ***CRT*** the level of cognitive ability (scores are 0,1,2, and 3); we used the Cognitive Reflection Test to measure subjects' level of cognitive ability (Frederick, 2005). ***Fixed*** is a dummy variable taking the value 1 if the observation is associated with a treatment where the leader initially receives a fixed amount in every period and 0 otherwise. P-values, reflecting robust standard errors, are in parentheses. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively.

	Observations associated with active members		Observations associated with passive members	
	Reward	Punishment	Reward	Punishment
	(1)	(2)	(3)	(4)
NPO	0.224***	-0.142**	0.134***	-0.173***
	(0.000)	(0.010)	(0.000)	(0.000)
match	0.360	-0.846***	0.366	-1.172***
	(0.230)	(0.001)	(0.174)	(0.001)
*NPO*match*	-0.081**	0.073	0.002	0.052
	(0.020)	(0.202)	(0.952)	(0.246)
Leader'sChoice	0.139	0.813**	0.122	1.297***
	(0.604)	(0.024)	(0.643)	(0.008)
*Stake*Leader'sChoice*	-0.171	-0.186	0.144	-0.590**
	(0.267)	(0.323)	(0.184)	(0.017)

Fixed	0.661*	1.059**	1.358***	0.539
	(0.050)	(0.012)	(0.007)	(0.260)
Female	-0.543*	-0.150	0.124	0.400
	(0.091)	(0.713)	(0.784)	(0.431)
CRT	-0.397**	-0.219	0.001	-0.137
	(0.028)	(0.211)	(0.996)	(0.589)
*CRT*match*	0.229	-0.251	0.115	-0.137
	(0.106)	(0.170)	(0.388)	(0.548)
CRT Leader'sChoice*	-0.024*	-0.004	0.000	-0.008
	(0.067)	(0.861)	(0.986)	(0.775)
Intercept	-1.463***	-2.242***	-3.561***	-2.655***
	(0.001)	(0.000)	(0.000)	(0.000)
N	1128	1152	1128	1152
Number of subject clusters	47	48	47	48

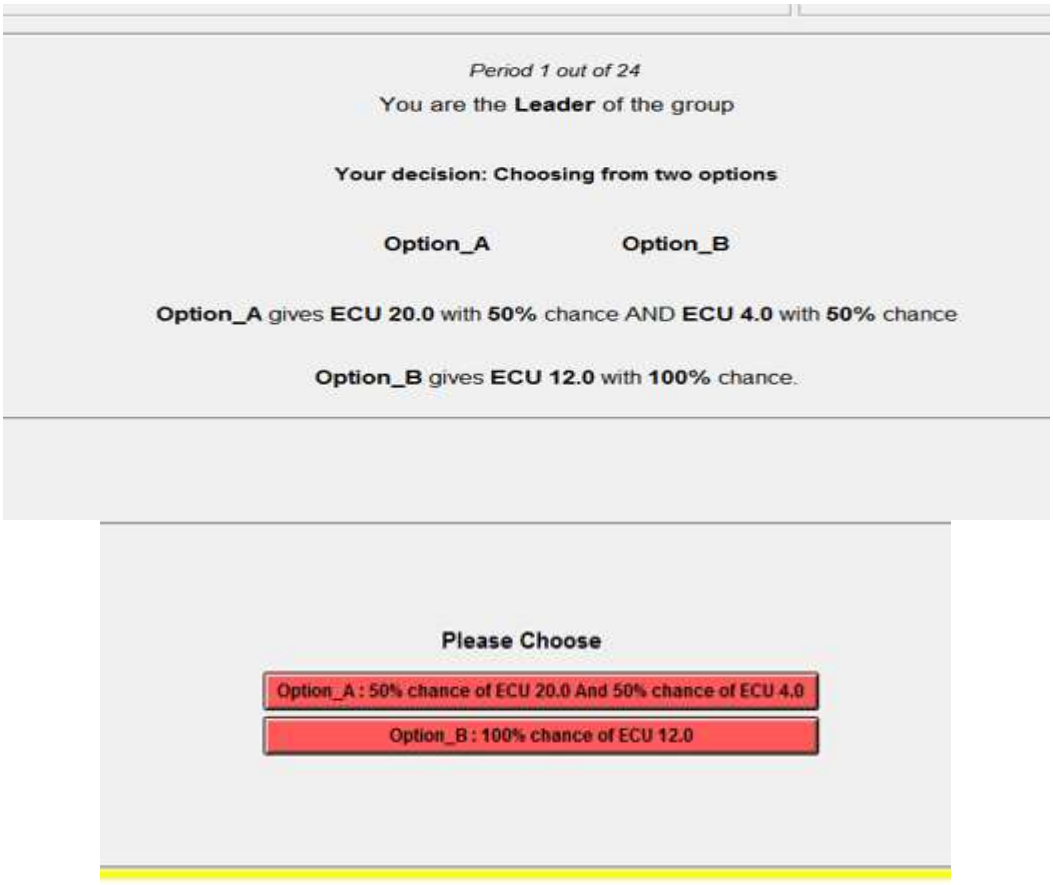
Table 16: Unmerited reward decisions. The table shows the number of decisions that were rewarded even after a mismatch between active member's preference and leader's choice for the prospect in different treatments. The table also presents how many of those decisions were risky choices.

	reward	*rewardFixed*	*rewpun*	*rewpunFixed*
Number of decisions rewarded after a mismatch	16	37	1	14
Risky choices	16	28	1	12
Percentage	100%	75.76%	100%	85.71%

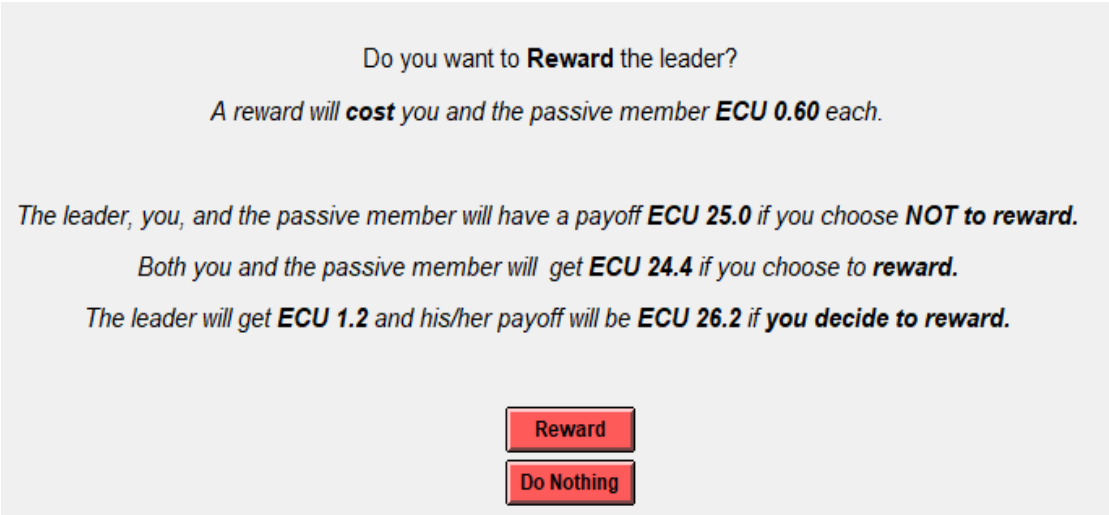
Table 17: Pearson Correlation coefficients among treatment averages. The variable average values are based on observations associated with the members or recipients. The variable ***match*** is a dummy which takes the value 1 if there is no discrepancy between the choices made by the leader and the corresponding member of the group (the value is 0 otherwise). ***Member's happiness*** ratings are coded as discrete values ranging from 0 to 9 where 9 is the highest value. ***Cumulated payoff*** is members' payoff at the end of the experiment (at the end of period 24). N is the number of treatments and p-values associated with the correlation coefficients are reported in the parentheses.

	Cumulated payoff (treatment averages)	*match* (treatment averages)
match (treatment averages)	-0.211	
p-value	(0.616)	
N	N = 8	
Members' happiness (treatment averages)	-0.417	**0.783**
p-value	(0.304)	(0.022)
N	N = 8	N = 8

Picture 1: Leaders' decision window: In the example, the **Leader** has a choice between two options:



Picture 2: The main decision screen for the **active member** would look like this (where the payoff resulted from the leader's decision is ECU 25): the example is related to the treatment *reward.*



A2.1: Games instructions. We present here the instructions associated with two treatments: *rewpunFixed* and *reward*.

Subjects were recruited at the CRIBS laboratory of University of Nottingham, using ORSEE (Greiner 2015). The role of a participant is randomly determined. Participants were asked to pick a number from a jar when they enter the lab and based on that number we determine the role of the participants. The experiment was conducted on computers using zTree (Fischbacher, 2007).

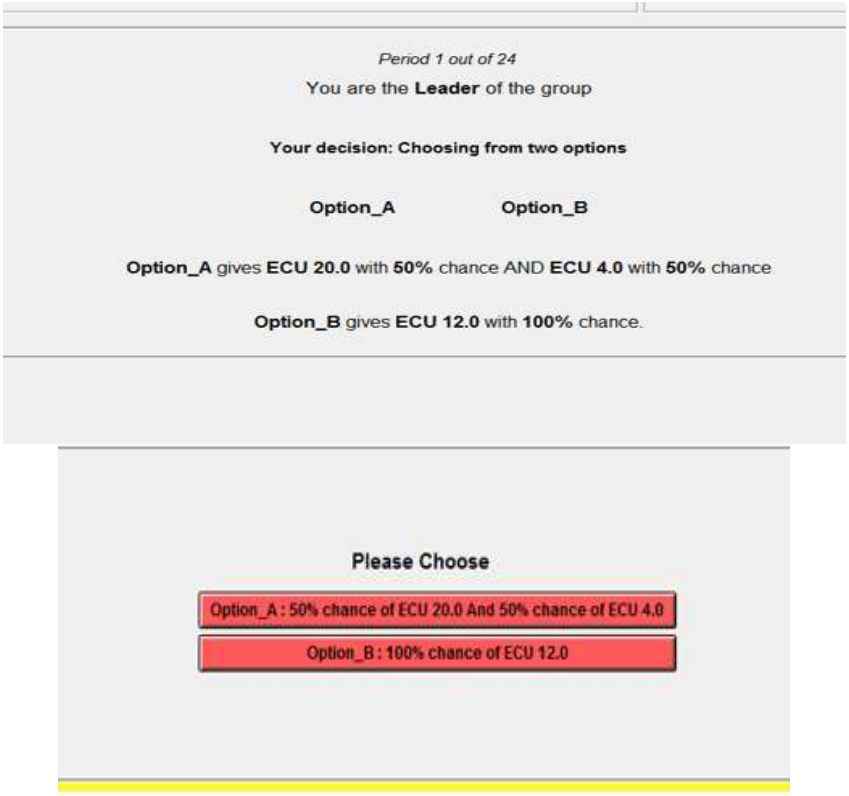
A2.1.1 Instructions associated with the *rewpunFixed* treatment:

In this experiment, you will be a part of a **three-member group**. At no point during or after the experiment other participants in the experiment learn your identity. In the same manner, you do not find out the identity of other participants at any point during or after the experiment. The game lasts for **24 periods**. Each group has

- **A leader**
- **An active member**
- **A passive member**

Each one of you will be given one of the three roles. Your role is randomly determined. Your role and group will remain **unchanged** for all 24 periods. The computer screen will inform you about your role at the beginning of the experiment. Money in this experiment is expressed in **ECU (**ECU 11 = GBP 1**).**

Leader's Tasks
The leader makes decisions on behalf of the group (for themselves and at the same time for active and passive member in the group). The decision involves choosing between a **risky option** and a **safer option**. A representative decision scenario for the **leader** may look like the following:



In the above example, the **Leader** has a choice between two options:

- **Option_A**, which yields **ECU 20.0** with a probability of **50%** and **ECU 4.0** with the complementary probability of **50%. Or**

- **Option_B**, which yields **ECU 12.0** with a probability of **100%**.

*Please note that **Option_A** is riskier than **Option_B.** This is true for all the periods associated with this experiment, i.e. **Option_A** is always riskier than **Option_B**.*

After making the decision, the Leader will be asked to indicate his/her level of satisfaction with the decision.

Tasks for Active and Passive members

Active member and passive member of a group will be provided with information on the leader's decision.

Active and passive members will also be asked to indicate their level of satisfaction regarding the leader's decisions and their preferences concerning the choices of the leader.

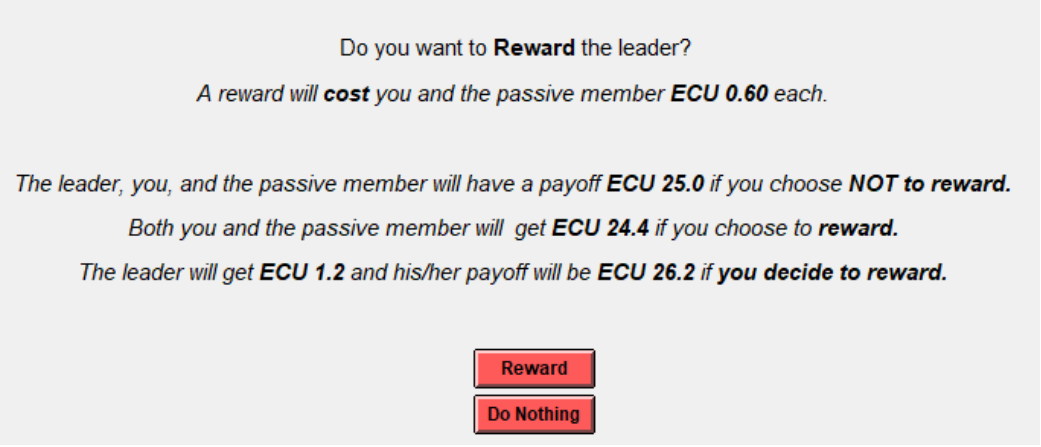
Active and Passive players can indicate whether they are "**very happy**" or "**very unhappy**" (on a scale 10 to 1 where 10 is for **very happy**) with their leader's decision. This information, however, will not be communicated to the leader.

After stating their level of satisfaction, active and passive members will be shown the result associated with their leader's decision.

The active member can reward or punish the leader and therefore can influence the payoff of the leader after observing the result. A reward adds ECU 1.20 to the leader's payoff in a period. On the other hand

Reward, however, is **costly**. Both active and passive members of the group will incur a cost equal to **ECU 0.60** if active member chooses to **reward** the leader.

The main decision screen for the **active member** would look like this (where the payoff resulted from the leader's decision is ECU 25):



The passive member of the group can suggest (after observing the result) whether or not the leader should be rewarded OR punished. **Passive member's suggestion does not influence the payoff of the leader.**

Payoff calculation:

The payoff in each period for all members of a group depends on **leader's** choice AND **active** member's actions. Two examples are discussed:

Example 1

Leader's choices are:

Option_A: *which gives* ***ECU 20.0*** *with* ***50%*** *chance and* ***ECU 4.0*** *with* ***50%*** *chance.*

Or

Option_B*: which gives* ***ECU 12.0*** *with* ***100%*** *chance.*

Leader picked **Option _A** and the resulted **payoff is ECU 20.0.**

If *Active member chooses to reward the leader*

Leader will get ECU 1.20

$$Leader's\ payoff\ will\ be = ECU\ (20.0 + 1.20) = ECU\ 21.20$$

$$Since\ reward\ is\ \boldsymbol{costly}\ and\ the\ cost\ is\ incurred\ by\ both\ active\ and\ passive\ members,$$

$$both\ of\ them\ will\ lose\ ECU\ 0.60$$

$$Active\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (20.0 - 0.60) = ECU\ 19.40$$

$$Passive\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (20.0 - 0.60) = ECU\ 19.40$$

If *Active member chooses "Do Nothing"*

$$Leader's\ payoff\ in\ that\ case\ will\ be = ECU\ 20.00$$

$$Since\ there\ is\ no\ reward, every\ member\ in\ the\ group\ will\ have\ identical\ payoff$$

$$Which\ is\ equal\ to\ ECU\ 20.00$$

Example 2

Leader's choices are:

Option_A: *which gives* ***ECU -12.0*** *with* ***50%*** *chance and* ***ECU 0.0*** *with* ***50%*** *chance.*

Or

Option_B*: which gives* ***ECU -6.0*** *with* ***100%*** *chance.*

Leader picked **Option _A** and the resulted **payoff is ECU -12.0.**

If *Active member chooses to reward the leader*

Leader will get ECU 1.20

$$Leader's\ payoff\ will\ be = ECU\ (-12.0 + 1.20) = ECU\ -10.80$$

$$Since\ reward\ is\ \boldsymbol{costly}\ and\ the\ cost\ is\ incurred\ by\ both\ active\ and\ passive\ members,$$

$$both\ of\ them\ will\ lose\ ECU\ 0.60$$

$$Active\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (-12.0 - 0.60) = ECU - 12.60$$

$$Passive\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (-12.0 - 0.60) = ECU - 12.60$$

If *Active member chooses* "*Do Nothing*"

$$Leader's\ payoff\ in\ that\ case\ will\ be = ECU - 12.0$$

$$Since\ there\ is\ no\ reward, every\ member\ in\ the\ group\ will\ have\ identical\ payoff$$

$$Which\ is\ equal\ to\ ECU - 12.0$$

Your payment from this experiment will be computed based on your **cumulated payoff at the end of Period 24**. The exchange rate is:

ECU 11 = GBP 1

A2.1.2 Instructions associated with treatment *reward*:

In this experiment, you will be a part of a **three-member group**. At no point during or after the experiment other participants in the experiment learn your identity. In the same manner, you do not find out the identity of other participants at any point during or after the experiment.

The game lasts for **24 periods**. Each group has

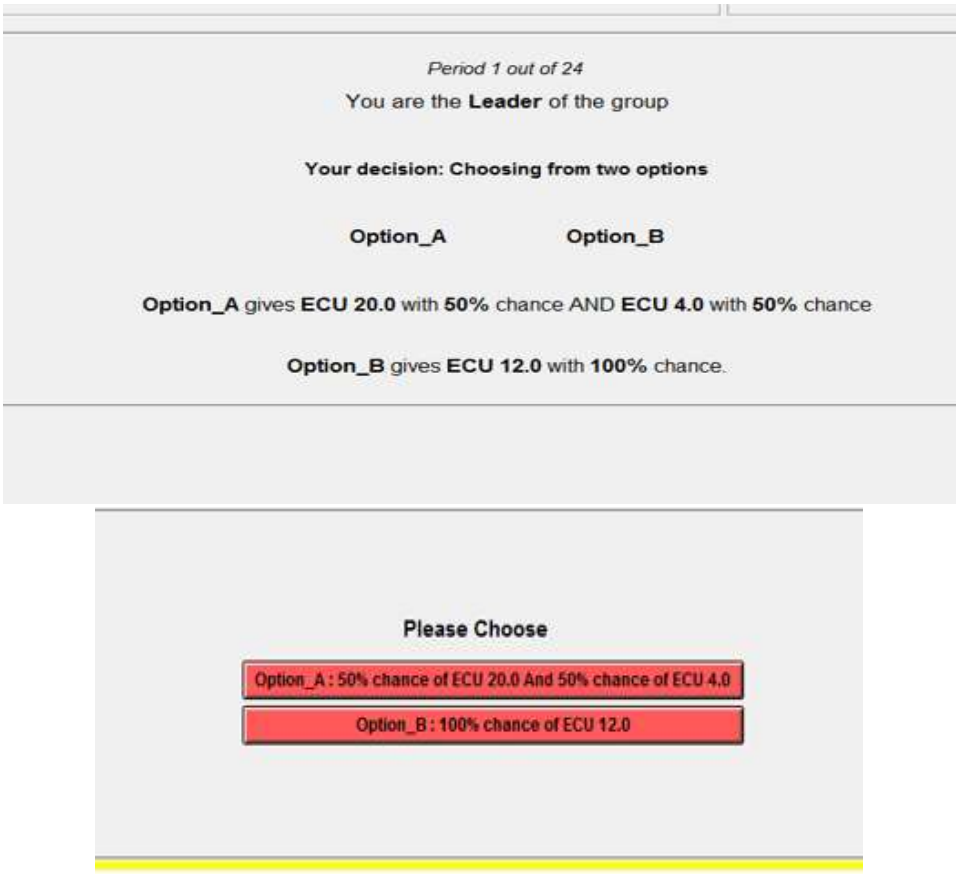
- **A leader**
- **An active member**
- **A passive member**

Each one of you will be given one of the three roles. Your role is randomly determined. Your role and group will remain **unchanged** for all 24 periods. The computer screen will inform you about your role at the beginning of the experiment. Money in this experiment is expressed in **ECU (**ECU 11 = GBP 1**).**

Leader's Tasks

The leader makes decisions on behalf of the group (for themselves and at the same time for active and passive member in the group). The decision involves choosing between a **risky option** and a **safer option**.

A representative decision scenario for the **leader** may look like the following:



In the above example, the **Leader** has a choice between two options:

- **Option_A**, which yields **ECU 20.0** with a probability of **50%** and **ECU 4.0** with the complementary probability of **50%. Or**
- **Option_B**, which yields **ECU 12.0** with a probability of **100%**.

*Please note that **Option_A** is riskier than **Option_B.** This is true for all the periods associated with this experiment, i.e. **Option_A** is always riskier than **Option_B**.*

After making the decision, the Leader will be asked to indicate his/her level of satisfaction with the decision.

Tasks for Active and Passive members

Active member and passive member of a group will be provided with information on the leader's decision.

Active and passive members will also be asked to indicate their level of satisfaction regarding the leader's decisions and their preferences concerning the choices of the leader.

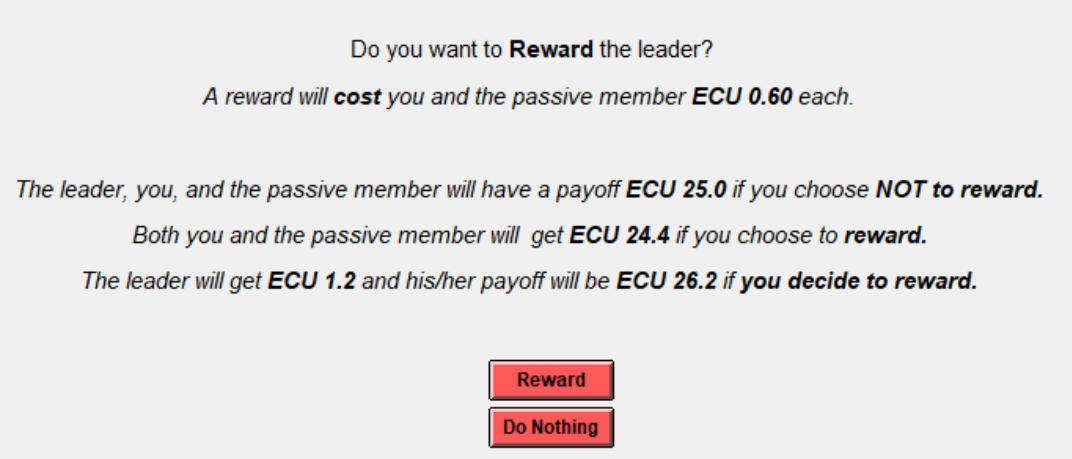
Active and Passive players can indicate whether they are "**very happy**" or "**very unhappy**" (on a scale 10 to 1 where 10 is for **very happy**) with their leader's decision. This information, however, will not be communicated to the leader.

After stating their level of satisfaction, active and passive members will be shown the result associated with their leader's decision.

The active member can reward the leader and therefore can influence the payoff of the leader after observing the result. A reward adds ECU 1.20 to the leader's payoff in a period.

Reward, however, is **costly**. Both active and passive members of the group will incur a cost equal to **ECU 0.60** if active member chooses to **reward** the leader.

The main decision screen for the **active member** would look like this (where the payoff resulted from the leader's decision is ECU 25):



The passive member of the group can suggest (after observing the result) whether or not the leader should be rewarded. **Passive member's suggestion does not influence the payoff of the leader.**

Payoff calculation:

The payoff in each period for all members of a group depends on **leader's** choice AND **active** member's actions. Two examples are discussed:

Example 1

Leader's choices are:

Option_A: *which gives* ***ECU 20.0*** *with* ***50%*** *chance and* ***ECU 4.0*** *with* ***50%*** *chance.*

Or

Option_B*: which gives* ***ECU 12.0*** *with* ***100%*** *chance.*

Leader picked **Option _A** and the resulted **payoff is ECU 20.0.**

If *Active member chooses to reward the leader*

Leader will get ECU 1.20

$$Leader's\ payoff\ will\ be = ECU\ (20.0 + 1.20) = ECU\ 21.20$$

Since reward is ***costly*** *and the cost is incurred by both active and passive members,*

$$both\ of\ them\ will\ lose\ ECU\ 0.60$$

$$Active\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (20.0 - 0.60) = ECU\ 19.40$$

$$Passive\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (20.0 - 0.60) = ECU\ 19.40$$

If *Active member chooses* "*Do Nothing*"

$$Leader's\ payoff\ in\ that\ case\ will\ be = ECU\ 20.00$$

$$Since\ there\ is\ no\ reward, every\ member\ in\ the\ group\ will\ have\ identical\ payoff$$

$$Which\ is\ equal\ to\ ECU\ 20.00$$

Example 2

Leader's choices are:

Option_A: *which gives* ***ECU -12.0*** *with* ***50%*** *chance and* ***ECU 0.0*** *with* ***50%*** *chance.*

Or

Option_B*: which gives* ***ECU -6.0*** *with* ***100%*** *chance.*

Leader picked **Option _A** and the resulted **payoff is ECU -12.0.**

If *Active member chooses to reward the leader*

Leader will get ECU 1.20

$$Leader's\ payoff\ will\ be = ECU\ (-12.0 + 1.20) = ECU - 10.80$$

$$Since\ reward\ is\ \boldsymbol{costly}\ and\ the\ cost\ is\ incurred\ by\ both\ active\ and\ passive\ members,$$

$$both\ of\ them\ will\ lose\ ECU\ 0.60$$

$$Active\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (-12.0 - 0.60) = ECU - 12.60$$

$$Passive\ member's\ payoff\ in\ that\ case\ will\ be = ECU\ (-12.0 - 0.60) = ECU - 12.60$$

If *Active member chooses* "*Do Nothing*"

$$Leader's\ payoff\ in\ that\ case\ will\ be = ECU - 12.0$$

$$Since\ there\ is\ no\ reward, every\ member\ in\ the\ group\ will\ have\ identical\ payoff$$

$$Which\ is\ equal\ to\ ECU - 12.0$$

Your payment from this experiment will be computed based on your **cumulated payoff at the end of Period 24**. The exchange rate is:

ECU 11 = GBP

Paper 3

Responsibility and Experimental Asset Markets

Abstract

Does responsibility reduce price bubbles? We compare two types of laboratory asset markets under responsibility with markets where traders are not responsible. We study two types markets under responsibility *(i)* markets where traders receive a fixed amount for trading on behalf of another subject *(ii)* markets where traders receive a performance-based variable amount for trading on behalf of another subject. Overpricing is significantly lower in markets where traders receive a fixed payoff for trading on behalf of another subject. Responsibility markets where traders receive a performance-based variable payoff are significantly different from baseline markets for the first ten periods of the asset's (a share) life. The study also explores the influence of participants' level of cognitive ability on the end value of wealth in different treatments. We use the subjects' cognitive reflection test (CRT) scores for this purpose. Traders with higher cognitive ability earn significantly more compared to low ability traders in the variable salary markets and baseline markets. The same pattern was not observed in the fixed salary under responsibility treatment.

Keywords: Asset market, Bubbles, Responsibility, Cognitive ability.

1. Introduction

Making risky decisions on behalf of others is a common phenomenon in finance and economics. For instance, professionals managing funds make investment decisions on behalf of their clients and individuals occupying top management positions in public limited companies act on behalf of their stockholders. Individual decision and its relationship with risk preferences have been one of the popular topics in economics. Nevertheless, many decisions under uncertainty are taken in social settings of groups and families, and the outcome of these choices affects not only the decision-maker but also others. We ask the following question in this paper: How

does being responsible for someone else's well-being (i.e., payoffs), as well as one's own, influence choice behaviour? Also, does responsibility affects decisions when decision-makers own payoff is unaffected by the outcomes? We investigate these issues in the context of laboratory asset market experiment. Vernon Smith, Gerry Suchanek, and Arlington Williams in their 1988 seminal paper found price bubble for the first time in finite-horizon laboratory asset markets with a relatively long lifespan. We follow their design in our experiment. Since we emphasise on conditions designed to abolish bubbles, the Smith et al. (1988) design is the natural choice as bubbles have been found to appear consistently in this setting.

The purpose of the study is to understand the impact of responsibility on asset market price stability. In particular, our study focuses on the influence of responsibility on trading behaviour in a laboratory environment. In our experiment, a trader trade on behalf of a passive individual, i.e., traders assume the responsibility of buying and selling of assets (i.e., shares). We study two responsibility environments, along with a baseline environment. There are six markets under each responsibility treatment. Traders receive a flat payment in one of the responsibility treatments. The payoff, on the other hand, in other responsibility treatment, is based on traders' performance during the experiment. We compared our results associated with these two responsibility treatments to results from six baseline markets. Responsibility significantly reduces overpricing in markets where traders receive a fixed salary for performing the task. We also find that the effect of responsibility is somewhat reduced when the trader's payoff is a function of his/her trading performance. Our study is closely related to Cheung and Coleman (2014), Kleinlercher et al. (2014), and Holmen et al. (2014). Authors in these experimental works paid attention to performance incentives and asset market dynamics. Our study is also associated with the work of Baghestanian et al. (2017), where they endogenise liquidity provision within a principal-agent setup. Their study focuses on the impact of various compensation schemes on trading prices. We extend prior studies by focusing on responsibility and its influence on traders' behaviour.

If individuals are concerned about the economic well-being of others, we might observe changes in the behaviour if instead of acting alone, someone is representing

others' economic interests, i.e. making a decision on behalf of other individuals. This issue has been receiving some attention in both social psychology and economics.[33] Following Charness (2000), it can be suggested that an economic agent who is looking after someone else's economic interests will conduct himself in a more 'pro-social' way. In their influential paper, Bolton and Ockenfels (2010) show that being responsible for others welfare increases risk aversion. Recent practices in the financial markets, mainly the designing of financial professionals' incentive structures have been severely criticised following the global economic meltdown. Many argued that these incentives might induce behaviour that makes the market environment more unstable. We argue that the distortive effect of incentives on markets prices can be weakened under conditions of responsibility, and the impact of responsibility is more pronounced in the absence of a monetary incentive.

Our study makes original contributions to the literature of experimental asset markets and decision making for others. To the best of our knowledge, we are the first to explicitly study the impact of responsibility on price stability in laboratory asset markets. While there is some research on responsibility in the field of economics in general, the issue has received little attention from researchers in experimental asset market literature. We predict that responsibility or being responsible for someone's' payoff will influence subjects' attitude toward risk, i.e. risk-taking behaviour. Professional managers who manage money have been receiving much attention (e.g., television interviews, books, featuring in press releases) in recent years (Ding and Wermers, 2012). As per Rajan (2006), the widespread use of convex incentive structures is one of the primary reasons behind the instability in the major markets around the world. The potential of very high upside gains and decreased downside risk encouraged managers to take a very high level of risk. Our study is an attempt to understand the forces that can prod the behaviour of individuals by subduing the influence of different incentive schemes. We also try to understand whether responsibility alone, compared to a combination of responsibility and monetary incentive, can induce pro-social decisions.

[33] For instance, as per Kerr and MacCoun (1985), individuals will knowingly allow free-riding if they feel that doing so is prescribed by their social role.

The notion that retail investors without expert investment knowledge can fulfil their objective of earning a better return by entrusting professionals in the financial markets (e.g., investment bankers, hedge funds, and mutual funds) started to gain popularity from the 1970s. The delegation of responsibility has provided tangible benefits such as reduced transaction costs and improved risk distribution. Nevertheless, it has also shaped conflict between the principal (i.e., the investor) and the agent (i.e., the asset manager), forming a source for information asymmetry and moral hazard. Various payment structures such as combinations of salary and bonus, options, incentives with caps, penalties have been employed in order to alleviate these two issues. The objective here is to align the interests of the principal and agent, who is primarily responsible for the well-being of the principal. Nonetheless, some studies (such as Rajan, 2006; Bebchuk et al., 2010) suggest that these arrangements, built on incentives, encouraged fund managers to take more risk with other people's money which consequently destabilised the market. Allen and Gorton (1993) present the theoretical model of the agency problem between investors and managers acting on behalf of the investors. In this framework, the manager collects a part of the proceeds but does not share the losses. Their study report "rational bubbles," as the convex incentive structure encourages managers to trade at prices much higher than the fundamental values.[34]

Our study directly addresses the role of responsibility on trading behaviour of subjects participated in laboratory asset market experiments within a principal-agent setting. The study is motivated by frequent occurrences of decision scenarios comprising of pecuniary contracts in which the incentive structure of the agent overlaps with the principal's incentive structure. For instance, managers who are rewarded with firm stocks or a stockbroker who is compensated based on the performance of his or her portfolio. Such different state of affairs has mostly been represented with a single objective function treating all choices as if those are

[34] In a similar setup, Jensen and Meckling (1976) modelled the agency problem between shareholders and debtholders of the firm. They show how limited liability allows the shareholder to transfer the risk to the bondholders by obtaining any upside gain but not bearing the full downside risk. Consequently, the firm can undertake projects that are not necessarily value-adding, i.e., negative net present value projects. Similarly, Allen and Gale (2000) study how intermediation by the banking sector results in a comparable agency problem which also leads to asset bubbles.

individual choices. We studied two scenarios where the trader is responsible for another member in the lab room. In one case, traders have zero monetary incentives (trader receives a fixed salary), and in the other regime, they share an equal amount of incentives. In the literature, the latter is termed as "interpersonal" context and the former as "relationship" context (see Leder and Betsch, 2016).

The study extends a small but budding literature on decision-making for other individuals. We examine the effectiveness of the feeling of being responsible for someone's economic well-being as a debiasing mechanism in laboratory asset markets. In experimental asset markets without responsibility, we replicated results similar to prior findings of substantial price deviations above the fundamental value of the asset. This is moderated with responsibility in markets where responsible traders' incentives are fully aligned with passive members' interest. In such a setting, there is a possibility for monetary incentives to have still some influence on asset prices which can produce mild bubbles. Consistent with this, we find that overpricing is smaller in the presence of both monetary incentives and responsibility. We then show that overpricing is significantly reduced in the responsibility regime without monetary incentives. It is observed that when we allow the effects of responsibility not to be contaminated by the presence of incentive schemes, traders are less risk-seeking. This environment significantly reduces overpricing and therefore leads to much smaller bubbles. Bubbles in this environment are formed only in the final third of the assets' lifespan. Besides, we found that reduced overvaluation is not due to the absence of trading activities in the responsibility only markets. Traders with high cognitive ability (measured by the cognitive reflection test or CRT) earned significantly more than traders with low cognitive ability in variable salary with responsibility markets and baseline markets. This pattern, however, was not observed in the fixed-salary with responsibility markets suggesting that the relationship between cognitive ability and wealth generation is environment-specific and not universal. The study contributes to the literature of risk-taking for others. Several related works documented cautious shift induced by responsibility (e.g. Charness, 2000; Charness and Jackson, 2009). Being responsible for someone else's welfare, according to these studies, should induce pro-social actions such as taking more risk

averse decisions. Our primary results are consistent with the notion of a cautious shift under responsibility. We also contribute to the literature on laboratory asset markets. In particular, our study is related to experimental works that studied the effect of different payment incentives in experimental asset markets (James and Isaac, 2000; Cheung and Coleman, 2014; Holmen et al., 2014; Kleinlercher et al., 2014; Baghestanian et al., 2017).

The following section presents a review of relevant literature. Part 3 discusses and develops testable hypotheses. Part 4 describes the experimental design and the treatments. Part 5 presents the results and robustness checks. Part 6 discusses generalizability concerns and Part 7 concludes.

2. Literature Review

2.1. Bubbles in experimental asset markets

The efficient market hypothesis suggests that asset prices should always reflect all the information relevant to the asset (Malkiel and Fama, 1970). Many academic works, however, focusing on field markets as well as experimental markets, demonstrated the fact that prices do not always reflect the fundamental value of the asset, i.e. prices are not sufficiently efficient. This is an indication that behavioural biases among economic agents may be at large. In their 1988 seminal study of experimental asset markets, Smith et al. (1988) (SSW henceforth) found the experimental results which would give birth to a new area in the experimental economic research. Earlier works such as Smith (1962), Smith (1965), and Forsythe, Palfrey, and Plott (1982) applied the double auction market set-up to examine intertemporal markets and assets with uniform value to traders. Nevertheless, the ground-breaking work of SSW was the work that combined all the prior studies. They unexpectedly discovered bubbles and crashes in experimental asset markets based on the design that was expected to generate informationally efficient prices.

Studies in the experimental asset market literature mainly focus on different factors that aggravate or reduce bubbles and crashes. Several works concentrate on the link between the structure of the asset market and performance. Studies especially

try to shed lights on how information is released, and some works focus on asymmetric information, i.e., information is distributed unevenly among the participants, which make some participants insider (Camerer and Weigelt, 1991; Noussair and Xu, 2015). Brandouy et al. (2000) show that the presence of insiders can improve market efficiency if that piece of information is disclosed to other participants. Schnitzlein (2002) shows a link between market price efficiency and the public knowledge of the number of insiders present in the market.

The study of SSW was first to notice that experience can play an influential role in mitigating bubbles in experimental asset markets. They found that the presence of twice experienced participants can significantly reduce market bubbles (SSW, pp. 1134– 1135). A similar pattern was observed in other studies such as King (1991), King et al. (1993) and Van Boening et al. (1993). The work of Dufwenberg et al. (2005) compared markets with more and less experienced traders. The study showed that the presence of even a small fraction of experienced traders could significantly reduce bubbles. Lei and Vesely (2009) demonstrate that observing and earning dividends associated with an asset in a pre-market stage can sufficiently eliminate bubbles in experimental asset markets.

Studies also focus on the link between traders' emotion and bubble occurrence. Although empirical works such as Kamstra et al. (2003) and Hirshleifer and Shumway (2003) explored this issue, little experimental work was done on this topic so far. Andrade et al. (2015) study the influence of heterogeneous emotional states on mispricing by inducing different types of emotions (e.g., excitement, sadness, fear) through the use of video clips. It was noticed that bubbles are more pronounced when traders are shown exciting video clips. The magnitude of the bubble is larger compared to situations where traders are shown sad, neutral, and fearful videos. The findings in Breaban and Noussair (2017), who used real-time emotions trackers, are also linked to the previous findings, i.e., positive emotions lead to overpricing, and negative emotion (such as fear) is associated with lower market prices. Kocher et al. (2018) examine the connection between self-control and mispricing by employing two different market environments. In one of the settings, traders had less self-control, and this treatment produced significantly higher mispricing relative to the treatment where

traders had more self-control. The findings are indicative of the fact that markets populated by traders suffering from self-control problems allow more exuberant activities which can cause overpricing. Corgnet et al. (2015) study the effect of house money on bubble formation. A comparison between markets with house money and earned money revealed that trading volumes and income spread are much higher in markets where subjects traded with house money. The study also shows that subjects with higher cognitive ability earned more than subjects with lower cognitive ability. Studies also find between tournament incentives and social comparison lead to bigger bubbles (see James and Isaac, 2000; Schoenberg and Haruvy, 2012).

Confusions associated with the fundamental value (Huber and Kirchler, 2012) can cause bubble formation, and common knowledge of rationality can virtually eliminate bubbles from experimental asset markets (Cheung and Palan, 2012). Noussair et al. (2001) show that bubble can occur (in a smaller size) even in markets where assets' fundamental value is constant. Studies also find that homogenous endowment and taxes and transaction costs do not have a significant influence on bubble elimination (see King et al., 1993). Michailova and Schmidt (2016) demonstrated a significant association between traders' level of overconfidence and bubbles. It was shown that high level of overconfidence in traders leads to larger bubbles. The study by Cheung and Palan (2012) focused on team trading, and they showed that trading as teams could reduce bubbles to some extent. This pattern was initially understood to be the result of agents' belief that other participants are irrational, i.e. they will trade irrationally. If someone believes that some of the traders operating in the market are prepared to purchase (sell) at prices higher (lower) than the fundamental value than he can generate capital gain even if he is buying at a price higher than the intrinsic value of the asset (Nuzzo and Morone, 2017).

The work of Baghestanian and Walker (2015) adds to the list of researchers that concentrate on participants' confusion and the magnitude of bubbles. They show that the occurrence of bubbles can be diminished by graphically displaying the fundamental values to the subjects. Passive participation before participating in actual trading can also reduce the occurrence of bubbles. Cason and Samek (2015) examine this link in laboratory asset markets with experienced and inexperienced subjects. The

study allows pre-session involvement in which one subject is paired with a trader from a different prior market and witnesses all trading details without getting involved with the actual trading process. They show that passive involvement, like direct experience, substantially decreases bubbles in subsequent experimental markets. The work of Cheung et al. (2014) emphasizes on the common knowledge of other participants' rationality and its influence on market mispricing. They assert that informing the traders that other participants operating in the market are trained can significantly reduce bubbles. On the other hand, even if traders are trained but not informed of the fact that others also received training, mispricing in the market persists. Lugovskyy et al. (2014) show that there is a link between traders' level of asset holding and mispricing. It was observed that positive bubbles are significantly smaller in case of permanent caps. The study suggests that bubbles can be eradicated by introducing asset-holding caps and proper market design.

The dynamic interaction among agents operating within financial markets has been the focus of many studies and motivated a number of models (Chen, Hong and Stein, 2001; Gaunersdorfer et al., 2008). Stylized facts in the form of leptokurtic returns and autoregressive dependence in volatility have been the subjects of many studies in this literature. Morone (2008) and Kirchler and Huber (2007) verified whether these are also true for experimental asset markets. They find that heterogeneity in fundamental information is the reason behind volatility clustering and fat tails. Kirchler and Huber (2007) show that declining absolute returns are positively associated with the influx of new information regarding the intrinsic worth of the security. Moreover, asset price volatility is found to be declining in quantity as well as the quality of information (Morone, 2008). Hussam et al. (2008) showed that bubbles could re-emerge and stay robust owing to increased liquidity shock and manipulation of factors such as dividend and a new entrance of traders.

2.2 Decision making for others

Popular economic theories associated with risky decisions, for instance, expected utility theory (Von Neumann and Morgenstern, 1947) or prospect theory (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992) only study decisions at the

individual level, i.e. study scenarios where the outcome is relevant only to the decision-maker. Nevertheless, in real-life situations, many decisions are taken on behalf of individuals who are not part(s) of the decision-making body. The economic circumstances in which decision-makers choices affect other individuals' outcomes, as well as her own, establish a common category of phenomena. For example, we can think of circumstances where a decision-maker might influence the outcomes for family members in addition to herself. We often observe that in financial-agency contracts, the incentive for an agent overlaps with that of the principal. Which means that both are affected by the outcomes of choices made by the agent in the set-up. For instance, in corporations, top managers are rewarded with company stocks and the investment manager rewarded based on the performance measured by the profit he makes. Managers in public companies act as shareholders' agents and the decision they take effect the wealth levels of shareholders. These different circumstances have mostly been demonstrated with a single objective function seeing all choices like decisions that do not affect others (when these decisions do affect others).

Many studies in recent years shaded lights on the topic "decision making for others." For example, Pahlke et al. (2015) compare decisions under responsibility with individual decisions, i.e. they compare decisions where the decision-maker is the only person to be affected with decisions where others' financial position is also affected. Bohnet et al. (2004) show that inequality resulting from a risky choice does not affect risk-taking. In Eriksen and Kvaløy (2010), decision-makers behaved cautiously when they were acting as agents for another entity, i.e. deciding on behalf of others. Chakravarty et al. (2011) study the attitude toward risk of subjects who make decisions for others in a simple experimental set-up. The focus of their study was on a comparison between subjective preferences in situations when only the decision-maker makes decisions for himself and situations when the decision-makers make decisions for others. It was found that subjects are much less risk averse when they are making decisions for others compared to the decisions they make for themselves. Subjects were generally risk averse when only they are affected by the decisions they make and appear close to risk-neutral when they are making decisions for others. Bolton et al. (2015) focused on the conjecture that social responsibility increases the

level of risk aversion in decision-makers compared to baseline scenarios (i.e. where decisions are made at an individual level). They observed a higher level of caution in decision making when agents are socially responsible.

Various hypotheses about the level of risk aversion have been tested in the literature of risk-shifting. Following the seminal work of Stoner (1961), various studies suggested that decisions made by groups exhibit a lower level of risk aversion when compared with decisions made by individuals. Some contemporary studies, however, show the opposite pattern, i.e. it is shown that decisions made by groups are riskier than individual decisions. The focus of our study are situations where an individual decides for a group rather than decisions made by groups. The contrasting feature between group decisions and decisions under responsibility is that in the latter case, the decision-maker is the only individual who is responsible for the outcome. A cautious shift is expected since the individual responsible for the outcome is attempting to avoid an adverse outcome, i.e. aversion toward guilt forces the decision-maker to act more cautiously.

Reynolds et al. (2007) find that people are less risk averse in making decisions for themselves than when making decisions that affect others. We also argue that the sense of being part of a group will lead traders in the two-person group to demonstrate a higher level of risk aversion. Rockenbach et al. (2007) show that teams gather considerably higher expected value compared to individuals, and by taking a significantly low level of risk. They suggest that this observation is an indication that teams are more likely to avoid taking excessive risk. Charness et al. (2007) show that teams less likely to violate Bayes' rule and first-order stochastic dominance compared to individuals.

Studies in the literature of decision making for others distinguish between responsibility and another closely related term in the form of accountability (Hall et al., 2017). As per Hall and Ferris (2011), accountability involves a perceived expectation that an audience will judge the decision maker's actions. It means that the decisions are evaluated by an individual or a group of individuals who might not be a part of the decision-making body but might be the one(s) affected by the decisions or actions of the decision-maker. As per Hall et al. (2017), expected evaluation and the

presence of a salient audience are the vital parts of accountability. Even though some studies (e.g. Frink et al., 2008) treat these terms as equivalents and use them interchangeably, other studies (such as Schlenker et al., 1994) differentiate accountability from responsibility. These works suggest that responsibility does not necessarily require the presence of an external audience. In our experimental design, decision-makers know they are making decisions on behalf of others as well as for themselves. At the same time, they also know that their choices are unobservable to the principal, and the principal is unable to affect their final payoff. So, the decision-maker or the trader in our experimental design is not accountable for his/her decisions; he is only responsible for other's economic well-being.

3. Hypotheses

It is well known that incentives can shape the behaviour of individuals. Incentive is one of the critical components in our experimental design, and our experiment is in the spirit of James and Isaac (2000). Their pioneering work on laboratory asset markets mainly focuses on the impact of non-linear incentives on asset price stability. They demonstrated the emergence of mild overpricing in the experimental asset markets. In our experimental setup, we also make the traders trading on behalf of the two-person group aware of the fact that the person they are responsible for is physically present in the room. We tell them that they are responsible for the final payoff of the subjects in their groups.

A set of studies in the literature of risk-taking for others relate choices for self and choices made for other individuals in the group to the outcome of the decision. In one case, the decider is unaffected by the outcome of the decision, and in the other case, the decision-maker is affected by the choices she makes. For example, Beisswanger et al. (2003) study decisions when the outcome does not have any economic relevance for the decider. On the other hand, Eriksen and Kvaløy (2010), and Chakravarty et al. (2010) study this phenomenon (i.e. decision making under responsibility) by incorporating monetary incentives, i.e. the decider is affected by the outcome of the decision. We vary the prominence of responsibility in two responsibility treatments.

In one of the responsibility treatments (responsibility with fixed salary), the role of responsibility is more prominent since any monetary rewards no longer motivate traders. The decider is essentially deciding for one other participant in this environment, and the outcome of the decision is irrelevant to the decider's final payoff. In terms of similarities in design, our study is closest to Baghestanian et al. (2017). However, whereas Baghestanian et al. (2017) mainly focused on the effect of different compensation schemes on trading behaviour of the participants who are paired with another subject in the experimental lab, we focused on the fact that traders are responsible for someone else's payoff. Several studies show that assets in laboratory markets do not follow the intrinsic value, which leads to the formation of bubbles that crushes in the final phases of the assets' life. While it is not completely obvious in our experiment what 'pro-social' behaviour would be, we might nevertheless expect players to choose a safer strategy when someone else's welfare depends on their decision.

An important issue concerning delegated decisions is whether there is a misalignment between the risk preferences of the decision-maker and parties affected by those decisions. According to Bolton and Ockenfels (2010), decision-makers, in general, tend to be more risk averse when others are also affected by what they decide. Charness and Jackson (2009) also studied the behaviour of decision-makers under two situations: decision-makers were making decisions for themselves, and they were also responsible for another individual. It was revealed that under responsibility, the safer strategy was selected in most cases. Eriksen and Kvaløy (2010) show how decision-makers (i.e., agents) become more loss averse when they are responsible for their principals. We predict that the effect of responsibility will be manifested in the magnitude of asset market bubbles. Based on the previous discussions, it is reasonable to expect that subjects will exhibit more cautious trading behaviour in markets with responsibility. We also expect that the size of the bubble is the smallest when the role of responsibility is more prominent. Between two responsibility treatments, the one where traders' salary is fixed is for that reason expected to induce the most significant cautious shift.

The study also investigates the association between subjects' cognitive ability and their trading behaviour in different types of markets to supplement the analysis of the responsibility effect. As pointed out by Corgnet et al. (2015), cognitive capabilities, as measured by the CRT, do seem to matter. They show that subjects with a high level of cognitive ability (i.e., with higher CRT scores) earn more money by feeding the bubble in the initial phases and getting out of it at the later stages of the markets' lifespan. Bosch-Rosa et al. (2018) showed that markets populated by traders with high cognitive ability did not produce bubbles. Markets with traders low in cognitive ability, nonetheless, produced bubbles. Whether varying the nature of the contract available to the trader can weaken or strengthen the relationship between cognitive capabilities and the magnitude of mispricing in experimental asset markets remains to be an exciting topic. Various studies find that individuals with higher cognitive ability are different from individuals who score low on this dimension in a number of ways. Typically, they have a longer life expectancy, higher income, longer working memories, quick reaction times and are more prone to visual illusions (Jensen, 1998). We used the Cognitive Reflection Test or CRT to measure subjects' level of cognitive ability (Frederick, 2005). The CRT is associated with general measures of intelligence along with essential decision-making aspects in the form of preferences for time and risk (Frederick, 2005) and reasoning levels (Branas-Garza et al., 2012). Oechssler et al. (2009) showed that high CRT subjects are less associated with incidences of behavioural biases such as conservatism and conjunction fallacy in probability updating. We expect that the influence of the cognitive prowess will be weaker in markets where traders are more conservative, i.e. in an environment where traders trade more cautiously. The feeling of being responsible for someone else's welfare should make the traders more circumspect, which in turn should limit the size of the price bubble in the corresponding markets. Following Corgnet et al. (2015), we conjecture that in a cautious environment, it will be more difficult even for a smart trader (a high CRT subject in our context) to take advantage of ones with lower cognitive ability. We expect this phenomenon to be translated into a lower earnings dispersion between these two groups, high CRT traders and low CRT traders.

Following hypotheses are proposed based on previous discussions:

H1: We expect the price bubble to be smaller in markets with responsibility and the smallest where traders do not have any monetary incentives.

H2: The association between CRT and profit is expected to be weaker in the absence of monetary incentives.

4. Asset Market Design, Treatments, Data, and Bubble Measures

4.1 Market design

The design of our experiment is built on the seminal work of SSW. Many studies used SSW design in order to understand the association between different treatment variables and asset market price stability. Many of those studies produced valuable insights that helped us to understand the behaviour of economic agents and the factors governing asset market bubbles. Our study follows the SSW market design to check whether bubbles in experimental asset markets are dampened when traders are responsible for their decisions. We needed a market design that produces price bubbles under baseline conditions. A typical SSW market is characterized by an asset with declining fundamental value and a non-constant asset to cash ratio. Prior studies showed that subjects often find the declining fundamental value feature confusing, which eventually leads to bubble formation (Nuzzo and Morone, 2017). Although this is one of the drawbacks of SSW market design, we can use this feature to our advantage. Declining fundamental value and a non-constant asset to cash ratio features are expected to generate bubbles as documented by many studies in the literature. This makes the SSW market design an ideal choice as we want to test the influence of responsibility on price stability. In a classic SSW market, prices start well below the intrinsic value of the asset and surpass the fundamental value long before the mid-point of the asset's life, which generates a bubble pattern. The initial undervaluation of the traded asset is followed by and an overvaluation. The bubble reaches the peak typically in the final quarter of the asset's life before it crushes, abruptly. The phenomenon has been replicated by many works in the literature of laboratory asset market (see Palan, 2013).

In the laboratory markets, traders have the chance to buy or sell an asset which pays stochastic dividends. Our markets have a finite life with a predetermined number of periods. The value of each asset is zero at the end of the prespecified number of periods as there is no terminal buy-out for shareholdings upon market completion. The distribution of dividends in our experiment is a piece of common knowledge, i.e., traders know that actual dividends are drawn from a publicly known distribution. Since the dividend process is common knowledge, subjects can calculate the fundamental value of the asset at any point during the experiment.

We implement SSW type markets with nine traders for each session where trading takes place for 15 periods. Each period lasts for 120 seconds. The structure of each market is indistinguishable to that of "design 4" defined in SSW (1988) seminal paper. The total number of shares in circulation at any point of the experiment is 36, i.e., nine traders trade 36 shares in 15 double-auction periods. Traders receive dividends against each of the shares they are holding when a period end. Payoffs were denoted in experimental currency units (ECU), where 125 ECU equals 1 British pound. Dividends are randomly determined at the end of every period from the following distribution: 0, 8, 16, or 40 ECUs.[35] Traders can see the dividend amount on the screen before moving on to the next period. Each number is equally likely to occur and therefore the expected dividend in a period is ECU 16. Since this information is common knowledge, traders can quickly determine the fundamental value of the share in every period. Given an expectation of ECU 16 in every period, the intrinsic value of a share in period t equals 16*(16 – t), i.e., ECU 240 in the first period, 224 in the second period, ... and 16 at the beginning of the final period (i.e., period 15). Each trading lasts for 120 seconds. At the beginning of period 1, a trader will be assigned to a group of 9 traders. Each one of the traders receives an equal amount of money or cash (ECU 280) and an equal number of shares (4 shares) at the beginning of the experiment. Traders can use the money to purchase shares. The value of the asset (share) will be 0 after receiving the final dividend payment, i.e., the dividend at the end of period 15.

[35] In our experiment, the money is expressed in ECU.

We treat each market session as an independent observation for statistical analysis purposes. In order to compare treatments, six observations from each treatment are taken to conduct a statistical test. For example, we calculate significance levels based on the Mann-Whitney U tests where the unit of observation is a bubble measure or some other measures associated with a single market session. We employ six bubble measures in this study. In order to verify whether or not there is a treatment effect, we calculate Mann-Whitney U test p-values and t-test p-values where the unit of observation is the relevant bubble measure (significance level is based on two-sided tests unless it is mentioned). For instance, the amplitude is one of the bubble measures we use in our analysis. The amplitude value is calculated for each market (i.e. session) for comparison purposes before employing the Mann-Whitney U test or t-test.

4.2 Treatments

The experimental study analyses three treatments: *baseline*, *variable*, and *fixed*. Table A summarises market and payoff characteristics in different treatments. The trading procedure is the same across all the treatments, but traders get different types of incentives in different treatments. In the *baseline* treatment, traders trade on behalf of themselves, i.e., they are not responsible for others' payoff. In the other treatments (*fixed* and *variable*), the traders represented the economic interests of someone else as well as his/her own. Subjects were randomly assigned the roles of traders and passive investors in fixed-payoff and variable-payoff treatments. In a session in these two treatments, 18 subjects participated, with half of them being assigned the role of traders and the other half the role of passive subjects. Traders and passive subjects were randomly and anonymously matched at the beginning of a session and remained paired for the entire experiment, which lasted fifteen 120-second long periods. As discussed, we ask each subject to pick a number (which is used as an ID) from a box. By picking a random number, they determine their role in the experiment. We make sure that subjects do not see the number before they pick one. If a subject picks a number between 1 and 9, he or she is given the role of a trader. If the number is greater than 9, subjects assume the role of a passive member.

Table A compares these three treatments in our experimental study. In the *baseline* treatment traders are not responsible for others payoff. On the other hand, in the *variable* treatment and *fixed* treatment traders trade on behalf of another subject (a passive subject) in the lab and receive a variable payoff and fixed payoff respectively. It can be noted that the interests of the trader and the passive subject in the group are perfectly aligned in the *variable* treatment, i.e. payoff for the trader and the passive member in *variable* treatment sessions is the same. In the variable-payoff treatment, the trader is choosing for both himself/herself and the passive member, and in the fixed-salary environment, traders are choosing for others. The role of responsibility is more prominent in the *fixed* treatment compared to that of variable treatment due to the absence of any monetary incentives. The feeling of being responsible for others' payoff is the only thing that incentivises the traders in this environment.

Table A: Market and payoff characteristics in different treatments.

Treatments	Number of traders in a session	Number of passive subjects in a session	Number of periods	Total shares in circulation	Length of a period (seconds)	Expected dividend per period	Salary structure of the trader	Market design	Average Earnings (ECU)*
baseline	9	0	15	36	120	16	Equal to the end value of the wealth	SSW	1330.67
variable	9	9	15	36	120	16	Equal to the end value of the wealth	SSW	1224.00
fixed	9	9	15	36	120	16	Fixed	SSW	1005.33

*Average earnings in the fixed treatment is actually the amount a trader earned for the passive member in his group. The traders in this treatment gets a fixed amount (ECU 1100) for performing the tasks on behalf of the passive subject.

Subjects who participated in buying and selling of shares are the traders. The trader knew he or she is trading on behalf of someone sitting in the same room. A passive participant does not trade shares. Traders (i.e. subjects who traded on behalf of a passive subject) were made aware of the fact that they are responsible for another passive member in the lab room and the passive member's final payoff depends entirely on how they perform during the experiment, i.e., passive members are dependent on their respective traders. The Passive member's payoff depends on how much wealth the trader (in the same two-person group) can create for him/her, thus making passive member's payment entirely dependent on the trader's performance. Both members (the passive member and the trader) in a group in variable payoff with responsibility markets will earn the same amount of money from this experiment. Passive players' payment from this experiment is computed based on the wealth level of the trading account managed by the trader at the end of Period 15. Subjects in the *variable* treatment are given the following instructions:

Both members (the passive member and the trader) in a group will earn the same amount of money from this experiment. ***The payment associated with a group depends entirely on the actions of the trader, thus making passive member's payment completely dependent on trader's performance.***

There is plenty of evidence that individuals are concerned not only about their own payoff but also about the payoff of other individuals in their social circle (Fehr and Schmidt, 2010). Numerous experimental works showed that individuals pay attention to the fairness of payoff distributions and do not like being worse off than other people (Rohde and Rohde, 2011). Bolton and Ockenfels (2010) studied risk-taking in a social context. They showed that subjects' perception of payoff inequality could have a significant influence on how they make risky decisions on behalf of others. It was important that the payoff traders in treatment *fixed* received was not perceived as unfair or too generous. A trader gets a fixed salary of ECU 1100 for carrying out the trading activities on behalf of the passive member of his/her group in the fixed payoff with responsibility markets. There is no reason to believe that the fixed payoff is not high enough to eliminate any perception of inequality ex-ante. The amount, although arbitrarily determined, is not dissimilar to the average amount earned by the passive

members and the traders in *variable* treatment (the average is ECU 1224.00) as well. The amounts are statistically the same according to a two-sided Mann–Whitney U test (we use session or market averages for this comparison). Similarly, the difference between the mean value associated with the amount earned in the baseline treatment (ECU 1330.67, the largest among three treatments) and the mean for the fixed salary with responsibility (ECU 1005.33, the lowest among three treatments) is statistically insignificant. We use each subject's wealth level at the end of the experiment as the relevant unit of interest. Comparison based on market or session averages also implies statistical insignificance. Based on the evidence, we are fairly confident that trading pattern in treatment *fixed* was not affected by any feeling of payoff inequality. It is highly unlikely that ECU 1100 was perceived as more or less favourable by fixed-salary traders.

The instructions given to subjects in treatment *fixed* include the following:

A trader will get a fixed salary of ECU 1100 from this experiment for carrying out the trading activities on behalf of the passive member of his/her group. The Passive member's payoff depends on how much wealth the trader (in the same two-person group) can create for him/her, thus making passive member's payment entirely dependent on the trader's performance. Passive players' payment from this experiment is computed based on the wealth level of the trading account managed by the trader at the end of Period 15.

We emphasise on the fact that the trader is solely responsible for the economic wellbeing of the passive member in both responsibility treatments.

4.3 Bubble Measures

It is difficult to identify a precise and commonly recognised definition of asset market bubble, and for that reason, the study uses various bubble measures following the literature on experimental asset markets. According to Stöckl et al. (2010), a general definition could be the following: "trade at significant volume at prices considerably deviating from fundamentally justified prices." Experimental setup, nevertheless, does present the interested researcher one opportunity not available in a

real-world market. Computing bubbles in real markets are complicated since it is challenging to determine the fundamental value of the asset. Moreover, the values would vary depending on the technique followed. In general, a bubble is hence only known subsequent to a burst. An experimental environment can be an ideal setup to gain valuable insights on asset market bubbles. The laboratory setup gives the researcher opportunities to precisely determine the fundamental value (FV) of the asset under consideration and therefore contrasts between the (exogenously derived) intrinsic value, and the (endogenously derived) trading prices are straightforward. To facilitate a formal comparison among three markets (six markets for each treatment), we borrow the knowledge from the literature in order to compute a variety of popular measures of mispricing.

In order to quantify treatment variations, the study uses a number of measures of bubbles (see Haruvy and Noussair, 2006; Stöckl et al., 2010). Some measures are reliant on average prices, and some of them used median prices. We do that to avoid any bias that might be associated with just one of the approaches (mean or median values). *Amplitude* measures the degree to which the value of the share or asset changes relative to the fundamental value. We follow the Porter and Smith (1995) definition of *Amplitude*, where the maximum and minimum price deviations are assessed concerning the initial fundamental worth of the share. The following is the measure:

$$A = MAX\left\{\frac{P_t - f_t}{E} : t = 1 \ldots .15\right\} - MIN\left\{\frac{P_t - f_t}{E} : t = 1 \ldots .15\right\}$$

Here P_t stands for the mean market price of the share in period t and f_t is the fundamental value of the asset in period t and E represents the estimated value of the dividend over the share's lifespan.

Average Bias calculates the mean of the deviation from the intrinsic value (based on the median price of the share each period). *Average Bias* is a popular way of measuring overpricing (Eckel and Füllbrunn, 2015). A positive (negative) Average Bias shows whether the price is higher or lower compared to the intrinsic value of the asset. A smaller value for Average Bias suggests that overpricing is less prevalent.

Total Dispersion measures the absolute deviation (periodic) of prices from the fundamental value. Following Haruvy and Noussair (2006), we use the median prices for a given period. We calculate the *Total Dispersion* by taking the 15-period sum of the absolute deviation. Unlike *Average Bias, Total Dispersion* computes the overall variability of the asset price compared to the fundamental value. A substantial value for *Total Dispersion* is suggestive of the fact that the overall distance from the fundamental value is very significant. We also used the Normalized average price deviation (NAV) as a bubble measure. NAV calculates the absolute deviation between the mean of observed share value and the intrinsic share price for each of the fifteen periods. It is defined as follows:

$$NAV = \sum_{t=1}^{15} \frac{|P_t - f_t|}{15}$$

Here P_t is the mean market price of the share in period *t* and f_t is the fundamental value of the asset in period t.

We also calculated *Relative Absolute Deviation* (RAD) and *Relative Deviation* (RD) to measure the size of the bubbles across treatments. The measures are developed by Stöckl et al. (2010). RAD measures the absolute price deviation and RD measures the raw price deviation compared to the intrinsic value of the share for each period. Following Stöckl et al. (2010) the study calculates RAD by taking the mean absolute differences between the volume-weighted average price and the intrinsic value for all 15 periods and normalising it with the absolute value of the mean fundamental value of the market. We then calculate RD to measure the extent of overvaluation or undervaluation. We take the raw difference between (volume-weighted) average prices and fundamental values in order to calculate the RD. Since raw differences rather than absolute differences are used in this measure, we can see whether the mispricing is resulting from an overvaluation or an undervaluation. Finally, we calculate the *Volume*, which is the standardised measure of the quantity of trading activity in the market. We take the sum of all the transactions that took place in the experimental market for a given period to calculate per-period *Volume*.

4.4 Data collection in the laboratory

The study is based on experimental data collected in the United Kingdom. Subjects were randomly invited from a pool of university students to participate in the laboratory experiment. We recruit all our participants from a major university located in the UK. Two hundred and sixteen subjects from that university participated in the responsibility with a variable salary (*variable*) treatment and responsibility with a fixed salary (*fixed*) treatment. We run experimental sessions at the CRIBS lab at the University of Nottingham. We recruit subjects using ORSEE (Greiner, 2015); the experiments were computerized using zTree (Fischbacher, 2007). We ran 12 sessions with 18 subjects each for the responsibility treatments. We also compared our results with results from six baseline markets where traders trade individually. We conducted experiments for three of those baseline markets and borrowed the rest of the data (three more markets) from another PhD student. The laboratory environment and the instructions were identical for both cases. Instructions – taken basically from Haruvy and Noussair (2006) and Haruvy el al. (2007) – were read aloud in the experimental lab to the audience (i.e. subjects in the laboratory) at the beginning of the experiment.

5. Results and Discussions

5.1 Initial impressions of price patterns

Figure 1 pictured the times series paths of transaction prices in different treatments. Median prices in each period are used for this graph for six markets associated with each treatment. The upper panel of Figure 1 illustrates the mean of median session prices by period for *baseline*, *fixed*, and *variable* markets. The lower panel (Panel B) of Figure 1 summarizes the price trends for individual asset markets. The fundamental value is represented by the bold diagonal line, which is declining over time. The fundamental value (FV) drops by ECU 16 every period, and the value is zero when period 15 ends.

We observed that bubbles occur in each treatment: *baseline* treatment in particular and *variable* treatment (variable salary with responsibility) to a smaller degree. Both these treatments demonstrate a bubble-and-crash pattern roughly comparable to what

is detected in various studies in experimental asset markets (e.g. Eckel and Füllbrunn, 2015). In the *fixed* treatment, prices rise above intrinsic value, mainly in the second half of the assets' life, but the magnitude of the discrepancy is comparatively much smaller, and prices track intrinsic values closely compared to the *baseline* markets. The figure (Figure 1) shows that markets did not value the share very highly in *fixed* sessions and the difference between the values appears substantial when compared with observed transaction prices in *baseline* sessions. Nevertheless, prices appear not to follow the fundamental value of the asset in all three types of markets. In *baseline* treatment, prices cross the fundamental value very quickly and remain at levels higher than the fundamental value for a longer duration. In contrast, in *fixed* markets, it takes a while for prices to cross the intrinsic value threshold. We also used averages of the mean session prices (untabulated) by period for three types of markets and found very similar patterns. We did not find any inconsistency in times series routes of transaction prices in *baseline, variable,* and *fixed* markets. Figure 1 remains robust to portraying average instead of median transaction prices. We found overpricing to be significantly higher in *baseline* markets and lower in other markets when the comparisons are based on mean session prices rather than median prices. As discussed before, some of the bubble measures employed in our study use mean prices rather than median prices to avoid biases.

It is essential to understand that traders in *fixed* markets are entirely free of any economic concern, and therefore the only tool that can act as motivation is being responsible for others' wellbeing. The *variable* markets, on the other hand, does not wholly remove the self-interest component, so the motivation is not purely altruistic. The absence of self-interest, coupled with responsibility, is expected to lead to low-risk trading behaviour. Which, in turn, should lead to smaller bubbles in fixed salary markets where traders are responsible for someone else's payoffs. The pattern that is visible on the surface is that *fixed* markets are significantly different from the *baseline* markets in the first half of the markets' life span. We compute both the volume-weighted average prices and median prices per session and treatment. The resulting prices for each treatment, together with the asset's fundamental value are shown in Figure 1. We observe that in fixed-salary with responsibility treatment asset

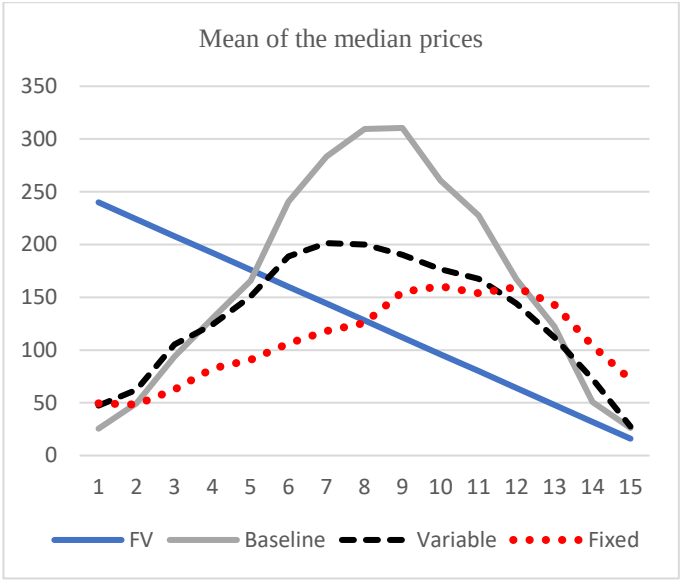
prices are lower than in variable salary with responsibility and in baseline markets. As shown in Table 3, in four out of the six fixed salary with responsibility sessions peak prices did not exceed even 50% of the highest possible dividend realization (i.e. ECU 600). We also observe that the average price deviation from fundamental value is the smallest in the *fixed* treatment and highest in the *baseline* treatment (see Table 2). Responsibility in the absence of economic incentives (since salary is fixed) seems to have a considerable effect on prices.

We also investigate the distributions of prices at which shares were traded and the price deviation from the corresponding fundamental values across different treatments. The summary statistics are presented in Table 1 and Table 2. It can be seen that the dispersion of prices is the highest in the *baseline* treatment. On the other hand, we observe the lowest dispersion in the *fixed* treatment. As shown by Figure 2, prices are most dispersed in the *baseline* treatment and least dispersed in *fixed* treatment. We looked at the price dispersion by session, measured by the standard errors of trading prices (see Table 1). Compared to the *baseline* treatment price dispersion is significantly lower in the *fixed* (two-sided Mann–Whitney U or MWU test *p-value* = 0.004) and *variable* treatment (two-sided MWU test *p-value* = 0.004). We also observe that the dispersion is marginally significantly lower in the fixed payoff environment compared to the variable payoff environment, two-sided MWU test *p-value* = 0.078. The unit of observation in MWU test statistics are session values unless it is mentioned.

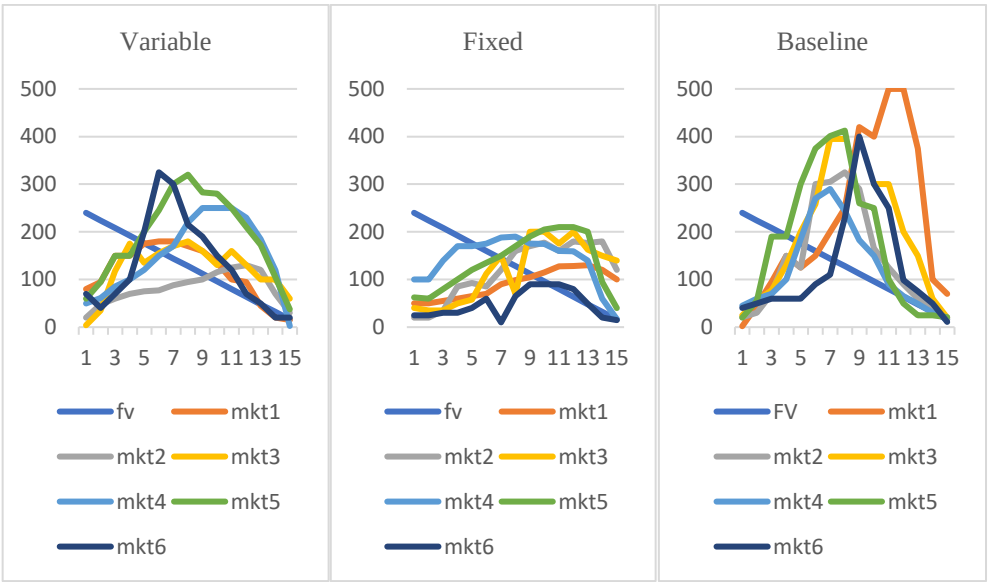
If the level of price volatility is an indication of market efficiency, it can be suggested that mispricing was less pronounced in the *fixed* treatment and highest in the *baseline* treatment. We observe a similar pattern when analysing price deviation from the periodic fundamental value of the share. The results are reported in Table 2. Once again, treatment *baseline* produced the highest average standard errors and *fixed* is found to be associated with the lowest average standard error figure. The

Figure 1: Price trajectory. Mean of the median market prices are used for this graph. Per period values are calculated for each treatment. Trajectories of median transection prices in *baseline*, *fixed*, and *variable* treatments are presented along with the fundamental value. Median of the transactions prices for individual markets are also presented.

Panel A



Panel B



difference between averages associated with these two treatments is statistically significant (two-sided MWU test *p-value* = 0.055). The difference is also significant when we compare the mean values associated with the *variable* markets and the *baseline* markets (two-sided MWU test *p-value* = 0.016).

Insert Table 1 and Table 2 from Appendix III here

The study looks into the price peak data for the treatments as well. As shown in Table 3 that the distribution of the peak prices for the baseline markets is significantly different from both types of responsibility markets. We observed the highest average price peak in the baseline treatment with an average value of ECU 441.67 and lowest in the *baseline* treatment with a value of ECU 240.83. The baseline peak price distribution is significantly different from the distribution associated with the fixed markets; the two-sided MWU test p-value is equal to 0.020. The difference is slightly less pronounced (marginally significant though) when we compare the averages of baseline markets and variable markets (with an average of ECU 325.67) with a corresponding two-sided MWU test p-value of 0.089. These are indicative of the fact that prices are significantly lower when traders are responsible for someone else's economic interests.

Insert Table 3 from Appendix III here

5.2 Bubble Characteristics in different market environments

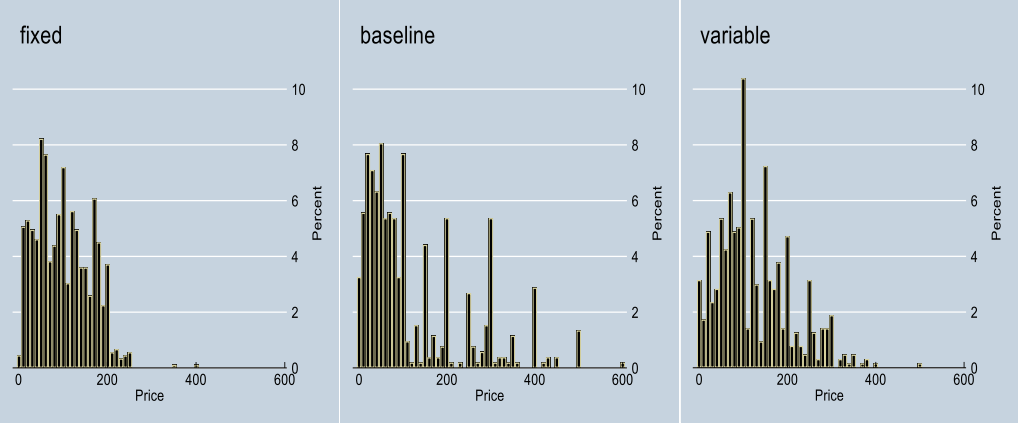
The study employs various bubble measures to check for the treatment effect on price dynamics in experimental markets with declining fundamental value. A bubble is typically formed when the prices deviate positively from the intrinsic value of the asset. Therefore, an *Average Bias* with a positive value, higher value for *Total Dispersion*, a positive value for RD, a higher value for *Amplitude*, bigger RAD and NAV figures are indicative of price bubbles. In this part of the analysis, we compare and contrast different treatments based on the values calculated for various bubble measures. Six observations are taken from each of the three treatments for comparison purposes. We use both Mann-Whitney tests p-values and t-test p-values for an unequal variance for between treatments comparisons.

Observation I: Responsibility with fixed payoff: overpricing is significantly lower in markets where traders receive a fixed amount for trading on behalf of another individual in the room.

We analyse the differences between fixed-payoff with responsibility markets (i.e. where traders receive a fixed amount in exchange for the trading tasks she performs

on behalf of another person in the lab) and *baseline* markets. Here we analyse and compare price efficiency and magnitude of bubbles based on different bubble measures following the literature. The results associated with the bubble measures are consistent with the initial findings that overvaluation of the asset is less prevalent in markets with responsibility. We computed the price *Amplitude* following Porter and Smith (1995) for corresponding markets to check the size of bubbles in each treatment.

Figure 2: Price dispersion in different treatments



As anticipated, *fixed* markets produced the smallest *Amplitude* value and *baseline* markets the highest. As shown in Table 4, the average of amplitudes in the *baseline* markets is more than 60% higher than the average figure associated with *fixed* markets. The difference is statistically significant with a two-sided MWU test *p*-value of 0.007 and two-sided t-test *p*-value of 0.007, suggesting a significant effect of the combination of responsibility and the absence of monetary incentives. The treatment average of *Average Bias* is the lowest and negative in *fixed* markets and equals -19.23 among the three treatments. Table 5 reports the relevant values associated with this measure. The value is negative in 4 out of 6 fixed-salary markets under responsibility. The baseline treatment markets produce the highest *Average Bias* mean value and is positive in 4 sessions. The baseline mean value equals 32.80.

Insert Table 4 from Appendix III here

The average for *variable* markets, on the other hand, is somewhere in the middle and very close to zero with the value 3.39. The mean *Average Bias* for the baseline markets is significantly higher than the mean value of *fixed* markets with responsibility at a 5 per cent significance level (two-sided MWU test $p = 0.037$; two-sided t-test, $p = 0.031$). The results are presented in Table 5. This is suggesting a strong responsibility effect in trading behaviour of the subjects, i.e. overpricing under responsibility is significantly reduced. The between-groups comparison of responsibility effects without the presence of any monetary incentive based on Relative Deviation (RD) yields similar outcomes. Once again, the difference is very significant and suggestive of the fact that overpricing is much less prevalent in the presence of responsibility and the absence of any monetary incentive. Consistent with our hypothesis, the fixed-salary markets with responsibility produced a lower RD compared to the *baseline* average (two-sided MWU test, $p = 0.010$ and two-sided t-test, $p = 0.018$).

In addition to *Amplitude*, *Average bias*, and *RD*, we employed *Total Dispersion (TD)*, *RAD*, and *NAV* to supplement our analysis. Figure 1 shows a negative deviation between the observed prices and the fundamental value in the fixed-salary sessions, which cancel outs *Amplitude*, *Average bias*, and *RD* as appropriate bubble measures to analyse treatments if the researcher is more interested in the overall price efficiency in different environments. Measures such as the *RD* and *Average Bias* are more informative about the direction of price deviation. The study, for that reason, investigates the values associated with these measures of price deviation from the fundamental value (*Total Dispersion (TD)*, *RAD*, and *NAV*). We find that mispricing wasn't wholly eradicated in the *fixed* markets. Mispricing persists, but the direction was different compared to the *baseline* markets. These markets are characterised by undervaluation in the first half of assets' life. Since overpricing was much more prevalent in baseline markets, we find statistically insignificant results for measures of absolute price deviations (i.e. *TD*, *RAD*, and *NAV*). It is observed that the price peaked well into the final third of the asset's life for the *fixed* markets. From Figure 1, it can also be noted that price followed the fundamental value more closely during the first half of the experiment under responsibility with a fixed salary. So, we

computed all the bubble measures, especially the measures of absolute price deviation, for the first ten periods of the shares' life. The 10-period treatment average results are presented in Table 6. The average *TD*, *RAD*, and *NAV* for the *fixed* markets are significantly lower than the averages for the *baseline* markets based on a t-test with p-values below 0.01 for every case. The difference for the last third of the assets' life, on the other hand, was insignificant. The results are suggesting that fixed-salary markets under responsibility track the fundamental value more closely compared to the *baseline* markets in periods 1 through 10 with a very high level of significance.

Insert Table 5 and Table 6 from Appendix III here

Observation II: Responsibility with variable salary: Bubbles are marginally smaller in markets where traders receive a variable amount for trading on behalf of another individual in the room.

The bottom left panel of Figure 1 presents the price paths of *variable* markets and the black dashed line in the top panel of Figure 1 shows the average of the median prices for the treatment. It can be noted that the price path crosses the fundamental value threshold roughly at the same time point as in the case of the baseline markets. We then observe the daylight between these two types of markets to become more significant during the mid-life of the asset. This results in a smaller bubble for the variable-salary with responsibility market compared to the *baseline* markets. Unlike the *baseline* markets, we do not observe a sudden crash of bubbles in this environment. Responsibility seems to take effect in the variable-salary environment, but the effect is not as strong as we can see in the fixed-salary environment.

There is some evidence that the magnitude of a bubble, i.e., the extent of mispricing, in markets where traders are responsible for someone else's economic well-being is smaller compared to baseline markets. The 10-period average *TD*, *RAD*, and *NAV* for the *variable* markets are significantly lower than the corresponding baseline mean values based on both MWU test and t-test; two-sided p-values are reported (see Table 6). The significance level for all three measures based on a two-sample t-test (assuming unequal variances) is at least 1%. The pattern is suggestive of the fact that prices under responsibility, even in the presence of

monetary incentives followed the intrinsic value more closely for a significant amount of time. Bubbles were significantly smaller in fixed salary markets with responsibility in the first half of the experiment compared to markets in the *baseline* treatment. The difference between *baseline* and *variable* treatment for this particular measure, however, is insignificant, suggesting that the impact of responsibility is somewhat reduced when traders have some self-interest on the outcome of their trading activities. We find some evidence that the size of the bubble in the *fixed* markets is slightly smaller than the bubble in the *variable* markets in the first half (as well as in the first ten periods of the asset's lifespan). The level of significance is less than 10% (Welch p-values) when compare these two treatments. We also compare treatments based on three phases of a market: the first five periods, five periods in the middle, and the last five periods. The magnitude of bubbles in the middle phase of the market is significantly higher for *baseline* markets compared to both responsibility markets. As before, the difference is more prominent when we compare *baseline* markets with *fixed* markets. We find a very similar pattern when we compare *variable* markets with *baseline* markets. The five-period average *TD, RAD,* and *NAV* (five periods in the middle) for the *variable* markets are significantly lower than the corresponding baseline mean values based on both MWU test and t-test (two-sided).[36] We find the same pattern when the comparison is based on the first eight periods of a market. We divided the assets life into two phases, first eight periods and last seven periods.

5.3 Cognitive ability and profits in fixed and variable environment

Observation: Profit made by high CRT traders is significantly lower in fixed salary markets under responsibility.

The study explores the link between traders' cognitive ability as measured by the CRT score and the magnitude of profit. The test (Frederick, 2005) is a task of an algebraic nature consisting of three questions, intended to evaluate the capability to overrule an instinctive response that is wrong and to involve in further thinking that guides them to the right answer.

[36] Untabulated results.

The CRT has been a popular approach to the measure of subjects' cognitive ability which is used to rank participants accordingly. We use scores (0, 1, 2, & 3) on the CRT and compare those with subjects' earnings across treatments. Corgnet et al. (2015) show that subjects with high CRT scores earn more money by making smart exits before the bubble would crash. They showed a significant and positive association between subjects' cognitive ability as measured by CRT scores. Bosch-Rosa et al. (2018) showed that cognitive ability of traders could play a significant role in determining price efficiency in experimental asset markets.

We could not replicate the results obtained by Corgnet et al. (2015) in fixed-salary markets under responsibility. The association between CRT scores and income holds for the variable payoff treatment (i.e. subjects with higher cognitive ability earns more for themselves and their passive group member) as well as the *baseline* markets where traders are not responsible for others but is insignificant in the fixed salary markets. We collected data on subjects' CRT scores for all the responsibility markets and three *baseline* markets, which means that we had 54 observations for each of the responsibility treatments and 27 observations for the baseline treatment. Panel C of Table 7 reports the Pearson correlation coefficient between traders' CRT scores and the level of wealth (i.e. the amount of money they generated for themselves and/or for the passive member). The relationship appears to be the strongest in the baseline treatment, with a correlation coefficient of 0.51 (p-value = 0.006). The link is also positive and significant for the *variable* markets as manifested in a correlation coefficient of 0.37 (p-value = 0.005). The correlation coefficient is not significant even at the 10 per cent level for the fixed markets.

Traders with a positive CRT score earned on average ECU 1435.70 in the variable salary with responsibility environment compared to ECU 763.24 earned by traders with a CRT score of 0. As shown in Panel B of Table 7, the difference is statistically significant (two-sided MWU test, $p = 0.040$). The MWU test unit of observation is the wealth level associated with a trader in the treatment. The difference is also big and significant for the *baseline* markets. Positive CRT subjects in three baseline markets for which we have CRT data earned on average ECU 1882.22 compared to a mere ECU 659.56 earned by traders with a CRT score of 0. Since we had a smaller sample

for the baseline markets, the two-sided MWU test p-value is marginally significant (p-value = 0.080). The difference, on the other hand, is smaller in the fixed salary with responsibility environment. The wealth accumulated for the passive members by traders with a CRT score higher than 0 is ECU 1022.64, which is not significantly different from the average wealth amount (ECU 944.75) generated by traders with a 0 CRT score (two-sided MWU test, $p = 0.448$) (see Table 7).

Insert Table 7 from Appendix III here

We investigated whether there was any link between subjects' cognitive ability and their trading patterns, especially in *fixed* markets. It could well be the fact that high CRT traders did not bother to trade or exert efforts in the absence of any monetary incentive. Based on a subject level comparison, we did not find any evidence that suggests traders with high cognitive sophistication in *fixed* markets were shirking. The results are reported in Panel D and Panel E of Table 7. Subjects with a CRT score of 0 or 1 on average traded 33.48 times whereas subjects with a CRT score of 2 or 3 traded 32.09 times. The difference is statistically highly insignificant based on a two-sided MWU test and a two-sided t-test. The MWU test unit of observation is the number of times each subject traded during the experiment. There were 31 traders in the low CRT group and 23 traders in the high CRT trader group. The difference remains to be statistically insignificant when we compare lowest CRT score group (subjects with score 0) with the highest CRT score group (subjects with score 3).[37] So it is clear that traders' cognitive sophistication did not influence their trading activities. The difference is also insignificant for the *baseline* and *variable* markets.

We then run OLS regressions in order to confirm that our initial findings on the impact of cognitive capability on earnings in different responsibility environments are robust. The results are presented in Table 8. We present results associated with three specifications. In all specifications, the dependent variable is the net profit generated by the traders by the end of period 15, and the key independent variable is the CRT score. In order to calculate net profit associated with a trader, the initial endowment

[37] There were seven subjects with a CRT score of 3 and 12 subjects with a CRT score of 0. MWU test p-value for the comparison is greater than 0.10.

(ECU 280) and the fair value of the shares initially received by the trader (ECU 240*4 = ECU 960) are deducted from the level of wealth in trader's account at the end of the experiment. We also control for gender and subjects' level of risk aversion.

The coefficient on CRT is statistically significant (p-value < 0.024) for the *variable* treatment and for the *baseline* treatment (p-value < 0.037). The coefficient, however, is statistically insignificant for the *fixed* treatment. The coefficient is economically more significant in the *baseline* markets. The results suggest that it was more difficult for the high CRT traders to take advantage of low CRT traders in the *fixed* markets. It is possible that traders were more conservative in the fixed treatment due to an increased level of responsibility. It seems traders behaved more cautiously in the fixed-salary regime, which reduced the earnings dispersion between the high CRT group and low CRT group. The results support our initial results that responsibility reduces risk-taking, i.e. responsibility increases risk aversion.

Bosch-Rosa et al. (2018) argued that heterogeneity in traders'' cognitive ability is the reason behind bubble formation and earnings dispersion. Our results, however, is in slight disagreement with that conclusion. We showed that the bubble could be somewhat dampened, and earnings dispersion can be reduced to an insignificant level in markets populated by both high and low CRT traders by incorporating responsibility and removing monetary incentives. Our results are suggestive of the fact that the particular experimental design can subdue the influence of cognitive ability on price dynamics; in our case, it is the combination of incorporation of responsibility and the absence of monetary incentives.

Insert Table 8 from Appendix III here

5.4 Robustness checks

We checked whether the lower magnitude of overvaluation in the *fixed* treatment has resulted from a lower level of trading activities in those markets. It could be argued that since traders who trade on behalf of others in exchange for a fixed salary will be less inclined to exert efforts. It is observed that the level of trading in the fixed markets

is in fact, the highest among the three treatments (untabulated). This phenomenon supports the notion that less overvaluation observed in *fixed* markets is not due to inactivity rather a reflection of responsible actions taken by traders operating on behalf of the other member in the group. We compare session averages associated with all treatments. The average volume (i.e. the number of shares traded in a period) in *fixed* markets is 9.9, which is higher compared to *variable* markets (volume is 7.1) and baseline markets with the figure 5.7. The difference is statistically significant compared to the baseline markets' average with a p-value less than 0.05 (based on two-sided MWU test).

We also compare our results with the results in Baghestanian et al. (2017). In their experimental setting, investors can entrust their money to traders and traders do not have full freedom regarding how much money they can spend for trading purposes. The study mainly focused on how compensation schemes affect two key asset market features in the form of liquidity provision and asset prices. The key difference between their study and our study is that whereas we emphasized on the fact that traders are entirely responsible for the passive members, they mainly focused on compensation schemes of the traders. Moreover, traders in our responsibility treatments are allowed a greater magnitude of freedom in terms of how much of the wealth they can use to conduct trading activities. The results associated with all the treatments in Baghestanian et al. (2017) shows more significant overvaluation compared to the variable- and fixed-salary treatments in our study (see Table B). We use RD as the measure to compare our results with Baghestanian et al. (2017). According to Stöckl et al. (2010), RAD and RD can be used for comparison across treatments with different market designs and parameters since the measures: "i. relates fundamental value and price, ii. are monotone in the difference between prices, and fundamental values; iii. are independent of the absolute level of fundamental value and the number of periods. It is proposed that with these properties, mispricing/overvaluations can be compared across different market designs and treatments with different parameter settings (Stöckl et al., 2010, p-296)."

Since undervaluation is observed in both responsibility treatments, we base our comparison on RD as this measure delivers information on whether the mispricing

results from overvaluation or undervaluation of the share. We observed that the extent of overvaluation is much lower in both treatments compared to all the treatments in Baghestanian et al. (2017).

We also check whether specific individual attributes influence the price stability in our markets. Subjects in our experimental sessions were invited on a random basis, so any individual attribute is unlikely to become influential. We focus on three individual traits, risk aversion, gender, and cognitive ability that, according to various studies, can have an impact on price dynamics in SSW type markets.

We calculate the mean CRT score of subjects based on available data in each treatment. We have data on the cognitive ability for all the markets in responsibility treatments and three markets in *baseline* treatment. The study compares the session averages associated with all treatments. The mean values of the session averages for CRT score are essentially the same all the treatments. We find no CRT score difference between our variable and fixed groups (two-sided MWU test, p-value =0.816), baseline and variable groups (two-sided MWU test, p-value = 0.766), and baseline and fixed groups (two-sided MWU test, p-value = 0.619).[38]

The experimental work of Eckel and Fullbrunn (2015) while studying gender effect on price dynamics in laboratory asset markets attributed some of the discrepancies in observed price differences to risk aversion. The results in our study are unlikely to be due to risk aversion as we do not find any differences in risk aversion in traders across treatments. We computed the mean level of risk aversion of subjects for three treatments and found no significant differences. The information on the level of risk aversion is based on subjects' responses to a question from Dohmen et al. (2011), "How willing are you to take risks, in general?" Although we do not have data on all the baseline markets, it seems unlikely that subjects' attitude toward risk influences our results. The study compares the session mean values associated with all treatments. The two-sided MWU test (untabulated) for all the combinations - i.e. *baseline* vs *variable*, *baseline* vs *fixed*, and *fixed* vs *variable* - reveals that there are no differences in distributions of the level of risk aversion in subjects in different

[38] Untabulated results.

markets. The highest level of risk aversion is observed in the *baseline* markets with a score of 6.44, whereas the lowest level of in *variable* markets with a score of 6.92 (two-sided MWU test p-value = 0.290). A similar pattern emerges when we compare *baseline* markets to *fixed* markets (two-sided MWU test p-value = 0.474) and *fixed* markets to *variable* markets (two-sided MWU test p-value = 0.600). The level of risk aversion is lower in *fixed* markets (statistically insignificant). Since we do not have data on all the baseline markets, we calculated the Pearson and Spearman correlation coefficients (untabulated) between the mean and median risk aversion score for each market and corresponding bubble measures (*Amplitude*, *NAV*, *Average bias*, and *RAD*).[39] In none of the instances, the correlation coefficient is statistically significant. It seems unlikely that our results are influenced by differences in traders' level of risk aversion.

Table B: A comparison with Baghestanian et al. (2017). Session averages are used for comparison purposes. Treatments are compared by using market RD values as the relevant unit of interest.

	RD in different treatments			
	T1	T2	T3	T4
Baghestanian et al. (2017)	1.170	0.980	0.450	0.600
Variable	0.025***	0.025**	0.025**	0.025**
Fixed	-0.179***	-0.179***	-0.179***	-0.179***

***, **, and * indicates whether the corresponding difference between two RD figures is statistically significant at 1%, 5%, and 10% level based on two-sided MWU tests.

We also examined whether our results are influenced by gender-composition of the markets. Studies such as Eckel and Fullbrunn (2015) & Cueva and Rustichini (2015) showed a significant gender effect on asset market price instability. In order to avoid any gender bias subjects were chosen on a random basis. We checked the gender - composition of our markets. The proportion of Female subjects appears to be similar across treatments. We do not find any significant differences in gender-composition (untabulated results) between our *fixed* and *variable* markets (two-sided MWU test p-value = 0.564); *fixed* and *baseline* markets (two-sided MWU test p-value = 0.999); *variable* and *baseline* markets (two-sided MWU test p-value = 0.693). We also find

[39] We have risk aversion data for twelve responsibility (*fixed* and *variable*) markets and three *baseline* markets.

that the proportion of male or female not to be significantly different from 0.50 in all three treatments based on a one-sample test of proportions.

6. Generalizability of Experimental Results

The complexity associated with field data has led many researchers to adopt experimental methodologies to study different asset market phenomena. Due to the complicated nature of field data, it is often impossible for researchers to control for all the variables that are likely to be related when a specific asset market topic is studied. Many asset market variables that we observe in a natural set-up, i.e. in a real-world market, are uncontrollable, and for that reason, do not allow the researcher to draw conclusions reliably on different issues related to mispricing and bubble formation. For example, variables such as the fundamental value of the asset, lifespan of the asset, and information conditions are hard to be accounted for due to measurement difficulties. Researchers in this area have often been limited to these constraints. In a laboratory setting, on the other hand, experimenters can exogenously manipulate and see the critical market parameters. Asset market literature has greatly benefited from the use of a regulated lab environment. Our baseline results are almost identical to results in other studies that used similar asset market design. Thus, we are fairly confident about the internal validity of our findings.

Although conclusions from experiments have a high degree of internal validity, external validity remains to be a concern. Our study is no different. The treatment differences we observe might be due to a more simplified environment in the lab compared to actual markets and a sample of student traders. Nevertheless, it can be argued that even though students are not greatly representative of skilled traders in the market floors, they may be more representative of retail investors taking investment decisions, from those managing retirement portfolios to those drawn to contemporary trends such as cryptocurrency. Asset market experiments employing similar market design with market professionals and corporate executives produced bubbles in the past (see Palan, 2013 for a review). Furthermore, bubbles and crashes do happen in even the most sophisticated markets. Various research endeavours based on market

data and anecdotal examples such as the dotcom bubble crash and 2007-2008 market crash are evidence indicative of the fact that mispricing is prominent in financial markets. We are not going as far as suggesting that our quantitative results in asset market experiments have a very high degree of external validity. There is no reason to believe that the qualitative results are externally invalid, however.

External validity can be improved by analysing bubble formation in field experiments, for instance, experimental stock markets. This is beyond the scope of our study. In order to alleviate generalizability concerns, we use real cash as the experimental payoff and connect the payoff of the participants to their performance when it is required. Moreover, we recruit subjects from a renowned university in the UK and follow a random process to invite them. A significant proportion of the subjects in the experimental studies are from Economics, Business, Math, and Engineering background. What is more, an overwhelming proportion of our subjects were in the third year of their undergraduate study or studying at a higher level. Subjects are also given instructions before the experiment. The instructions were read out loud at a comfortable pace. Subjects were also given a chance to ask questions after the instruction to make sure that they did not start the experiment with a vague understanding of the experiment.

7. Conclusion

Several studies endorse the notion that individuals behave differently when they are looking after the economic interest of other individuals. The feeling of being responsible for a third party can act as a strong debiasing force and can shape the risk-taking behaviour of individuals (Leder and Betsch, 2016). The main objective of the study is to understand the influence of responsibility in experimental asset markets with assets with a finite but relatively long lifespan, i.e. whether bubbles are dampened in the presence of responsibility. The study uses the SSW (1988) laboratory market set-up to examine how social context affects decision making. We compare markets where traders are responsible for someone else's payoff with markets where traders are not responsible. We study two scenarios under responsibility: responsibility in the

presence of monetary incentives and responsibility in the absence of monetary incentives. We find smaller price bubbles in markets where traders are responsible but have monetary incentives, and the effect is stronger when traders do not have any monetary incentives, i.e., traders receive a fixed payoff for performing trading activities on behalf of others. In the fixed-salary treatment, the role of responsibility is even more significant as any monetary incentive no longer motivates traders. We observed that although the magnitude of bubbles is smaller in fixed-salary markets under responsibility, the price did not come down quickly to the fundamental value during the latter part of the experiment. The design of our study did not allow us to check whether increasing the length of the experiment, i.e. adding more periods the asset's lifespan, makes the price to come down closer to the fundamental value in the last third of the experiment.

The study incorporates responsibility by allowing the subjects for whom traders make choices (i.e., perform the trading activity) to be physically present during the sessions. Traders in responsibility treatments are informed that they are paired with a passive subject in the room. We find that the feeling of being responsible has an impact on prices dynamics, especially when responsibility is not coupled with monetary incentives, i.e. when responsibility plays a more prominent role. The amplitudes of the bubbles in our declining-value markets under responsibility are significantly smaller than those without responsibility. Interestingly, markets with fixed salaries where traders do not have any monetary incentives did not induce low trading activities compared to both baseline and variable salary markets, suggesting that traders took their job seriously. Finally, we show that traders with higher cognitive ability earns more than traders with lower cognitive ability in the variable salary with responsibility markets. We do not see the same pattern in the fixed treatment.

Our study extends the current literature along multiple dimensions. First of all, the study contributes to the literature on whether the feeling of being responsible for someone reduces risk-taking. Our results are in line with the prediction of Charness and Jackson (2009) and Bolton and Ockenfels (2010) that responsibility can induce a "cautious sift" in agents' behaviour. We document the influence of responsibility on

asset market price stability. Secondly, unlike some prior studies in the laboratory asset market literature, we explored a principal-agent set-up where the principal is truly passive. For example, Bao et al. (2017) and Giamattei et al. (2020) have setups where subjects are assigned to the role of analysts and traders. In Baghestanian et al. (2017), investors can entrust their money to traders and traders do not have full freedom regarding how much money they can spend for trading purposes. In those arrangements, both members bring something to the table, i.e. the non-trader member is also actively participating in the experiment. We also contribute to the discussion on the link between traders' cognitive ability and earnings. Noussair et al. (2014) and Corgnet et al. (2015) showed a positive association between CRT scores and earnings. Our study makes a novel contribution to this stream of literature by documenting the fact that the link between cognitive ability and wealth generation becomes insignificant when traders trade on behalf of others in the absence of monetary incentives. Our study is the first to shed light on the connection between traders' cognitive ability and monetary incentives.

It can be argued that the recent economic meltdown was mostly due to the competitive nature of the financial industry. We documented that overpricing mainly arise because of monetary incentives. Overpricing is significantly reduced when we remove the monetary incentive component and replace it with a non-pecuniary incentive (i.e. responsibility). Moreover, taking responsibility for a passive individual's wellbeing found to induce cautious actions, even in the presence of monetary incentives. There was no indication that subjects were taking their job lightly in responsibility markets. Although there were incentives, we did not observe shirking behaviour in responsibility treatments, particularly in *fixed* markets. Our work complements prior studies that documented the detrimental effect of monetary incentives on price stability (e.g. Kleinlercher et al., 2014). Policies and regulations aiming at preventing significant mispricing and disproportionately high level of risk-taking by professionals in the markets ought to incorporate our results. People do not take more risk just because they are making decisions for others. Our results suggest that risk-taking has more to do with the misalignment of monetary incentives in agency situations. Our findings are significant because of the pervasiveness of

decision making for others in many real-life situations in society, due to the influence professional investment entities exercise in financial markets, and due to the significance of managed investments to the public in the economy.

While we study an important issue (delegated responsibility) in the context of asset market price stability, we do not touch upon all the possible brunches in this research area. Future studies can vary the number of passive members in a group and check whether that changes the trading pattern of the trader making decisions on behalf of others. Studies can also check whether increasing the number of periods from 15 allows the bubbles to disappear in a fixed salary with responsibility market completely. We observed that prices peak only in the middle of the second half in this environment and it is possible that with more time (i.e. greater number of periods) bubble would be smaller. We find under-pricing in the responsibility markets with a flat payoff. In our setting traders could not be held accountable for their actions. Future studies can zero in on the issue of non-trader members' role in the group. For example, studies can check whether giving the non-trader member the role of a monitor or having multiple non-trader members with different roles can reduce mispricing. Future studies can also focus on mixed-responsibility experimental markets. A number of extensions are possible. For instance, in a mixed responsibility set-up, some traders would be responsible for someone else's economic wellbeing, and some traders would trade for themselves. Studies can vary the proportion of responsible traders in the markets as well. Moreover, researchers can investigate whether hiding the responsibility composition effects the price level in the market. Additional treatments can be run with or without the common knowledge of responsibility. We predict that beliefs regarding how other traders are behaving in the market would influence subjects' trading pattern. In *variable* and *fixed* markets, expectations regarding market price might be higher if subjects do not know that other traders are also making decisions on behalf of a passive member.

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8. Appendix III

Table 1: The table presents standard errors associated with the trading prices. Table reports the standard error for each market in three treatments. P-values are associated with the treatment distribution differences (two-sided MWU test) and treatment mean differences (two-sided t-test). The comparisons are based on market level values.

Markets		Markets		Markets	
variable 1	8.401	baseline 1	21.690	baseline 1	21.690
variable 2	3.692	baseline 2	9.898	baseline 2	9.898
variable 3	5.724	baseline 3	13.662	baseline 3	13.662
variable 4	7.134	baseline 4	9.608	baseline 4	9.608
variable 5	7.985	baseline 5	13.766	baseline 5	13.766
variable 6	9.190	baseline 6	9.986	baseline 6	9.986
Mean	**7.021**		**13.101**		**13.101**
fixed 1	1.936	variable 1	8.401	fixed 1	1.936
fixed 2	7.412	variable 2	3.692	fixed 2	7.412
fixed 3	7.684	variable 3	5.724	fixed 3	7.684
fixed 4	3.504	variable 4	7.134	fixed 4	3.504
fixed 5	5.558	variable 5	7.985	fixed 5	5.558
fixed 6	2.347	variable 6	9.190	fixed 6	2.347
Mean	**4.740**		**7.021**		**4.740**
P-value MWU test	0.078		0.004		0.004
P- value t-test	0.110		0.019		0.004

Table 2: The table presents standard errors associated with the price deviation from the fundamental value. Table reports values for each market associated with the *baseline*, *variable*, and *fixed* treatments. P-values are associated with the treatment distribution differences (two-sided MWU test) and treatment mean differences (two-sided t-test). The comparisons are based on market level values.

Markets		Markets		Markets	
variable 1	10.210	baseline 1	31.559	baseline 1	31.559
variable 2	9.015	baseline 2	12.279	baseline 2	12.279
variable 3	9.815	baseline 3	19.296	baseline 3	19.296
variable 4	11.001	baseline 4	13.246	baseline 4	13.246
variable 5	11.643	baseline 5	16.302	baseline 5	16.302
variable 6	10.936	baseline 6	10.321	baseline 6	10.321
Mean	**10.437**		**17.167**		**17.167**
fixed 1	5.584	variable 1	10.210	fixed 1	5.584
fixed 2	14.364	variable 2	9.015	fixed 2	14.364
fixed 3	13.953	variable 3	9.815	fixed 3	13.953
fixed 4	6.656	variable 4	11.001	fixed 4	6.656
fixed 5	10.008	variable 5	11.643	fixed 5	10.008
fixed 6	7.358	variable 6	10.936	fixed 6	7.358
Mean	**9.654**		**10.437**		**9.654**
P-value MWU test	0.522		0.016		0.055
P- value t-test	0.641		0.086		0.064

Table 3: Peak price associated with individual markets in each treatment. Table reports values for each market associated with the *baseline*, *variable*, and *fixed* treatments. P-values are associated with the treatment distribution differences (two-sided MWU test) and treatment mean differences (two-sided t-test). The comparisons are based on market level values.

Markets		Markets		Markets	
variable 1	300	baseline 1	600	baseline 1	600
variable 2	322	baseline 2	350	baseline 2	350
variable 3	200	baseline 3	450	baseline 3	450
variable 4	300	baseline 4	300	baseline 4	300
variable 5	330	baseline 5	450	baseline 5	450
variable 6	500	baseline 6	500	baseline 6	500
Mean	**325.33**		**441.67**		**441.67**
fixed 1	150	variable 1	300	fixed 1	150
fixed 2	350	variable 2	322	fixed 2	350
fixed 3	400	variable 3	200	fixed 3	400
fixed 4	200	variable 4	300	fixed 4	200
fixed 5	250	variable 5	330	fixed 5	250
fixed 6	95	variable 6	500	fixed 6	95
Mean	**240.83**		**325.33**		**240.83**
P-value MWU test	0.296		0.089		0.020
P- value t-test	0.200		0.073		0.009

Table 4: Amplitude: Measures the trough-to-peak change in asset value relative to its fundamental value. Amplitude measures the degree to which the value of the share or asset changes relative to the fundamental value. We follow the Porter and Smith (1995) definition of Amplitude, where the maximum and minimum price deviations are assessed in relation to the initial fundamental worth of the share. P-values are associated with the treatment distribution differences (two-sided MWU test) and treatment mean differences (two-sided t-test). The comparisons are based on market level values.

	Amplitude based on starting FV		**Amplitude** based on starting FV
Markets		Markets	
baseline 1	2.840	baseline 1	2.840
baseline 2	1.738	baseline 2	1.738
baseline 3	1.974	baseline 3	1.974
baseline 4	1.428	baseline 4	1.428
baseline 5	2.001	baseline 5	2.001
baseline 6	2.114	baseline 6	2.114
Mean	**2.016**	**Mean**	**2.016**
fixed 1	1.195	variable 1	0.968
fixed 2	1.477	variable 2	1.220
fixed 3	1.414	variable 3	1.185
fixed 4	1.008	variable 4	1.518
fixed 5	1.374	variable 5	1.436
fixed 6	0.956	variable 6	1.532
Mean	**1.237**	**Mean**	**1.310**
P-value MWU test	0.007	P-value MWU test	0.016
P- value t-test	0.007	P- value t-test	0.011

Table 5: This table reports the observed values of various measures of the magnitude of bubbles for each session. *Average Bias* is defined following Haruvy and Noussair (2006). The measure calculates the mean of the deviation from the intrinsic value (based on the median price of the share each period). Total Dispersion (TD) measures the absolute deviation (periodic) of prices from the fundamental value. Following Haruvy and Noussair (2006), we use the median prices for a given period. We calculate the Total Dispersion (TD) by taking the 15-period sum of the absolute deviation. RAD and RD are defined following Stöckl, Huber, and Kirchler (2010). The study calculates RAD (Relative Absolute Deviation) by taking the mean absolute differences between the volume-weighted average price and the intrinsic value for all 15 periods and normalising it with the absolute value of the mean fundamental value of the market. We then calculate RD (Relative Deviation) to measure the extent of overvaluation or undervaluation. We take the raw difference between (volume-weighted) average prices and fundamental values in order to calculate the RD. NAV calculates the absolute deviation between the mean of observed share value and the intrinsic share price for each of the fifteen periods. The unit of observation associated with the Mann-Whitney U (two-sided MWU) test and two-sided t-test is every single market's measure of bubble (N =6 for each treatment).

Markets	TD	Average Bias	RAD	RD	NAV	Markets	TD	Average Bias	RAD	RD	NAV
baseline 1	2728.00	97.50	1.44	0.76	184.51	baseline 1	2728.00	97.50	1.44	0.76	184.51
baseline 2	1479.50	12.77	0.73	0.09	93.68	baseline 2	1479.50	12.77	0.73	0.09	93.68
baseline 3	2021.00	43.10	1.01	0.33	129.71	baseline 3	2021.00	43.10	1.01	0.33	129.71
baseline 4	1112.00	-4.67	0.59	-0.02	74.92	baseline 4	1112.00	-4.67	0.59	-0.02	74.92
baseline 5	1660.50	50.20	0.88	0.41	112.50	baseline 5	1660.50	50.20	0.88	0.41	112.50
baseline 6	1723.00	-2.33	0.89	-0.01	114.05	baseline 6	1723.00	-2.33	0.89	-0.01	114.05
Mean	**1787.33**	**32.76**	**0.92**	**0.26**	**118.23**	**Mean**	**1787.33**	**32.76**	**0.92**	**0.26**	**118.23**
variable 1	642.00	-11.33	0.38	-0.06	48.67	fixed 1	1324.50	-37.03	0.70	-0.30	89.69
variable 2	1205.50	-46.43	0.64	-0.35	82.07	fixed 2	1634.50	-9.23	0.85	-0.05	108.42
variable 3	1048.50	-7.23	0.54	-0.05	68.56	fixed 3	1714.00	-9.47	0.90	-0.04	115.80
variable 4	1618.00	21.53	0.91	0.46	116.74	fixed 4	918.50	13.23	0.49	-0.09	62.60
variable 5	1755.50	62.50	0.63	0.02	81.02	fixed 5	1392.50	7.17	0.74	0.04	94.65
variable 6	1212.00	1.33	0.82	0.13	105.15	fixed 6	1255.00	-80.07	0.66	-0.63	84.05
Mean	**1246.92**	**3.39**	**0.65**	**0.03**	**83.70**	**Mean**	**1373.17**	**-19.23**	**0.72**	**-0.18**	**92.54**
P-value MWU test	0.109	0.200	0.109	0.200	0.109		0.109	0.037	0.200	0.010	0.200
P- value t-test	0.078	0.203	0.089	0.179	0.089		0.138	0.031	0.171	0.018	0.171

Table 6: 10-period average values. This table reports the values associated with period 1 to period 10 of various bubble measures for each session. *Average Bias* is defined following Haruvy and Noussair (2006). The measure calculates the mean of the deviation from the intrinsic value (based on the median price of the share each period). Total Dispersion (TD) measures the absolute deviation (periodic) of prices from the fundamental value. Following Haruvy and Noussair (2006), we use the median prices for a given period. We calculate the Total Dispersion by taking the 10-period sum of the absolute deviation. RAD and RD are defined following Stöckl, Huber, and Kirchler (2010). The study calculates RAD (Relative Absolute Deviation) by taking the mean absolute differences between the volume-weighted average price and the intrinsic value for the first 10 periods and normalising it with the absolute value of the mean fundamental value of the market. We then calculate RD (Relative Deviation) to measure the extent of overvaluation or undervaluation. We take the raw difference between (volume-weighted) average prices and fundamental values in order to calculate the RD. NAV calculates the absolute deviation between the mean of observed share value and the intrinsic share price for each of the first ten periods. The unit of observation associated with the Mann-Whitney U (two-sided MWU) test and two-sided t-test is every single market's measure of bubble (N =6 for each treatment).

Markets	TD	Average Bias	RAD	RD	NAV	Markets	TD	Average Bias	RAD	RD	NAV
baseline 1	1423.00	10.47	1.15	0.24	98.19	baseline 1	1423.00	10.47	1.15	0.24	98.19
baseline 2	1389.00	6.73	1.03	0.08	88.30	baseline 2	1389.00	6.73	1.03	0.08	88.30
baseline 3	1531.00	10.47	1.13	0.12	96.33	baseline 3	1531.00	10.47	1.13	0.12	96.33
baseline 4	1094.00	-5.60	0.86	-0.05	73.06	baseline 4	1094.00	-5.60	0.86	-0.05	73.06
baseline 5	1591.50	51.57	1.26	0.13	107.44	baseline 5	1591.50	51.57	1.26	0.13	107.44
baseline 6	1468.00	-18.67	1.14	-0.21	96.99	baseline 6	1468.00	-18.67	1.14	-0.21	96.99
Mean	**1416.08**	**9.16**	**1.09**	**0.05**	**93.38**	**Mean**	**1416.08**	**9.16**	**1.09**	**0.05**	**93.38**
variable 1	575.00	-13.67	0.47	-0.18	39.87	fixed 1	959.00	-61.40	0.76	-0.74	65.22
variable 2	970.50	-62.10	0.76	-0.73	64.67	fixed 2	1058.00	-47.67	0.83	-0.52	70.48
variable 3	738.50	-27.90	0.56	-0.32	47.44	fixed 3	1126.50	-48.63	0.89	-0.53	76.29
variable 4	1044.00	-14.93	0.94	0.27	80.10	fixed 4	622.50	-6.50	0.50	-0.10	42.70
variable 5	1220.50	26.83	0.90	-0.02	77.08	fixed 5	877.50	-27.17	0.71	-0.34	60.86
variable 6	1148.00	-1.33	0.80	-0.22	68.30	fixed 6	1215.00	-81.00	0.96	-0.96	81.50
Mean	**949.42**	**-15.52**	**0.74**	**-0.20**	**62.91**	**Mean**	**976.42**	**-45.39**	**0.78**	**-0.53**	**66.17**
P-value MWU test	0.010	0.149	0.010	0.150	0.010		0.010	0.006	0.010	0.007	0.010
P- value t-test	0.003	0.137	0.003	0.130	0.003		0.002	0.003	0.003	0.003	0.003

Table 7: The table reports the traders' level of wealth in the account and trading frequencies in relations to their CRT scores. The CRT is a measure of subjects' cognitive ability consisting of three questions; the score relates to the total number of accurate responses and ranges from 0 to 3. Panel A and Panel B report the earning levels for different CRT groups and the corresponding statistical significance based on a two-sided MWU test. Table also reports the number observations (i.e. number of traders) for each CRT group. Panel C reports the Correlation coefficient between traders' CRT scores and end wealth along with the p-values. Panel D and Panel E reports the trading frequencies for different CRT groups and the corresponding statistical significance based on an MWU test (unit of observation is subjects' wealth level at the end of the experiment). ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively, suggesting whether the wealth level distribution or trading frequency distribution of the CRT score group is significantly different from the counterpart CRT score group based on a two-sided MWU test (unit of observation is subjects' level of trading).

Panel A

CRT Score/Wealth	*CRT Score <= 1*	*CRT Score > 1*	P-value (MWU test)
Variable salary wealth	850.25	1767.64***	**0.006**
n	[32]	[22]	
Fixed salary wealth	833.52	1236.91*	**0.059**
n	[31]	[23]	
Baseline wealth	841.94	2395*	**0.093**
n	[11]	[16]	

Panel B

CRT Score/Wealth	*CRT Score = 0*	*CRT Score > 0*	P-value (MWU test)
Variable salary wealth	763.24	1435.70**	**0.040**
n	[17]	[37]	
Fixed salary wealth	944.75	1022.64	0.448
n	[12]	[42]	
Baseline wealth	659.56	1882.22*	**0.080**
n	[9]	[18]	

Panel C

Treatment	**Pearson Correlation coefficient** (CRT score and wealth)	P-value	*n*
baseline	0.5157	**0.006**	27
variable	0.3736	**0.005**	54
fixed	0.1341	0.334	54

Panel D

CRT Score/Trading	*CRT Score <= 1*	*CRT Score > 1*	P-value (MWU test)
Variable salary wealth	24.94	21.45	0.316
n	[32]	[22]	
Fixed salary wealth	33.48	32.09	0.903
n	[31]	[23]	
Baseline wealth	22.18	21.00	0.952
n	[11]	[16]	

Panel E

CRT Score/Wealth	*CRT Score = 0*	*CRT Score > 0*	P-value (MWU test)
Variable salary wealth	26.29	22.24	0.162
n	[17]	[37]	
Fixed salary wealth	35.67	32.10	0.443
n	[12]	[42]	
Baseline wealth	21.78	21.33	0.950
n	[9]	[18]	

Table 8: The table presents the OLS regression results associated with the link between subjects' CRT score and the level of profit generated by them at the end of the experiment. In order to calculate net profit associated with a trader, the initial endowment (ECU 280) and the fair value of the shares initially received by the trader (ECU 240*4 = ECU 960) are deducted from the level of wealth in trader's account at the end of period 15 (i.e. the final period in the experiment). The variable *Risk aversion* takes the value between 0 to 10 (where 0 means not willing to take risk at all). The variable CRT is the CRT score (possible scores are: 0,1,2, and 3). The CRT is a measure of subjects' cognitive ability consisting of three questions; the score relates to the total number of accurate responses. The variable *Female* takes the value 1 if the subject is a female and 0 otherwise. Results are presented separately for each of the treatments: *baseline, variable, and fixed*. P-values are reported in the parenthesis and are based on robust standard error. ***, **, and * represent the level of significance at the 1%, 5%, and 10% level, respectively. Since we do not have data on CRT scores and other subject-specific controlling variables for three sessions (27 subjects) in the *baseline* treatment, the number of observations is lower in this treatment.

Dependent variable: Net profit	(1) All	(2) *baseline*	(3) *variable*	(4) *fixed*
variable	-327.6			
	(0.296)			
fixed	-502			
	(0.101)			
CRT	354.3***	698.2**	300.0**	224.1
	(0.002)	(0.037)	(0.024)	(0.218)
Female	-502.8***	-477.1	-644.9**	-432.9*
	(0.008)	(0.440)	(0.023)	(0.091)
Risk aversion	91.58	221.1	-59.9	149.5
	(0.115)	(0.220)	(0.469)	(0.156)
CONST	173.6	-1093.8	1034.9	-586.3
	(0.683)	(0.446)	(0.114)	(0.453)
N	135	27	54	54
Adjusted R^2	0.167	0.277	0.175	0.068

A3.1: Instructions for the fixed treatment markets:

Thank you for agreeing to participate in today's experiment. You are about to participate in some decision-making experiments and at the end of the session you will be paid in cash for your participation. Different participants may earn different amounts, depending upon their decisions and/or the decisions of other participants. The instructions which we have distributed to you, are solely for your private information. Please do not communicate with the other participants during the experiment. Please raise your hand if you have questions; an experimenter will help you.

This is an experiment in the economics of market decision making. In this experiment, you will be a part of a **two-person group**. At no point during or after the experiment other participants in the experiment learn your identity. In the same manner, you do not find out the identity of other participants at any point during or after the experiment.

- **Money in this experiment is expressed in ECU (**ECU 125 = GBP 1**).**
- **The assets or goods that can be bought and sold in the market are called Shares.**

The experiment lasts for **15 periods**. Each group has

- **A trader**
- **A passive member**

Each one of you will be given one of the two roles. Your role is randomly determined.

The experiment consists of a **sequence of 15 trading periods** in which **traders** will have the opportunity to **buy and sell shares**. Each period will last for **120 seconds**.

Half of the subjects (9 out of 18 subjects) in this experiment will **participate in buying and selling of shares** and the rest of the subjects will play a **passive role**. Subjects who are going to participate in buying and selling of shares are the **traders**. Each **passive** participant is paired with a **trader** in this room. Which means that **a trader will be trading on behalf of the passive participant he is paired with**, i.e. the **trader makes decisions on behalf of the passive member in his/her group.**

- **A trader will receive a <u>fixed salary</u> for performing the tasks (i.e. trading activities) on behalf of the passive member.**
- **Each of the 9 traders will receive a fixed salary of <u>ECU 1100</u> for this experiment.**
- **A Passive participant's payoff from this experiment depends on the trader's performance in his/her group**.

A Trader might earn a considerable amount of money for the individual he is representing if he follows the instructions and makes good decisions.

Trading

At the beginning of period 1, a trader will be assigned to a group of 9 traders. Each one of the traders will receive equal amount of money or **cash** (**ECU 280)** and equal number of **shares** (**4 shares)** at the **beginning** of the experiment. This money can be used to purchase shares.

At the end of every period, traders will receive a dividend against each share they are holding. The dividend payments are randomly determined. The chance of receiving a specific amount as dividend is described below:

- **25% chance of a dividend ECU 40**
- **25% chance of a dividend ECU 16**
- **25% chance of a dividend ECU 8**
- **25% chance of a dividend ECU 0**

The expected dividend in each period is ECU 16. Dividends are revealed at the end of every period. So, you will receive dividends 15 times if you hold a share for 15 periods. The value of the asset (share) will be 0 after receiving the final dividend payment, i.e. dividend at the end of period 15.

- When a share is bought, ***Cash*** balance decreases by the price of the purchase, but ***Shares*** increases by one.
- Similarly, when a share is sold, ***Cash*** balance increases by the price of the sale and ***Shares*** decreases by one.

The number of shares at the end of a trading period is the number of shares at the beginning of the next period and is calculated as follows:

Shares = Number of Shares at the beginning of the period + Number of Shares Bought –Number of shares sold.

The cash balance at the end of a trading period is the cash balance at the beginning of the next period, which is calculated as follows:

Cash = Starting Cash Balance + Selling price*Number of shares sold - Buying price*Number shares bought + Dividend payment*Number of shares remaining.

The wealth level for a given period is calculated as follows:

Wealth = Cash + Number of shares*16*(15 - Period), where *Period* is an integer ranging from 1 to 15.

The amount of profit for a given period is calculated as follows:

Profit = Wealth at the end of current period - Wealth at the end of previous period.

Two examples are discussed:

Example 1: Suppose you have 4 shares and ECU 280 in Cash at the start of a period, and you make one transaction during the period; let's say you **purchased one share** for ECU 60 and the **dividend** for the period is ECU 8.

The share holdings at the end of that period will be 5 units.

The new cash holding will be:

280 - 60 (from purchasing 1 share) + **5*8**(each of the 5 shares received a dividend equal to 8) = ECU **260**

Example 2: Suppose you have 4 shares and ECU 300 in Cash at the start of a period and you made two transactions during the period: **purchased a share** for ECU 50 and **sold a share** for ECU 70. Suppose the **dividend** for the period is ECU 40.

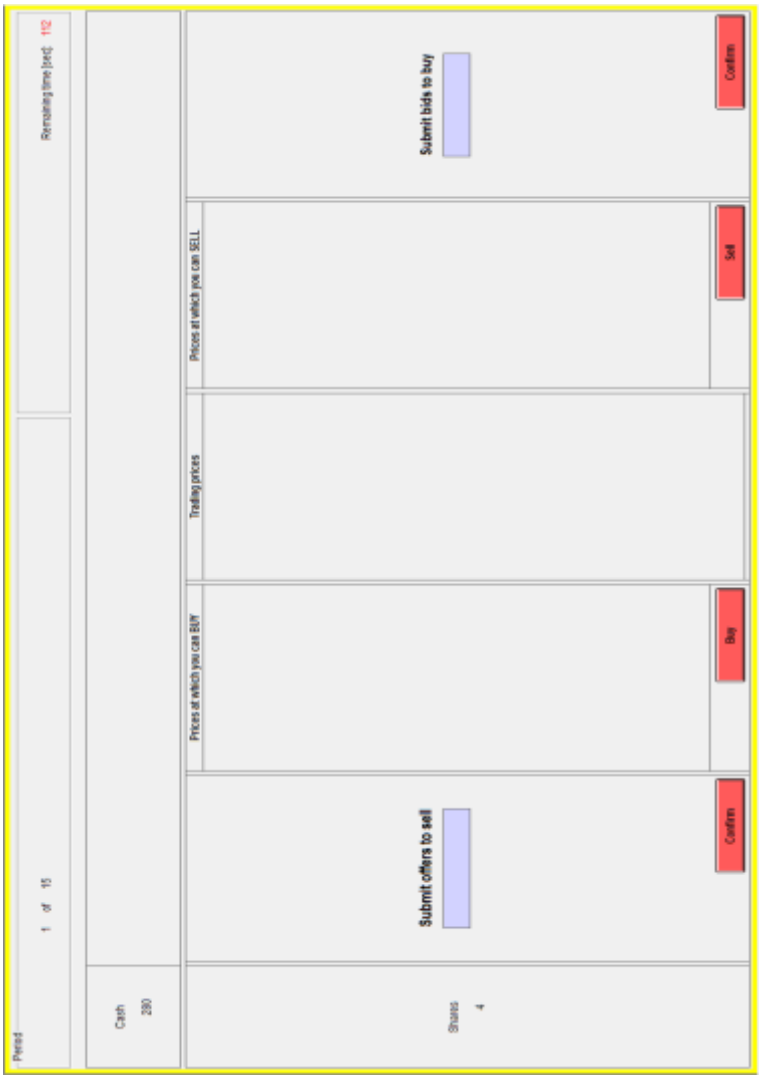
The share holdings at the end of that period will still be 4 units. The new cash holding will be:

300 + 70 (from selling 1 share) **- 50** (from buying 1 share) + **4*40** (each of the 5 shares received a dividend equal to 40) = ECU **480**

Traders' tasks:

The trading screen would look like Figure F1

Figure F1



On the left panel of the **Trading Screen** you will see the amount of **Cash** available to buy shares and the current balance of **shares**. During every period, traders can buy or sell shares from one another by making offers to buy or to sell.

If you would like to offer to **sell a share**, use the text area labelled "**Submit offers to sell**". In that text area you can enter the price at which you are offering to sell a share and then select "confirm". Other traders can buy a share from you at this price.

All prices offered will appear in the column with the heading "**Prices at which you can BUY".** The highest ask price will always be on the top of that list and your offer will be highlighted in blue. If you want to purchase a share, you can select the price you are prepared to pay for a share and then press **"Buy".** Remember, you cannot select your own offer.

In the same way, if you would like to offer to **buy a share**, use the text area entitled "**Submit bids to buy**". In that text area you can enter the price at which you are offering/willing to buy a share. You need to select "Confirm" in order to place your offer. Other traders can sell a share to you at this price.

All bid prices will appear in the column with heading "**Prices at which you can SELL".** The lowest offered price will always be on the top of that list and your offer will be highlighted in blue. If you want to sell a share, please select the price of your choice and then press "**Sell".** Remember, you cannot select your own offer. The column with the name "**Trading prices**" lists prices associated with all the successful transections.

Traders will be provided with information on their performance after every period. The pieces of information include: cash balance before dividend payment, dividend per share, total income from dividends, current cash balance, and current shares.

Payment

A trader will get a fixed salary of ECU 1100 from this experiment for carrying out the trading activities on behalf of the passive member of his/her group.

The Passive member's payoff depends on how much wealth the trader (in the same two-person group) can create for him/her, thus making passive member's payment entirely dependent on the trader's performance.

Passive players' payment from this experiment is computed based on the **wealth level** of the trading account managed by the trader at the end of Period 15. The exchange rate is:

ECU 125 = GBP 1

A3.2: Instructions for the variable treatment markets:

Thank you for agreeing to participate in today's experiment. You are about to participate in some decision-making experiments and at the end of the session you will be paid in cash for your participation. Different participants may earn different amounts, depending upon their decisions and/or the decisions of other participants. The instructions which we have distributed to you, are solely for your private information. Please do not communicate with the other participants during the experiment. Please raise your hand if you have questions; an experimenter will help you.
This is an experiment in the economics of market decision making. In this experiment, you will be a part of a **two-person group**. At no point during or after the experiment other participants in the experiment learn your identity. In the same manner, you do not find out the identity of other participants at any point during or after the experiment.

- **Money in this experiment is expressed in ECU (**ECU 125 = GBP 1**).**
- **The assets or goods that can be bought and sold in the market are called Shares.**

The experiment lasts for **15 periods**. Each group has

- **A trader**
- **A passive member**

Each one of you will be given one of the two roles. Your role is randomly determined.

The experiment will consist of a **sequence of 15 trading periods** in which **traders** will have the opportunity to **buy and sell shares**. Each period will last for **120 seconds**.

Half of the subjects (9 out of 18 subjects) in this experiment will **participate in buying and selling of shares** and the rest of the subjects will play a **passive role**.

Subjects who are going to participate in buying and selling of shares are the **traders**. Each **passive** participant is paired with a **trader** in this room. **A passive participant will not trade shares, but his payoff from this experiment depends on his trader's performance**. Which means that **a trader will be trading on behalf of the passive participant he is paired with**.

A Trader might earn a considerable amount of money for the individual he is representing if he follows the instructions and makes good decisions. **The payoff for both the trader and the passive member in a pair will be the same.**

The trader will receive a fixed salary for performing the tasks on behalf of the passive member. Which means that traders payoff is not performance dependent.

At the beginning of period 1, a trader will be assigned to a group of 9 traders. Each one of the traders will receive equal amount of money or cash (**ECU 280)** and equal number of shares (**4 shares)** at the **beginning** of the experiment. You (if you are a trader) can use the money to purchase shares.

At the end of every period, traders will receive a dividend against each share they are holding. The dividend payments are randomly determined. The chance of receiving a specific amount as dividend is described below:

- **25% chance of a dividend ECU 40**
- **25% chance of a dividend ECU 16**
- **25% chance of a dividend ECU 8**
- **25% chance of a dividend ECU 0**

The expected dividend in each period is ECU 16. Dividends are revealed at the end of every period. So, you will receive dividends 15 times if you hold a share for 15 periods. The value of the asset (share) will be 0 after receiving the final dividend payment, i.e. dividend at the end of period 15.

- When you buy a share, your ***Cash*** balance decreases by the price of the purchase, but ***Shares*** increases by one.
- Similarly, when you sell a share, your ***Cash*** balance increases by the price of the sale and ***Shares*** decreases by one.

The number of shares at the end of a trading period is the number of shares at the beginning of the next period and is calculated as follows:

Shares = Number of Shares at the beginning of the period + Number of Shares Bought –Number of shares sold.

The cash balance at the end of a trading period is the cash balance at the beginning of the next period, which is calculated as follows:

Cash = Starting Cash Balance + Selling price*Number of shares sold - Buying price*Number shares bought + Dividend payment*Number of shares remaining.

Your wealth for a given period is calculated as follows:
Wealth = Cash + Number of shares*16*(15 - Period), where *Period* is an integer ranging from 1 to 15.

Your profit for a given period is calculated as follows:

Profit = Wealth at the end of current period - Wealth at the end of previous period.

Two examples are discussed:

Example 1: Suppose you have 4 shares and ECU 280 in Cash at the start of a period, and you make one transaction during the period; let's say you **purchased one share** for ECU 60 and the **dividend** for the period is ECU 8.

Your share holdings at the end of that period will be 5 units.

Your new cash holding will be:

280 - 60 (from purchasing 1 share) + **5*8**(each of the 5 shares received a dividend equal to 8) = ECU **260**

Example 2: Suppose you have 4 shares and ECU 300 in Cash at the start of a period. You made two transactions during the period: **purchased a share** for ECU 50 and **sold a share** for ECU 70. Suppose the **dividend** for the period is ECU 40.

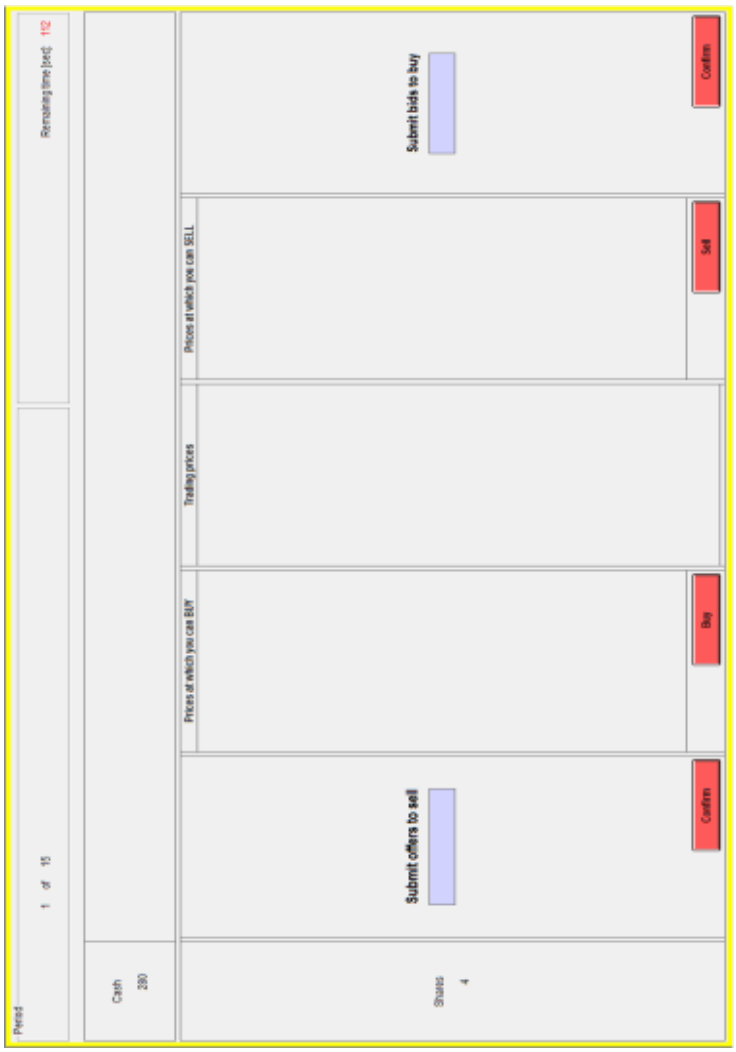
Your share holdings at the end of that period will still be 4 units. Your new cash holding will thus be:

300 + 70 (from selling 1 share) **- 50** (from buying 1 share) + **4*40** (each of the 5 shares received a dividend equal to 40) = ECU **480**

Traders' tasks:

Your trading screen would look like Figure F1

Figure F1



On the left panel of the **Trading Screen** you will see the amount of **Cash** available to buy shares and the current balance of **shares**. During every period, traders can buy or sell shares from one another by making offers to buy or to sell.

If you would like to offer to **sell a share**, use the text area labelled "**Submit offers to sell**". In that text area you can enter the price at which you are offering to sell a share and then select "confirm". Other traders can buy a share from you at this price.

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In the same way, if you would like to offer to **buy a share**, use the text area entitled "**Submit bids to buy**". In that text area you can enter the price at which you are offering/willing to buy a share. You need to select "Confirm" in order to place your offer. Other traders can sell a share to you at this price.

All bid prices will appear in the column with heading "**Prices at which you can SELL".** The lowest offered price will always be on the top of that list and your offer will be highlighted in blue. If you want to sell a share, please select the price of your choice and then press "**Sell".** Remember, you cannot select your own offer. The column with the name "**Trading prices**" lists prices associated with all the successful transections.

You will be provided with information on your performance after every period. The pieces of information include: cash balance before dividend payment, dividend per share, total income from dividends, current cash balance, and current shares.

Payment

Both members (the passive member and the trader) in a group will earn the same amount of money from this experiment. **The payment associated with a group depends entirely on the actions of the trader, thus making passive member's payment completely dependent on trader's performance.**

The payment from this experiment is computed based on the wealth level of the trader at the end of Period 15. The exchange rate is:

ECU 125 = GBP 1

Paper 4

Honesty, Framing, Emotional state, and Responsibility

Abstract

We ran two experiments to investigate the impact of framing, emotional states induced by video clips, and responsibility on honesty and lying behaviour. We use a die-rolling experiment with economic incentives following Fischbacher and Föllmi-Heusi (2013). In our experiments, subjects can get a bigger payoff by misreporting privately observed information. Lying cannot be detected at the individual level. In the first part of the paper, we use different types of intervention to check whether that has any influence on how individuals lie about privately observed information. We find that negative message about dishonest actions and a video clip intended to elicit the emotional state in the form of contentment decrease lying. We find that individuals with higher cognitive ability (i.e., higher CRT scores) are less likely to be influenced by interventions. We also find that narcissists are more likely to lie. In experiment two, we observed that truth-telling was less evident in the presence of added responsibility. Both men and women tell a white lie (i.e. a lie that helps another individual), but men are more likely to tell a white lie in the presence of an economic incentive.

Key words: Lying, Framing, Emotion, CRT, Responsibility, White lie.

1. Introduction

Dishonesty is a persistent phenomenon and can have far-reaching consequences. Economists have long been interested in this topic which allowed our understudying of this issue to be broadened (Becker, 1968). Traditional economic theories predict that individuals will not hesitate to misreport privately owned information if doing so maximises their utility (the Beckerian view). If lying cannot be detected, a person should misreport as long as it maximises his/her economic benefits. As per Batigalli et al. (2013), however, an individual might find lying or

cheating to cause emotional discomfort even if there is no possibility of detection.[40] Several experimental studies in recent years document honest disclosure of private information when lying about it would have maximised the utility (see Jacobsen and Fosgaard, 2018 for a review). According to a growing body of literature, non-economic factors such as social norms, environmental mechanisms, preferences, ethical values are connected to dishonesty (Gneezy, 2005; Erat and Gneezy, 2016; Gächter and Schultz, 2016). As per Jacquemet et al. (2019), certain environmental factors and context can influence dishonesty in human beings.

We investigate the impact of message framing, emotional states induced by video clips, and responsibility on honesty and lying behaviour. Our paper consists of two experiments. We employ Fischbacher and Föllmi-Heusi (2013) die-rolling game in both experiments. In this set-up, subjects self-report the outcome of a die-roll under total anonymity. The anonymity condition allows them to report favourable outcomes to get bigger payoffs. In experiment one, we investigated whether manipulation of simple micro-environment mechanisms can influence dishonest actions. The experiment uses two simple mechanisms - messages and video clips - as interventions to influence the level of honesty in subjects. Subjects report privately observed numbers in two stages. An Intervention is used between reporting stages to examine whether those have any impact on the preference for truth-telling. In two treatments involving a message intervention, subjects are shown a message on the computer screen between the stages. Subjects see a message that supports actions that are considered immoral by many in one of those treatments. The message used in the other treatment, in contrast, does not support immoral actions. We use short video clips in two other treatments. The purpose was to elicit two distinct types of emotions: positive emotion and negative emotion. This allows us to check whether honesty depends on emotional states. The study also focuses on the personality dimension narcissism and cognitive ability in the context of unethical behaviour. The second part of the paper sheds lights on the

[40] The notion that Individuals are often motivated by social goals and not just by self-interest has been gaining popularity in the literature of economics (Fehr and Schmidt, 1999; Andreoni and Miller, 2002).

link between responsibility and lying behaviour. We investigate whether taking responsibility for someone else's wellbeing leads to more (or less) dishonesty. In this experiment, we have an active participant who performs the die-rolling task and then reports the outcome on behalf of another subject. We run two treatments in this study *(i)* the active member receives a fixed payment for performing the die-rolling task on behalf of the passive member *(ii)* the active member receives the same amount as the passive member in the group for performing the die-rolling task on behalf of the passive member.

Popular honesty experiments involve simple non-strategic tasks such as die-rolling or coin toss, where participants self-report privately observed outcomes that are linked to their payoffs. For example, in the die-rolling experiment, subjects can make more money by reporting some specific outcomes under conditions of total privacy. Researchers can draw conclusions on whether or not cheating was prominent by comparing the reported figures with the expected distribution (for instance, 50 per cent heads for a fair coin toss experiment). In such conditions, the researcher is unable to corroborate lying or cheating at an individual level directly, which means that participants can influence their earnings by falsifying the outcomes (Gneezy et al., 2013). Fischbacher and Föllmi-Heusi (2013) use an innovative game involving die-rolling to show that individuals systematically misreport the true value of the outcome from a die-roll for financial gain. The innovative but simple experiment can detect the propensity of lying in a subject pool. In our experiments, lying cannot be detected at the individual level. We conducted sessions at the University of Nottingham. Overall, 318 subjects participated in the first experiment. A total of 56 subjects actively participated in the second experiment. All the sessions for this experiment were run in Nottingham.

The key findings are as follows. In the first experiment, it is found that message framing and emotional states elicited by a video clip can influence subjects' preference for truth-telling. We show that a message containing information about the negative consequences of dishonest actions (i.e. a message that acts as an honesty reminder) and a video clip that is assumed to be inducing positive emotion

in the form of contentment can nudge some subjects toward honesty. We also find that narcissism levels in individuals are positively associated with the propensity to lie. The average payoff reported by individuals with a high level of narcissism is significantly higher than payoff claimed by less narcissistic subjects. In Addition, we find a link between subjects' cognitive ability and the degree to which they are likely to be influenced by the intervention. We measure cognitive ability employing the cognitive reflection test or CRT (Frederick, 2005). Subjects with higher cognitive ability (i.e., higher CRT scores) seemed to be unaffected by intervention and reported a higher amount in the second stage. In experiment two, we observed that truth-telling was less evident in the presence of added responsibility. We find that both men and women tell a white lie (i.e. a lie that helps another individual), but women are less likely to tell a white lie compared to their male counterparts. On the other hand, we observed that male subjects are more likely to tell a Pareto white lie (i.e. a lie that benefits both the liar and another individual). Our study is related to works on lying aversion and white lies (Gneezy, 2005; Mazar et al., 2008; Erat and Gneezy, 2012; Fischbacher and Föllmi-Heusi, 2013). The findings of our study are also related to the literature of narcissism (Paulhus and Williams, 2002; Resick et al., 2009), behavioural biases and cognitive ability (Frederick, 2005; Oechssler et al., 2009), and decision making for others (Bolton and Ockenfels, 2010). Overall, the results of our study question the traditional view on honesty, i.e. a zero lying cost implied by Becker (1968) and indicate that individuals are not always motivated by monetary incentives.

The following section presents the design of the experiments and treatment descriptions. Section 3 presents a review of relevant literatures and discusses the hypotheses. Section 4 presents and discusses the results. Section 5 presents a discussion on generalizability of the findings and Section 6 concludes.

2. Experimental Design and Treatments

2.1 Experiment one

This experiment consists of two identical stages. The task chosen is a simple die-roll, which is a non-strategic individual-level decision. Subjects report the outcome of the die throw and the corresponding payoff in both stages. The die-roll task and payoff calculation are in line with Fischbacher and Föllmi-Heusi (2013). Subjects determine their payoff by rolling a six-sided dice twice in both stages. They are instructed to report the first roll, which decides how much money they make in each stage. The following chart shows the exact payoff.

Chart 1: Payoff rules

Number reported	**1**	**2**	**3**	**4**	**5**	**6**
	ECU	ECU	ECU	ECU	ECU	ECU
Resulting payoff	1.00	2.00	3.00	4.00	5.00	0.00

For example: if the reported number is "3", the payoff will be ECU 3.00 for the corresponding stage. We tell the subjects that the second throw only serves to ensure that the dice is working correctly. So only the first roll counts. The exchange rate for the experiment is ECU 3.00 = GBP 1.00. In both stages, subjects were presented with opportunities to overreport under total anonymity.

It is not possible for the experimenter to detect dishonesty at the individual level under the condition of full anonymity. Nevertheless, aggregate behaviour contains information. In a subject pool where dishonesty is prominent, the average of the reports for this experiment would be significantly higher than 2.5. In the case of 'full dishonesty', the average is exactly equal to 5. The study also incorporates the notion "justified ethicality" to the analysis (following Gächter and Schulz, 2016). The idea here is that individuals have the need for maintaining a clean image of themselves. In this experiment, lying about the outcome of die throw threatens this self-image. Nevertheless, bending the rule may not jeopardize the self-image. For instance, recording the more favourable of the throws involves justified dishonesty

standard. Under justified dishonesty, the mean claim should be 3.473. Chart 2 shows probability distributions of payoffs under full honesty and under justified dishonesty.

Chart 2
Probability distributions of claims with different honesty benchmarks.

	Full Honesty	**Justified Dishonesty**	**Full Dishonesty**
Claims	Probability	Probability	Probability
0	0.167	0.028	0.000
1	0.167	0.083	0.000
2	0.167	0.139	0.000
3	0.167	0.194	0.000
4	0.167	0.250	0.000
5	0.167	0.306	1.000
Sum	1.000	1.000	1.000

2.1.2 Treatments in the first experimental study

The experimental study uses framing to observe whether it has any impact on the aggregate level of intrinsic honesty. Overall, this experiment considers five treatments. In addition to a baseline treatment, there are two treatments involving interventions in the form of a message and two more treatments involving interventions in the form of a video clip. In one treatment, subjects see a message concerning the negative aspects and impacts of dishonesty in between two stages. The other treatment involving message framing shows a message associated with actions that have been perceived by many as immoral but not quite illegal. Subjects see a message that justifies those immoral actions. The fourth treatment contains a video clip showing generosity involving an ambulance; the clip shows how people react in a positive manner to an ambulance siren. The fifth treatment contains a video clip showing insensitive responses to an ambulance siren. Treatments are beefily described in Chart 3.

As discussed before, in both stages, participants determine their payoffs by throwing dice in a cup. In *NegMsg* and *PosMsg*, participants are shown a message on the computer screen between two stages. The following message was used in *NegMsg*:

"A staggering £16 billion is still lost to tax fraud every year." Meg Hillier, a British politician, on tax fraud in the United Kingdom.

On the other hand, the following message was shown in the *Positive Message* treatment:

A famous businessperson was criticised after it was revealed that he used loopholes in tax laws to legally avoid paying huge amount of taxes for years. And this was the reaction from a close friend: "There aren't very many smart businessmen that don't take advantage of the legal tax laws that are there, and if they are, then they're not very good businessmen."

Chart 3
Treatment descriptions: Experiment 1

Treatment name	Description
Plain	No message or video clip
NegMsg	Intervention with a negative message about dishonesty
PosMsg	Intervention with a positive message about dishonesty
NegVideo	Intervention with a negative video (negative response to ambulance siren)
PosVideo	Intervention with a positive video (positive response to ambulance siren)

One of the objectives of our study is understanding the link between incidental emotional states and truth-telling. The study induces participants' incidental emotional states by means of video clips, which is a standard procedure (Rottenberg et al., 2007) that is known to influence decisions with economic incentives (Andrade and Ariely, 2009). Andrade et al. (2015) study the influence of heterogeneous emotional states on mispricing by inducing different types of emotions (e.g. excitement, sadness, fear) through the use of video clips.[41] A number of studies examined the elicitation of emotional states employing video clips in individuals (see Hewig et al., 2005).

We use short video clips as interventions in two other treatments. The intention was to elicit different types of emotions in subjects by showing them a generous rection and an unsympathetic reaction to ambulance sirens in different countries.

[41] It was noticed that bubbles are more pronounced when traders are shown exciting video clips. The magnitude of the bubble is larger compared to situations where traders are shown sad, neutral, and fearful videos.

In each treatment, the video clip was shown between the reporting stages. Subjects were requested to watch the video clip lasting for approximately 100 seconds. The video clip associated with treatment *PosVideo* is intended to elicit a positive emotion in the form of contentment or happiness. The video showed how German people generously reacted to ambulance sirens. In this brief video, people in vehicles make the way free so that the ambulance could move swiftly. The other video clip, the one used in treatment *NegVideo,* also involves ambulances, but the scenario was different. Participants watched how people (in India) responded unenthusiastically to the siren of an ambulance despite the urgency. People were not very helpful to accommodate the ambulance so that it could move freely. Drivers of the vehicles in front and on both sides of the ambulance did not react in a positive manner to the sound of the ambulance siren. Some of them were selfishly blocking the path and even trying to overtake the ambulance. The ambulance was struggling to navigate its way in the traffic. We assume that observing this kind of behaviour should elicit negative emotion in the form of anger in viewers.

Finally, no such message or video clip was shown in the Plain treatment.

2.2 Experiment two

In this study, one group of subjects actively participated in the experiment, and another group of subjects had a very passive role. The active subject performs the die-rolling tasks on behalf of another subject (passive) present in the laboratory. Subjects were randomly assigned the roles. At the beginning of the experiment (when they showed up at the laboratory door), we ask each subject to pick a number (ID for the subject in the session) from a box. By picking a random number, they determine their role in the experiment. We make sure that subjects do not see the number before they pick one. If a subject picks a number between 1 and 14, he or she is given the role of the group leader. The group leader would perform the die-rolling task on behalf of the group. If the number is greater than 14, subjects assume the role of a passive member. Payoffs were denoted in experimental currency units (ECU), where ECU 1 equals 1.25 British pound.

2.2.1 Treatments in the second experimental study

In the second study of this paper, we explore the impact of responsibility on participants' tendency to lie about privately observed information. In order to test the hypotheses (discussed in Section 3), we used a variation of Fischbacher and Föllmi-Heusi (2013) die-rolling experiment. Unlike the previous treatments where subjects participated in the experiments as individuals, the decision-maker decides on behalf of another individual, and for himself/herself, i.e. the decision-maker was responsible for some else's pay off. We collect data for two treatments and at the end of experimental session, participants filled in a questionnaire containing questions regarding their demographics, study area, personality, psychology and more.

Chart 4
Treatment descriptions: Experiment 2

Treatment name	Description
TRV	Responsibility with variable payment: The leader of the two-person group throw the dice on behalf of the group. The payment is the same amount for both the leader and an anonymous passive member. The payment is equivalent to the amount reported by the leader.
TRF	Responsibility with fixed payment: The leader of the two-person group throw the dice on behalf of the passive member. The payment is for the leader is Fixed (ECU 4.00) the anonymous passive member receives the amount reported by the leader.

We have two treatments in this experiment *(i)* TRV *(ii)* TRF. See Chart 4 for treatment descriptions. In treatment TRV, the responsible member of the group (the leader) makes a decision, i.e. rolling the die and reporting the number, on behalf of the group and receives a variable payment. The payment for both leader and the passive subject is identical and depends entirely on how much the leader reports (as shown in Chart 1). In treatment TRF, the group leader receives a fixed payment of ECU 4.00 for deciding on behalf of the passive participant, but the recipient of the group receives a variable amount. The variable amount directly depends on how much the leader reports (as per rules in Chart 1). We wanted to choose a flat payment for the leader that would have been the payoff reported by the participants in a baseline environment had they been reporting for themselves. Although the fixed payoff is an arbitrary amount, it is not statistically significantly different from

the average payoff (ECU 3.75) claimed on behalf of the passive members in treatment TRF. The flat payoff is also equal to the median first stage payoff claimed by subjects in the first experiment. We chose ECU 4.00 in order to maintain comparability to the other treatments. The full-honesty benchmark payoff, i.e. ECU 2.50, was another candidate, but we did not choose that amount due to the potential influence of inequality aversion on the reporting behaviour of the participants. As per Bolton and Ockenfels (2010), social comparison can affect decision making under responsibility. A payoff ECU 2.50 might come across as too low to active subjects in treatment TRF. For the same reason, we refrain from choosing ECU 5.00 (i.e. the maximum payoff) as the flat payment; the maximum payoff might be seen as too generous. The former might induce a more conservative reporting behaviour and the later a more generous reporting behaviour.

2.3 Instruction

In experiment one, as well as in experiment two, instructions were read aloud by the investigator before the experiment took place. The experiments were conducted in such a set-up that the experimenter could not see what the subjects were doing. So, there was absolutely no chance that subjects could be caught misreporting. We made sure that the subjects know that their identity would never be revealed during or after the experiment. Still, the presence of the experimenter in the same room where the experiment was taking place could influence the reporting behaviour of the subjects.[42] Therefore, the experimenter, after giving the instruction, left the lab and went to a separate room (the server room) to make sure that subjects feel comfortable about their anonymity. The subjects, however, were not explicitly told that instructors would leave the room during the experiment as this might create a non-neutral environment inside the laboratory.

[42] If someone wanted to cheat, he or she might still hesitate in the presence of the experimenter.

2.4 Data

Data for the first experiment: 318 subjects were recruited at the CRIBS laboratory of the University of Nottingham, using ORSEE (Greiner, 2015). Overall, 318 subjects participated in the first experiment. The experiment lasted approximately 8 minutes (including instructions). Participants earned £2.49 on average. The experiment was conducted on computers using zTree (Fischbacher, 2007). The fraction of female subjects in the experiment was 52.83 per cent, and the mean age of the sample was 22.73 years (standard deviation = 3.11). In the second experiment, we recruit 56 subjects at the CRIBS laboratory of the University of Nottingham, using ORSEE (Greiner, 2015) who played an active role in the experiment. We also recruit subjects who played a passive role in the experiment. Subjects' role was determined randomly. This experiment was conducted on computers using zTree (Fischbacher, 2007). The experiment lasted for approximately 4 minutes (including instructions). The mean age of the subjects is 21.17, and 55.43 per cent of them are women. Subjects who actively participated in the experiment have a mean age of 21.18 and 57.14 per cent of them are women. Participants in the second experiment earned £5.42 on average. Participants who actively participated earned almost an identical amount (£5.40) from the experiment.

3. Literature Review and Hypothesis Development

Fischbacher and Föllmi-Heusi (2013) conducted a six-sided die roll experiment where the participants were asked to report private information. The piece of information was the die-roll result which is linked to subjects' payoff. The experiment is set-up such a way that individuals could secure higher pay off by misreporting, i.e. reporting numbers that are higher than the observed numbers. The study found that some subjects reported zero, which is indicative of the fact that they were telling the truth. It was also found that some subject pools were not telling the truth; the portion of participants reporting 4 and 5 were significantly higher than one-sixth. In a coin-tossing experimental study by Houser et al. (2012),

only 74.5 per cent of observations were associated with the profitable outcome, which means a considerable number of subjects were telling the truth (also see Bucciol and Piovesan, 2011).

Our study is in the spirit of those where experimenters used framing and looked at situational subtleties to study self-reporting behaviour. Studies reported a decrease in the overreporting best outcome (i.e. a fall in the level of lying) in situations where subjects were given explicit instructions not to cheat (Bucciol and Piovesan, 2011) and took moral vows (Utikal and Fischbacher, 2013). Other studies showed that the propensity to cheat or lie falls when subjects were not under any time pressure (Shalvi et al., 2012), the difference between the date of reporting and the date of payments was greater (Ruffle and Tobol, 2014). On the other hand, the costs of lying appear to fall (i.e. over-reporting of favourable outcomes increases) when the proceeds are given to charitable entities (Lewis et al., 2012), participants have previously behaved in an environmentally-friendly way (Mazar and Zhong, 2010), subjects played the game in as a team (Conrads et al., 2013).

In the first two treatments in our first experiment, we study the effect of negative and positive messages on subjects' reporting behaviour. Studies used different types of message framing, such as communication in pictures and words to influence the behaviour of subjects (Gifford and Comeau, 2011). Researchers can employ frames for various purposes such as defining problems, suggesting who is responsible, and figuring out solutions to issues (Corbett, 2006; Shanahan and Good, 2000). Theoretical developments in areas such as psychology, sociology and economics indicate that intrinsic honesty at the individual level is influenced by the pervasiveness of rule violation in the social environment (see Hauk and Saez-Marti, 2002). Gächter and Schulz (2016) reported a strong link between pervasiveness of rule violation and individual level intrinsic honesty based on a study that includes subjects from 23 countries. We propose that subjects are less likely to lie after seeing a negative message about honesty. On the other hand, we conjecture that subjects would continue to lie after observing a positive message about ethically incorrect actions, i.e. a statement that justify (to some extent) actions that are perceived to be dishonest by many.

The study also investigates whether there is a link between the subjects' emotional states and the propensity to lie. Lowenstein and Lerner (2003) suggest that different types of emotions can manipulate subjects' behaviour in a direct manner as well as through their influence on judgments of anticipated outcomes. Our study adds to the literature by exploring the association between two types of emotional states, anger and contentment, and the propensity to lie. The study uses short video clips in order to elicit these two types of emotional states. Several studies investigated the elicitation of emotional states employing video clips in subjects in different countries (Gross and Levenson, 1995; Philippot, 1993; Hewig et al., 2005). The study uses two types of video clips: *(i)* showing a positive and generous response to ambulance sirens; *(ii)* showing a negative and ungenerous response to an ambulance siren. We conjecture that the first video would induce contentment, and the second video clip would induce anger among subjects. We hypothesise that the emotional state contentment will direct subjects toward honesty, i.e. they are less likely to lie under this emotional state. Moreover, we predict that subjects will continue to lie when they are angry.

The study examines whether there is a link between the level of narcissism and the propensity to lie. Narcissism has frequently been considered as a dark side of the personality and certainly forms a portion of the dark triad of personality traits since a narcissistic individual is often associated with arrogance and immodesty (Resick et al., 2009; Paulhus and Williams, 2002). Campbell et al. (2004) suggest that narcissism can influence an individual's motivations and decision-making. Highly narcissistic individuals are expected to value themselves highly, and they are expected to be less attentive to the needs of others. Moreover, due to an ever-present concern about themselves, narcissistic individuals often lack moral sensibility (Campbell et al., 2011).

We also delve into the relationship between subjects' cognitive ability and the extent to which they are likely to be influenced by different forms of message framing, i.e. whether interventions make them change their lying patterns. Scholars began to pay attention to the influence of cognitive ability on decision making in recent years. Various studies find that individuals with higher cognitive ability

are different from individuals who score low on this dimension in a number of ways. Typically, they have longer working memories, quick reaction times, and are more prone to visual illusions (Jensen, 1998). We used the Cognitive Reflection Test or CRT (Frederick, 2005) to measure the subjects' level of cognitive ability (Frederick, 2005). The CRT is associated with general measures of intelligence along with essential decision-making aspects in the form of preferences for time and risk (Frederick, 2005; Oechssler et al., 2009) and reasoning levels (Branas-Garza et al., 2012). Individuals with higher cognitive ability are less prone to behavioural biases such as conservatism and conjunction fallacy in revising probabilities. (Oechssler et al., 2009). We suspect that individuals with higher CRT scores are less likely to be swayed away by different types of intervention between two reporting stages, i.e. we expect them to report a higher amount following the intervention.

The first experiment in this paper considers the following hypotheses:

H1: Message framing should have a material impact on reporting behaviour of private information. Exposure to a negative message about dishonest actions decreases the propensity to lie.

H2: Subjects are less likely to lie when they are happier and would continue to lie when they are angry.

H3: Narcissism level in individuals is positively associated with lying behaviour.

H4: Subjects with a high level of cognitive ability (i.e. higher CRT scores) are less likely to be swayed by interventions.

In the second experiment, the focus is on responsibility and its influence on the degree of lying. The key interest in this part of the paper is whether subjects' lying pattern changes when other individuals are also benefiting from lying. Several recent studies shaded lights on the topic "decision making for others". For example, Pahlke et al. (2015) compare decisions under responsibility with individual decisions, i.e. they compare decisions where the decision-maker is the only person to be affected with decisions where others' financial position is also affected.

Reynolds et al. (2007) documented an increase in individuals' risk aversion when they make decisions that affect others. We argue that the sense of being part of a group will lead traders in the two-person group to demonstrate a higher level of risk aversion. Bolton et al. (2015) suggest that individuals are likely to change their behaviour on the conjecture that social responsibility increases the level of risk aversion in decision-makers compared to baseline scenarios (i.e. where decisions are made at an individual level). They observed a greater level of caution in decision making when agents are socially responsible. It is reasonable to suggest that responsibility forces the decision-maker to become more concerned about the wellbeing of the people they are representing. We conjecture that individual responsible for someone else's payoff will over-report (i.e. lie) even if there is no monetary benefit form that. Moreover, we predict that the degree of overreporting is higher when responsibility is combined with monetary gain.

The study also explores the issue of gender differences in lying. We propose to study the role of gender in the context of honesty and responsibility. Prior studies show that women exhibit more honesty in a group setting (Muehlheusser et al., 2015). So, we predict that women are more likely to tell the truth under responsibility compared to their male counterparts. Erat and Gneezy (2012) distinguishes between black lies (i.e. lies that benefit the liar at the expense others), Pareto white lies (i.e. lies that benefits others and the liar), and altruistic white lies (i.e. lies that benefit others at the expense of the liar)

H5a: Subjects who are responsible for others are expected to lie even if there is no economic benefit from doing that.

H5b: More lying is observed when subjects are responsible for others as well as can be economically benefitted from lying.

H5c: Women are less likely to lie under responsibility.

4. Results and Discussions

4.1 Experiment one results

4.1.1 Lying behaviour

The paper has two experimental designs. So, we presented data separately. First, the aggregate result associated with all the treatments in the first experiment is presented in Table 1. A total of 318 subjects took part in five treatments. The summary statistics associated with reported payoffs in both stages are presented in Table 1. In all treatments (five treatments and ten stages), the lying is very prominent (as shown in Figure 1 and Figure 2). The mean of the reported payoffs in every stage is much higher than the full honesty benchmark (i.e. a payoff 2.5) and the portion of participants who reported the maximum payoff (i.e. payoff of 5) is significantly higher than 16.7 per cent for all the treatments (see Figure 1). The average of the two payoffs in every treatment is significantly higher than the full honesty benchmark of 2.5 (solid black line in Figure 2).[43] Nevertheless, some unconditionally honest reporting behaviour is observed in all the treatments, i.e. the fraction of participants who reported 0 is above 0 per cent in all the stages.

Insert Figure 1 & 2 from Appendix IV here

Insert Table 1 from Appendix IV here

In order to check whether there was any treatment effect on lying behaviour, we follow three approaches. First, we look at the statistical significance associated with the difference between *Payoff1* and *Payoff2* in a treatment (see Table 1). We also check whether the proportions of subjects reporting the highest payoff, i.e. ECU 5, are different in different stages (see Table 2). Finally, we estimate the mean per cent of dishonest participants in both stages following Garbarino et al. (2018). Table 3 presents the estimation of the mean per cent of dishonest subjects. In this method, the mean per cent of individuals who lie when reporting the outcome of a random device is conditional on having received a bad outcome. To estimate the

[43] Untabulated results. The two-sided p-value from a binomial test is < 0.01 for every treatment average payoff.

mean lying rate, the following information are used: N = total number of subjects who observe the outcome of a random device; R = number of subjects who report the high payoff outcome ($0 \leq R \leq N$) ; p = probability of receiving the low payoff outcome [0,100]. First, we estimate the mean percentage of dishonest reports based on the assumption that a payoff greater than 3 is a high payoff. We repeat the estimation process with 5 as the higher payoff for robustness purposes. Estimations using both approaches are presented in Table 3.

Insert Table 2 & 3 from Appendix IV here

The mean of the payoffs in the second stage in treatment *plain* is slightly higher than (statistically insignificant though) the payoff in the first stage. The proportions of participants claiming the highest payoff is also the same. In treatment *PosMsg Payoff2* is exactly the same as *Payoff1*, suggesting that subjects maintained the same level of lying after exposure to a positive message about corner-cutting behaviour. As expected, a message that somewhat justifies actions perceived by many as dishonest does not encourage subjects to tell the truth. Following the approach proposed by Garbarino et al. (2018), we find that the mean per cent of dishonest reports in the second stage is either marginally higher or the same in treatment *plain* and the same in treatment *PosMsg*. Treatment *NegMsg* produces different results. Although the mean payoff in the second stage (ECU 3.51) is marginally lower (at 10% level only) from the mean payoff in the first stage (ECU 3.79), the proportion of the subjects claiming the highest payoff is significantly lower in the second stage of reporting. Similarly, the percentage of dishonest reports in the second stage is significantly lower compared to the first-stage percentage. This is indicative of the fact that a negative message about unethical behaviour (in our context statistics associated with the amount of loss due to corruption) influenced some subjects and nudged them toward the truth. A one-sided binomial test reveals that the proportion of subjects in the second stage reporting the highest payoff, i.e. Pr_2^5= 0.306, is significantly lower than the proportion in the first stage, i.e. Pr_1^5= 0.458, with a p-value of < 0.01. The mean per cent of dishonest subjects in the second stage calculated following Garbarino et al. (2018) is also significantly different (p-value< 0.001) in *NegMsg* compared

to the mean in the first stage.[44]This is quite a contrast to what we observed in treatment *Plain* and *PosMsg.*

In the rest of the treatments, we induce subjects' incidental emotional states by showing them video clips. The video clips are shown in between the reporting stages and subjects roll the die in the second stage after the emotion induction. The lowest mean payoff in video clip treatments is associated with the second stage of the positive reaction to video message *(PosVideo*) treatment. The reported mean payoff for that stage is 3.57. The difference between *Payoff1* and *Payoff2* is statistically significant (one-tailed p-value < 0.05) for this treatment. The percentage of estimated dishonest subjects is also significantly lower in the second stage (p-value < 0.05; based on a one-sample test of proportion). We do not observe any reduction in dishonest reporting in the second stage of *NegVideo* treatment. This is suggestive of the fact that subjects maintained the same level of lying after watching a video clip supposed to make them angry. It appears that some subjects choose not to lie or at least did not lie maximally in the second stage of the treatment (*PosVideo*). We are inclined to suggest that this is due to the fact that watching the video clip made them happier. On the other hand, subjects continued to lie in treatment *NegVideo* due to anger.

4.1.2 Narcissism and Cognitive ability

Narcissism and lying: The study also investigates whether there is a link between narcissism and dishonest behavior. The level of narcissism in a subject is measured by using the forty-item Narcissistic Personality Inventory (NPI-40) following Raskin and Terry (1988). This is done after the experiment (students answer several questions at the end of the experiment). Narcissism has frequently been considered as a dark side of the personality and certainly forms a portion of the dark triad of personality traits since a narcissistic individual is often associated with arrogance and immodesty (Paulhus and Williams, 2002; Resick et al., 2009). Campbell et al. (2004) suggest that narcissism can influence an individual's motivations and decision-making. Highly narcissistic individuals are expected to

[44] The p-value is based on a one-sample test of proportion.

value themselves highly, they are expected to be less attentive to the needs of others, and for that reason are expected to be associated with dishonest actions.

Subjects answer each of the forty questions in the NPI-40 by choosing one of the two statements (narcissistic vs non-narcissistic). Studying the correlation coefficients (reported in Panel A of Table 4) reveals a strong and positive association between average payoff claimed by the subjects and their level of narcissism (Pearson correlation coefficient p-value < 0.01). The regression results reported in Table 5 confirms that narcissistic individuals are more likely to cheat to gain financially. Specification 1 and 2 in Table 5 are related to this analysis. In Specification 1 the dependent variable is *Payoff1* and in Specification 2 it is *Payoff2*. We include treatment dummies to both specifications and control for variables that might influence the dishonest behaviour. The set of control variables includes gender, age, praying frequency, corruption proxy in the form of CPI index ranking from the year 2008, and subjects' CRT scores.[45] In both specifications the variable NS (subjects' narcissism scores) are statistically significant (with p-values < 0.01).[46] This is a strong evidence that narcissistic individuals are more prone to lying. Our findings are in line with empirical studies such as Rijsenbilt and Commandeur (2013) where they show that narcissistic CEOs are more likely to enter the courtroom due to fraudulent activities and Amernic and Craig (2010) where they documented a strong link between CEO narcissism and unethical actions.

Insert Table 4 & 5 from Appendix IV here

Cognitive ability and payoff difference: We find that there is a strong link between subjects' cognitive ability as measured by the CRT and the extent to which subjects are influenced by intervention between two reporting stages. The CRT scores (Frederick, 2005) consists of the following three questions: "(1) A bat and

[45] We choose the year 2008 for the corruption ranking. We assume that subjects participated in the experiment could not have affected the level of corruption in their respective countries in the year 2008.

[46] The results are the same if we use an ordered logit regression.

a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost? (2) If it takes five machines 5 min to make five widgets, how long would it take 100 machines to make 100 widgets? (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?" The CRT score corresponds to the total number of correct answers and varies from 0 to 3.

Figure 3 shows the payoff difference between the first and second stages of the game assumed to have resulted from the intervention in the form of a message or video clip (*Payoff difference = Payoff2 -Payoff1*). We also include observations associated with treatment *plain* in this analysis. Although subjects did not see any message or were not interrupted by any video clips between the reporting stages in this treatment, there was a brief pause between two stages which might play into their minds. A clear pattern emerges as displayed by Figure 3, individuals with higher cognitive abilities (i.e. with higher CRT scores) seems to be less affected by a message, a video clip, and a pause. Participants with the lowest CRT score, score 0, are associated with the highest negative difference (-0.434) suggestive of the fact that they were heavily influenced by interventions. They generally report a lower amount in the second period. Participants with the highest CRT scores, on the other hand, generally reported a more significant number in the second period of the experiment. We also report the correlation coefficient between CRT and payoff difference (Panel A of Table 4). The Pearson correlation coefficient is positive and statistically significant with a p-value < 0.05, implying the fact that subjects with higher cognitive ability are less likely to be influenced by an intervention. The MWU test p-values associated with the distribution differences for the variable (*Payoff2-Payoff1*) are reported in Panel B of Table 4. The variable (*Payoff2-Payoff1*) is significantly higher for the group of subjects with the highest CRT score (CRT score of 3) when compared to the group with the lowest CRT score (a score of 0). The corresponding two-sided MWU test p-value is less than 0.01. The same pattern emerges when we compare the group with a 0 CRT score to the group with a CRT score above 0 and high CRT group (CRT score 3) with low CRT group

(CRT score < 3). The payoff difference associated with the subjects with a positive CRT score is significantly higher (two-sided MWU p-value < 0.05) than the payoff difference associated with subjects with a positive CRT score.

Insert Figure 3 from Appendix IV here

It is understood that this pattern might be influenced by the nature of the intervention that our subjects were exposed to and for that reason, we run a regression using the variables under consideration. Model 3, 4, and 5 of Table 5 reports the results. Here the dependent variable is the difference between the payoffs (*Payoff2 – Payoff1*). We control for the treatment effects by including a dummy for each treatment (except the plain treatment) in our ordinary least squared regressions. We also control for other variables that may influence the dependent variable under consideration. The set of control variables includes gender, age, praying frequency, corruption proxy in the form of CPI index ranking from the year 2008, and subjects' narcissism scores. It is found that the coefficient associated with CRT concerning the payoff difference is positive and statistically significant at less than 0.05, i.e. p-value <0.05, suggesting different forms of interventions did not manipulate subjects with higher cognitive ability. The decision scenarios in both stages are identical, and there is no reason why a subject should report a lower amount in the second stage unless the intervention plays a significant role.

4.2 Experiment two results

4.2.1 Lying under responsibility

Table 6, 7, and Figure 4 presents results associated with this part of the study. Lying was spotted in both responsibility treatments. As discussed previously, in an honest pool of subjects, the mean number reported would be 2.50. In both responsibility sample, the reported number is significantly different from the full honesty benchmark of 2.50. Based on a one-sample t-test, we can reject the null hypothesis that the mean payoff (ECU 3.75) claimed by the subject in TRF is not different from 2.50 (two-sided p-value < 0.0001). As shown in Table 6, the t-test

value based on one-sample t-test is > 4.70 in TRF. The surprising observation is that subjects exhibited dishonesty even in the absence of any monetary incentive. In TRF, subjects received a fixed payoff of ECU 4.00, and they knew that there was no monetary gain from overstating the die-roll outcome. The mean value of the reported number (i.e. ECU 3.75) is very similar to the overall first stage mean for the prior treatments and is slightly higher than the mean value of 3.52 in the study of Fischbacher and Föllmi-Heusi (2013). Ezquerra et al. (2018) showed that subjects do not cheat in the die-rolling experiment in the absence of an economic incentive. In our TRF treatment, subjects are not provided with any economic incentives. The only incentive is the feeling of being responsible for someone else's welfare. The type of lie that is conceivable in this setting can be termed as an altruistic white lie (Erat and Gneezy, 2012), a lie that benefits others at the expense of the liar, assuming that there is an internal cost of lying.

Insert Table 6 & Figure 4 from Appendix IV here

Cheating behaviour is even more prominent in TRV treatment. Based on a one-sample t-test, we can reject the null hypothesis that the mean payoff (ECU 4.64) claimed by the subjects in TRF is not different from 2.5 (two-sided p-value < 0.0001). As shown in Table 6, the t-test value based on one-sample t-test is > 11.00 in the TRV treatment. We observe an excessive level of overreporting of private information in a setting that provides the subjects to tell a Pareto white lie, a lie that benefits both the liar and the passive dependent. The average payoff reported by subjects under responsibility associated with the TRV treatment is 4.64, which is much higher than any other mean value we have observed in the previous treatments (the highest for the previous treatments including the second stage reporting is 4.01, the exchange rate was different though). The proportion of subjects who reported the maximum amount (i.e. ECU 5.00) is an astonishing 82.14 per cent (see Figure 4). This value is again higher than any other treatment in our experiment.

A comparison between treatments show that lying is more prevalent in TRV, i.e. subjects are more likely to tell a Pareto white lie than a pure (altruistic) white lie. Based on a two-sided MWU test, we can reject the hypothesis that the payoff

distribution in TRV is the same as the payoff distribution in TRF (p-value < 0.01). It appears that responsibility does not reduce misreporting (overstating) of privately observed information. The disproportionately high level of lying seemed to be fuelled by the fact that it was easier for the subjects to justify the lie (overstating) as others are also benefitted from this ("I am doing for the other person in my group"). The reporting pattern in the TRF appears to be the same as in the case of other treatments. The mean (ECU 3.75) is very similar to the overall first stage means for the treatments in Experiment one. It appears that responsibility does not reduce misreporting (overstating) of privately observed information.

4.2.2 Gender differences

In the first experiment, we did not observe any substantial influence of gender on the preference for honesty. Some of the studies in the past revealed a gender effect (i.e. men are slightly more likely to lie), but often the effect is small or trivial (Childs, 2012; Erat and Gneezy, 2012; Fischbacher and Follmi-Heusi, 2013; Abeler et al., 2014). When we consider all the observations, the comparison between male and female shows that male subjects are more prone to lie when lying generates some economic benefits for others (see Table 7). The payoff distribution of male subjects is significantly different from the payoff distribution of female subjects (two-sided MWU test p-value < 0.001). A comparison between the treatments (TRF vs TRV) revels that the gender effect is weaker in a setting when there are no economic benefits from lying, i.e. male subjects are more likely to lie under responsibility (tell a Pareto white lie) when lying generates some economic benefits for themselves (and off course for another individual). The two-sided MWU test p-value associated with the gender comparison is less than 0.02 in treatment TRV. We find 16 out of a total of 16 male subjects in treatment TRV reported the maximum payoff (i.e. payoff ECU 5.00). This implies that male subjects found it easier to justify lying when lying benefits other individuals as well as the liar. On the other hand, female subjects are less prone to lie purely for altruistic purposes than their male counterparts, but the effect is not significant (two-sided MWU test p-value = 0.116).

Insert Table 7 from Appendix IV here

5. Generalizability of Experimental Results

We observed heterogeneous preferences for truth-telling in every treatment in both experiments. The pattern is in line with several studies in the literature and ensures that our subject pools are internally valid. The external validity of the study can be questioned due to smaller stakes used in the experiments. Stake in the honesty literature, however, was not found to play the usual role. One might expect the propensity to lie would increase when stakes are also increased. Studies such as Fischbacher and Föllmi-Heusi (2013) and Kajackaite and Gneezy (2017), nevertheless, showed that subjects did not lie more when stakes are increased by several folds. Abeler et al. (2019) also find that subjects refrain from reporting the highest amount when the stakes are raised by several hundred folds. The reason, as suggested in Abeler et al. (2019), is perhaps the fact that when the stakes are high, the psychological costs of lying is also high. Based on these findings, we can suggest that in real-life scenarios, when the stakes are much higher, we should observe qualitatively similar patterns.

One might argue whether the influence of the interventions will be less emphatic in the general population. Studies in the past that compared student subject pools with non-student samples mostly showed that students are more dishonest than non-student (i.e. more representative) subject pools (Abeler et al., 2014; Fosgaard et al., 2018). So the concern that some of the treatment effects are due to the fact that students are more malleable compared to the general population is likely to be invalid. The size of the reward from dishonest actions is a central component of experimental studies on honesty. We used a relatively small stake in our honesty experiments and did not help the external validity argument. It might appear to be counterintuitive, but studies on honesty suggest that bigger stakes might lead to more honesty as the psychological costs of acting in a dishonest manner go up (see Mazar et al., 2008; Abeler et al., 2019). Our findings related to narcissism and honesty (in Experiment 1) should assuage some generalizability concerns. We document a positive association between narcissism and dishonest behaviour which in line with empirical studies based on real-world data such as Rijsenbilt and Commandeur (2013) where they show that narcissistic CEOs are more likely to be

involved in fraudulent activities and Amernic and Craig (2010) where they showed a strong link between CEO narcissism and unethical actions. The findings in Paper 1 in this thesis also agrees with this result. We do not directly study why narcissistic CEOs face more activism events, but in light of findings from prior studies, it can be suggested that it is possibly because of dishonest actions and self-serving behaviour.

6. Conclusion

Situations in which an individual can generate economic benefits by holding back privately possessed information are unavoidable in most societies. Examples are plentiful in the field of economics, psychology, and political science. The experimental study adds to the literature of honesty and lying by shedding lights on the effects of message framing and emotional states on the way individuals report privately observed outcomes. The study shows how two dimensions in the form of narcissism and cognitive ability can influence lying behaviour. In addition, the study contributes to the literature by exploring the influence of responsibility on reporting behaviour of privately observed information.

The paper consists of two experiments; we ran experimental sessions at The University of Nottingham (UK). In both experiments, we use the die-rolling game, where participants can make more money (for themselves and others in some treatments) by misrepresenting (i.e. overreporting) the outcome of the die-rolling task. Data from the first experiment show that lying behaviour was at large before and after the intervention. The distribution of reported payoffs is monotonic in all treatments suggesting that dishonest reporting behaviour was prevalent. However, in some treatments, the post-intervention payoffs are significantly smaller than the first stage payoffs, indicating a treatment effect. Chong and Druckman (2007) stressed the importance of framing and suggested that the elite can manipulate public opinion on an issue through framing. For example, the opinion on using tax loopholes to avoid paying taxes by business entities may depend on whether the elite chooses to frame the action as immoral or a smart business move. We

experimentally show that a negative message about dishonest actions encourages some subjects to cheat less.

We also show that a positive emotional state (believed to be contentment) elicited by a short video clip can prod some subjects toward honesty. Our result indicates a strong and positive association between the subjects' level of narcissism and propensity of lying, i.e., narcissistic individuals are more likely to report a higher payoff. Moreover, we find that there is a definite link between subjects' cognitive abilities as measured by the CRT and the extent to which an intervention between two reporting stages influences them. High CRT individuals seem to be less influenced by an intervention, i.e., they are less likely to change their lying behaviour following an intervention. Based on the observations, however, we cannot suggest that individuals with higher cognitive ability do not have psychological costs of lying. We are more inclined to suggests that they are less likely to be influenced by external factors.

The over-reporting phenomenon was also observed under responsibility. In fact, we observed that responsibility could amplify the magnitude of lying when coupled with monetary incentives. The pattern is indicating that added responsibility can compromise the intrinsic honesty of individuals. Most studies thus far treated responsibility as a positive force and debiasing tool. We argue that responsibility in some contexts can prompt dishonest behaviour. Shalvi et al. (2012) suggest that it is easier for individuals to cheat when they justify that unethical action to themselves. In many day-to-day affairs, individuals represent other people's interest, and in countless situations, the decision-maker's own interests are also aligned with the interests of the recipient. In circumstances like this, individuals might find it easier to justify dishonest actions since they might be thinking that their actions are entirely self-serving. We argue that the psychological costs of lying may be lower when others are benefitting from dishonest actions. Finally, gender composition is important under responsibility. Males are more prone to dishonesty compared to females when dishonesty benefits others as well as themselves.

The study contributes to a few works of literature. First, we add to the growing literature on lying behaviour in a non-strategic environment. We consider the influence of non-monetary factors on lying behaviour in our first experiment. Subjects first report the die-rolling outcome in a neutral environment and then in a loaded environment. It is shown that manipulating seemingly trivial situational factors can influence dishonesty. Priming subjects with a message and video clips can lead people to act in a more pro-social manner. Our finding is consistent with Mazar et al. (2008) and Gino et al. (2009), but we use different methods to remind subjects of moral concepts. The study also experimentally documents a positive association between high-level narcissism and self-serving behaviour, which is in line with the previous findings that associate narcissism with greed and self-serving tactics. We also contribute to the strand of literature that links cognitive ability to different economic and non-economic scenarios by documenting the fact that individuals with a high level of cognitive ability are less likely to be influenced by moral framing. Our work is related to Mead et al. (2009) and Gino et al. (2011), who showed a positive association between cognitive depletion and unethical actions. Our contribution, however, is different from their contributions since we focus on the link between cognitive ability and the influence of an exogenous intervention on subjects. Finally, we show that responsibility can encourage individuals to lie more as it possibly reduces the psychological cost of lying since others are also benefited from the misreporting of privately observed information. Our results are in line with the notion that individuals lie more when it is easier to justify their actions (Gächter and Schultz, 2016). Although it is generally believed that lying is a bad thing and nothing good can come from it (Kant, 1949 [1785]), white lies, i.e. lies that benefits another individual, has been getting supports from moral philosophers (e.g. Bentham, 1789) for a very long time. Studying white lie situations is important as welfare-increasing unethical actions with high economic significance is often observed in organisations (Erat and Gneezy, 2012). For example, managers giving employees incorrect feedbacks to enhance their confidence. The second study shows that people lie not only due to self-concern but also due to concerns for others. When both incentives are aligned, the level of lying is very high.

One promising path for future studies is to explore the role of accountability in situations when people are given the opportunity to act dishonestly. We study the influence of responsibility on honesty. The decision was made in a non-strategic environment as the recipient is entirely passive and do not play any role whatsoever. Incorporation of accountability would certainly induce strategic behaviour from both parties in agency situations. Future studies can also investigate the link between inequality aversion and white lie. For example, studies can vary the fixed payoff to the reporters to check whether altruistic white lies are less frequent when the payoff is lower, i.e. when the payoff is perceived as unfair by the subject. Studies can also focus on the perception of pro-social behaviour and lying. Dishonest actions can be easily justified when the situation is not very complicated. For example, in experiment two, subjects could easily justify lying since someone in the laboratory (the passive group member) was also benefited from dishonest actions. We do not know whether subjects were thinking about the loss of the experimenter. Additional treatments can be conducted where subjects can be explicitly told that experimenters lose more money if the magnitude of lying is too high. Finally, there should be more research works on honesty that pay attention to cultural differences. Most of the studies on honesty were carried out in developed countries, even though corruption is a bigger problem in less developed parts of the world. Future research works that explore cross-country heterogeneity will further our understanding of factors that determine the level of honesty in individuals.

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7. Appendix IV

Figure 1 (First experiment): Percentage of reported number by subjects in different treatments in the first experiment. *Payoff1* is the reported payoff in first stage of the experiment and *Payoff2* is the reported payoff in the second stage of the experiment.

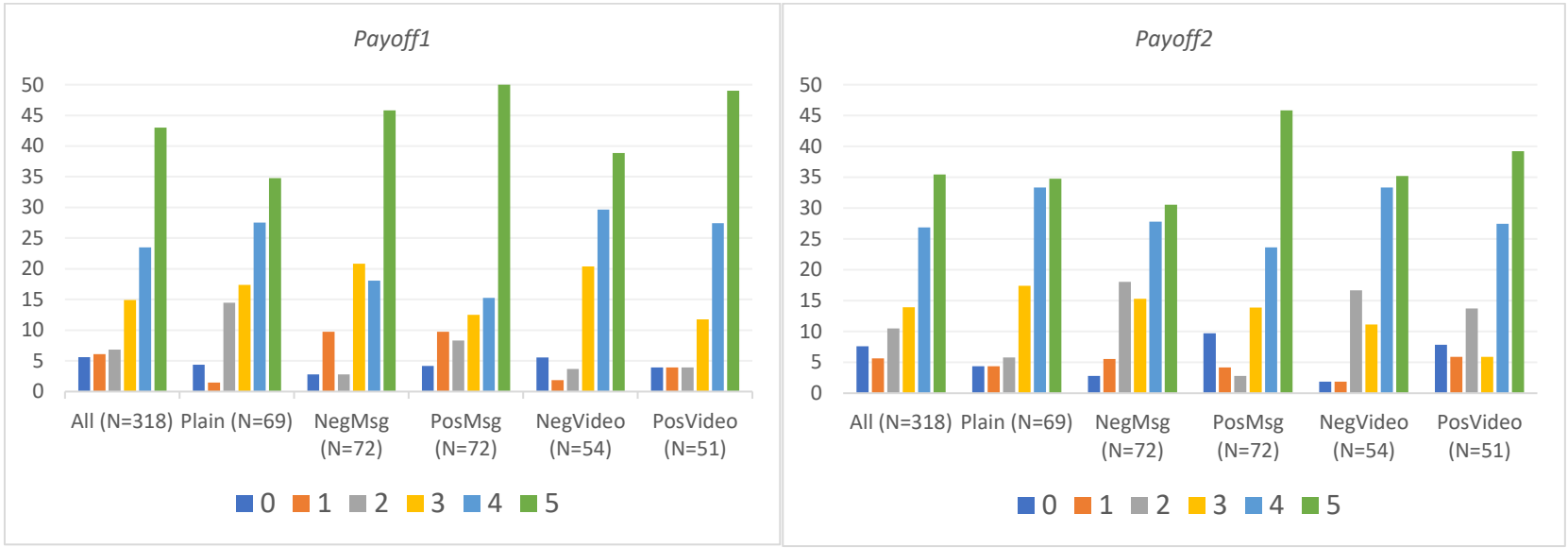


Figure 2 (First experiment): Average payoff, i.e. the mean of payoffs in stage 1 and stage 2 in each treatment, is reported. The number of subjects is reported in the parenthesis. The black solid line represents the full honesty benchmark, i.e. a payoff ECU 2.5.

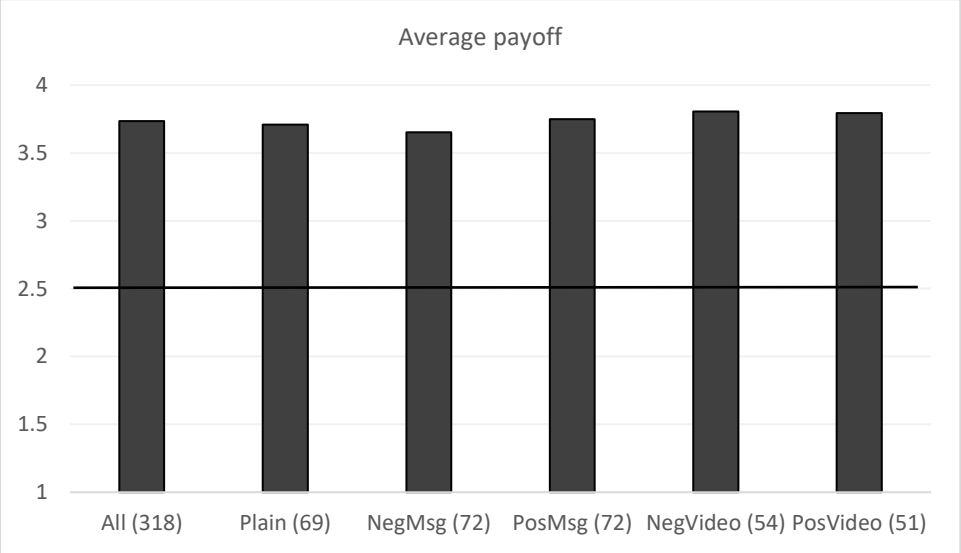


Figure 3 (First experiment): CRT scores and payoff difference. Here payoff difference is the difference between the claimed payoff in the first stage and the claimed payoff in the second stage by the same subject (*Payoff difference = Payoff2 -Payoff1*). The cognitive reflection test scores (CRT scores) are based on Frederick (2005). The CRT scores ranges between 0 and 3. The CRT consists of the following three questions: (1) A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost? (2) If it takes five machines 5 min to make five widgets, how long would it take 100 machines to make 100 widgets? (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? The CRT score corresponds to the total number of correct answers and varies from 0 to 3.

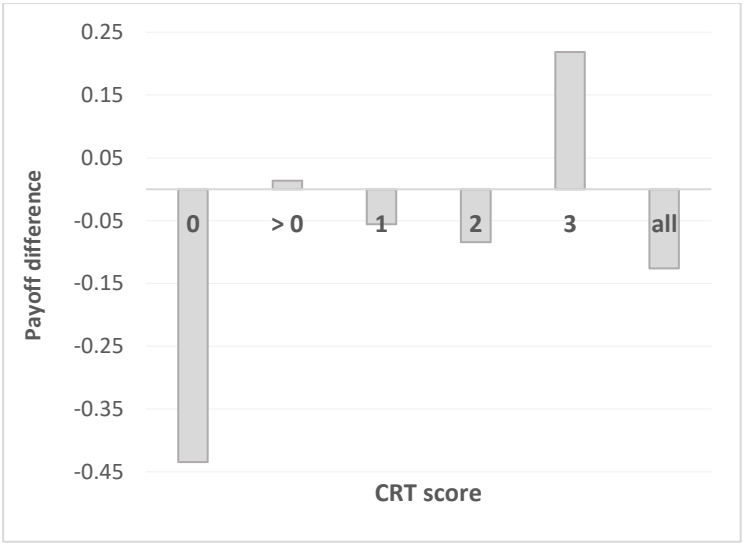


Figure 4 (Experiment two): The figure shows the percentage of subjects reporting the corresponding payoff (as per the payoff rule in Chart 1) in each treatment. The total number of subjects in each treatment is 28.

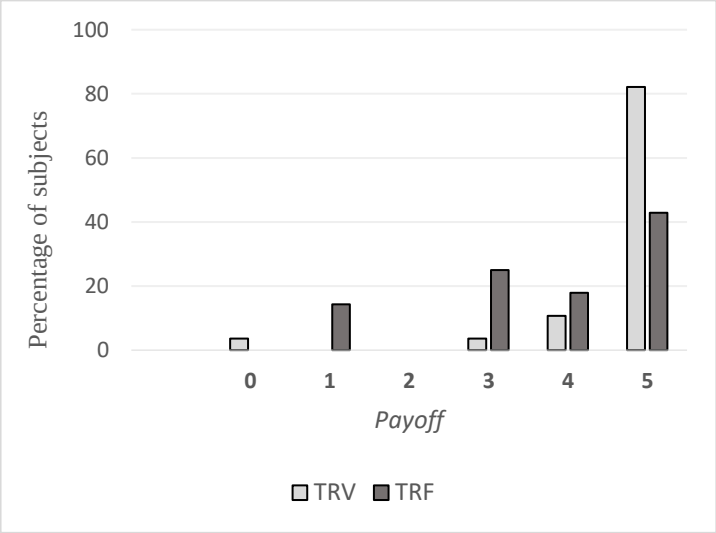


Table 1 (First experiment): Table presents the summary statistics associated with the payoffs reported by the participants in the first experiment. *Payoff1* is the reported payoff in the first stage of the experiment and *Payoff2* is the reported amount in the second stage of the experiment. P-values are reported in the parenthesis; stars display the significance levels; *,**, and *** indicates whether *Payoff2* is significantly lower than *Payoff1* based on a one-tailed p-value (from the t-test) at 10 per cent level, 5 per cent level, and 1 per cent level respectively.

Payoff	**N**	**Mean**	**Std. Dev.**	**Min**	**Max**
Full sample					
Payoff1	318	3.79	1.496	0	5
Payoff2	318	3.67	1.556	0	5
Treatment: *Plain*					
Payoff1	69	3.67	1.358	0	5
Payoff2	69	3.75	1.344	0	5
Treatment: Negative message *(NegMsg)*					
Payoff1	72	3.79	1.443	0	5
Payoff2	72	3.51*	1.384	0	5
Treatment: Positive message *(PosMsg)*					
Payoff1	72	3.75	1.572	0	5
Payoff2	72	3.75	1.616	0	5
Treatment: Negative reaction to video message *(NegVideo)*					
Payoff1	54	3.83	1.342	0	5
Payoff2	54	3.78	1.254	0	5
Treatment: Positive reaction to video message *(PosVideo)*					
Payoff1	51	4.02	1.334	0	5
Payoff2	51	3.57**	1.628	0	5

Table 2 (First experiment): Table repots the proportions of subjects reporting the highest payment in both stages. Pr_i^5 is the proportion of subjects reporting the maximum payoff, i.e. a payoff 5, in the corresponding stage where *i ϵ {1,2}*. Minus (plus) sings are based on one-sided p-values from a binomial test suggesting that the frequency in the second stage is statistically significantly smaller (larger) than the proportion associated with the first stage; - (+) at 10 per cent level, --(++) at 5 per cent level, and --- (+++) at 1 per cent level.

Treatment	N	Pr_1^5	Pr_2^5
All	318	0.437	0.371^{---}
Plain	69	0.348	0.348
NegMsg	72	0.458	0.306^{---}
PosMsg	72	0.500	0.458
NegVideo	54	0.389	0.352
PosVideo	51	0.490	0.392

Table 3 (First experiment): Estimations of the mean per cent of dishonest subjects for each treatment in both stages of the experiment are reported. The estimation method is developed by Garbarino et al. (2018), which allows estimating the mean per cent of individuals who lie when reporting the outcome of a random device, conditional on having received a bad outcome.[47] To estimate the mean lying rate, following information are used: N = total number of subjects who observe the outcome of a random device; R = number of subjects who report the high payoff outcome ($0 \leq R \leq N$) ; p = probability of receiving the low payoff outcome [0%,100%]. In the first case, a payoff is considered high (low) payoff if the reported amount is greater (lower) than ECU 3 (ECU 4). In the second case, a payoff is considered high (low) payoff if the reported amount is greater (lower) than ECU 4 (ECU 5). The study employs the one-sample test of proportion to calculate the p-values indicating whether the percentage in the second stage is significantly lower or higher than the percentage associated with the first stage. Minus (plus) signs are associated with the one-sided significance from a one-sample test of proportion suggesting that the mean per cent of dishonest subjects in the second stage is statistically significantly smaller (larger) than the mean per cent associated with the first stage; - (+) at 10 per cent level, --(++) at 5 per cent level, and --- (+++) at 1 per cent level.

Estimated mean per cent of dishonest participants						
	All	*Plain*	*NegMsg*	*PosMsg*	*NegVideo*	*PosVideo*
N	318	69	72	72	54	51
Assuming 4 or 5 as the higher payoffs						
Stage 1	49.92	43.06	45.45	47.54	52.32	64.35
Stage 2	48.98	51.82^{+}	37.05^{-}	53.84	52.32	49.49^{--}

[47] Available at: http://lyingcalculator.gate.cnrs.fr/

			Assuming 5 as the higher payoff			
Stage 1	32.41	19.78	34.82	39.83	26.39	38.58
Stage 2	24.48^{---}	19.78	16.45^{---}	34.82	21.93	26.77^{--}

Table 4: Panel A of the table reports the Pearson correlation coefficients; p-values are reported in the parenthesis. *Payoff difference = Payoff2 -Payoff1*. The variable NS is the level of narcissism in subjects measured by the 40-item Narcissistic Personality Inventory or NPI-40. The cognitive reflection test scores (CRT scores) are based on Frederick (2005). The CRT scores ranges between 0 and 3. The CRT consists of the following three questions: (1) A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost? (2) If it takes five machines 5 min to make five widgets, how long would it take 100 machines to make 100 widgets? (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? The CRT score corresponds to the total number of correct answers.

Panel B of the table reports the p-values associated with two-sided MWU test conducted to check whether the variable payoff difference distribution for various CRT groups are significantly different from each other.

Panel A

	Mean payoff	Payoff difference	NS
Payoff difference	0.022 (0.698) N=318	-	
NS	**0.167** (0.003) N=318	-0.001 (0.998) N=318	-
CRT	0.059 (0.292) N=318	**0.132** (0.018) N=318	**-0.158** (0.005) N=318

Panel B

	CRT = 0	CRT = 1	CRT = 2	CRT < 3
CRT = 1	0.163	-		
CRT = 2	0.217	0.835	-	
CRT = 3	**0.009**	0.303	0.185	**0.031**
CRT > 0	**0.030**	-	-	-

Table 5 (First experiment): Table reports the OLS regression results associated with reported payoffs from die-rolling experiments in both stages. In specification one the dependent variable is *Payoff1*, and in specification 2 the dependent variable is Payoff2 (*Payoff1* is the reported payoff in the first stage of the experiment and *Payoff2* is the reported amount in the second stage of the experiment). In specification 3,4, and 5, the dependent variable is *Payoff difference = Payoff2 - Payoff1*. *NegMsg, PosMsg, NegVideo,* and *PosVideo* are the treatment dummies. NS is the level of narcissism in subjects measured by NPI-40; *Female* is a dummy variable; *Pray* is the praying frequency value reported by subjects (1 to 6). CRT scores range between 0 and 3; The CRT consists of the following three questions: (1) A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost? (2) If it takes five machines 5 min to make five widgets, how long would it take 100 machines to make 100 widgets? (3) In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake? The CRT score corresponds to the total number of correct answers. CPI2008 is the corruption perception index of a subject's country in the year 2008. *Payoff1* is the payoff in the first stage of the experiment. P-values are reported in the parenthesis; P-values are based on robust standard error; stars display the significance levels; *10 per cent level, **5 per cent level, ***1 per cent level).

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	Payoff1	Payoff2	Payoff difference	Payoff difference	Payoff difference
NegMsg	0.093	-0.330		-0.394	-0.394
	(0.689)	(0.132)		(0.148)	(0.148)
PosMsg	0.024	-0.116		-0.126	-0.133
	(0.922)	(0.620)		(0.660)	(0.646)
NegVideo	0.051	-0.108		-0.135	-0.144
	(0.843)	(0.663)		(0.667)	(0.659)
PosVideo	0.133	-0.373		-0.489	-0.465
	(0.612)	(0.181)		(0.110)	(0.161)
NS	0.036***	0.030***			0.005
	(0.004)	(0.010)			(0.703)
CRT	-0.015	0.186***	0.196**	0.189**	0.196**
	(0.845)	(0.008)	(0.013)	(0.016)	(0.028)
Female	0.258	0.157			-0.022
	(0.131)	(0.319)			(0.911)
Age	-0.039	-0.006			0.021
	(0.177)	(0.841)			(0.623)
Pray	-0.046	-0.063			-0.031
	(0.349)	(0.182)			(0.596)
CPI2008	0.034	0.022			-0.001
	(0.488)	(0.648)			(0.981)
Payoff1		0.309***			
		(0.000)			
CONST	3.958***	2.117**	-0.391**	-0.162	-0.620
	(0.000)	(0.031)	(0.011)	(0.510)	(0.620)
N	314	314	318	318	314

Table 6 (Experiment two): Panel A of the table presents some statistics associated with the variable in Treatment TRV and TRF. *, **, and *** indicates whether the distribution of the payoffs in TRV is significantly different than the distribution of payoffs in TRF based on a p-value (from a two-sided MWU test) at 10 per cent level, 5 per cent level, and 1 per cent level respectively.

Panel B shows the treatment effects.

Panel A

	N	Mean	Median	SD
Payoff in TRV	28	4.643***	5	1.026
Payoff in TRF	28	3.750	4	1.404

Panel B

	TRV	TRF	p-value (two-sided MWU test)
Mean payoff	ECU 4.64	ECU 3.75	0.002
Difference with full honesty benchmark (2.50)	ECU 2.14 (t-statistics = 11.05)	ECU 1.25 (t-statistics = 4.71)	
Percentage of subjects who reported the highest amount (ECU 5.00)	82.14 per cent	42.86 per cent	

Table 7 (Experiment two): Gender differences.

	Overall		**TRF**		**TRV**	
	N	Payoff	N	Payoff	N	Payoff
Male	24	4.75	8	4.25	16	5.00
Female	32	3.78	20	3.55	12	4.17
p-value (two-sided MWU test)		(0.000)		(0.119)		(0.016)

A4.1 Instructions used in the first experiment:

Thank you for agreeing to participate in today's experiment. You are about to participate in some decision-making experiments and at the end of the session you will be paid in cash for your participation. Different participants may earn different amounts, depending upon their decisions and/or the decisions of other participants. The instructions which we have distributed to you, are solely for your private information. Please do not communicate with the other participants during the experiment. Please raise your hand if you have questions; an experimenter will help you.

This experiment consists of **two identical stages**.

In both stages, you determine your own payoff by throwing a dice in a cup **twice**. Your **first throw** decides how much money you make in each stage.

Money in this experiment is expressed in **ECU.**

The following chart shows the exact payoff:

Number thrown	Payoff
1	ECU 1.00
2	ECU 2.00
3	ECU 3.00
4	ECU 4.00
5	ECU 5.00
6	ECU 0.00

For example: if the result from the first throw is "3", your payoff will be ECU 3.00 for the corresponding stage.

The second throw only serves to ensure that the dice is working properly. **So only the first throw counts.**

Your tasks and payoff rules in both stages are exactly the same, i.e. you will repeat Stage 1 tasks in Stage 2.

Your payment from this experiment is computed based on your **total payoff from Stage 1 and Stage 2**. The exchange rate is:

ECU 3 = GBP 1

A4.2 Instructions used in treatment TRF (experiment two):

Thank you for agreeing to participate in today's experiment. You are about to participate in a decision-making experiment and at the end of the session you will be paid in cash for your participation. Different participants may earn different

amounts, depending upon their decisions and/or the decisions of other participants. The instructions which we have distributed to you, are solely for your private information. Please do not communicate with the other participants during the experiment. Please raise your hand if you have questions; an experimenter will help you.

In this experiment, you will be a part of a **TWO-member group**. At no point during or after the experiment other participants in the experiment learn your identity. In the same manner, you do not find out the identity of other participants at any point during or after the experiment.

The game lasts for **1 period**. Each group has

- **A leader**
- **One passive member**

Money in this experiment is expressed in **ECU** (ECU 1 = 1.25 GBP)**.**

The leader determines the payoff for the passive member of the group by throwing a dice in a cup **twice**. The **first throw** decides how much money the group make in this experiment.

The leader receives a FIXED amount (ECU 4.00) for performing the tasks on behalf of the passive member in the group.

The following chart shows the exact payoff for the passive member of the group:

Number thrown	Payoff
1	ECU 1.00
2	ECU 2.00
3	ECU 3.00
4	ECU 4.00
5	ECU 5.00
6	ECU 0.00

For example: if the result from the first throw is "3", the payoff for the passive member will be ECU 3.00.

The second throw only serves to ensure that the dice is working properly. **So only the first throw counts.**

Payoff calculation:

The payoff for passive member of a group depends on **leader's** choice. Three examples are discussed:

Example 1

- If the result from the first throw is “3”, the leader will get a fixed amount of ECU **4.00.**
- The Passive member in the group will receive ECU **3.00**

Example 2

- If the result from the first throw is “6”, the leader will get a fixed amount of ECU **4.00.**
- The Passive member in the group will receive ECU **0.00**

Example 3

- If the result from the first throw is “4”, the leader will get a fixed amount of ECU **4.00.**
- The Passive member in the group will receive ECU **4.00**

The exchange rate for this experiment is the following:

ECU 1 = 1.25 GBP