Risky choices in strategic environments: An experimental investigation of a real options game

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Abstract

Managers frequently make decisions under conditions of fundamental uncertainty due the stochastic nature of the outcomes and competitive rivalry. In this study, we experimentally test a theoretical model under fundamental uncertainty and competitive rivalry by designing a sequential interaction game between two players. The first mover can decide either to choose a sure outcome that assigns a risky outcome to the second mover or to pass the decision to the second mover. If the second player gets the chance to decide, she can choose between a sure outcome, conditioned by the assignment of a risky payoff to the first mover, or the sharing of the risky outcome with the first mover. We then introduce the following experimental treatments: i) relegating second-mover participants to a purely passive role and substituting them with a random device (absence of strategic uncertainty – that is, when the source of uncertainty is a human subject); ii) providing information about the behaviour of second-mover counterparts; and iii) completely removing the second-mover participant.

We find that decision makers are sensitive to the presence or absence of strategic uncertainty; indeed, in the presence of strategic uncertainty, first movers more often diverge from the behaviour predicted by the model. Given our experimental results, the theoretical model needs to be revisited. The standard model of monetary payoff-maximizing agents should be substituted by one of decision makers who maximize a utility function which includes the psychological cost induced by strategic uncertainty.

Keywords: Behavioural OR, Strategic uncertainty, Fundamental uncertainty, Continuous distributions, Real options games; Laboratory experiments

1. Introduction

Firms must often make investments decisions when a project's commercial prospects are uncertain. There are two related sources of uncertainty: *fundamental uncertainty* over the future rewards from the investment (Dequech, 2000; Dosi & Egidi, 1991) and *strategic uncertainty* due to the possible reactions of the firm's competitors (Smit & Trigeorgis, 2007). Both sources of uncertainty can impact the value of investment opportunities.

Ferreira, Kar, and Trigeorgis (2009) highlight the importance for firms to consider both fundamental uncertainty and strategic interactions with their competitors, suggesting the adoption of an assessment tool that combines real options with game theory, namely the real options game (ROG) approach. In this approach, real options are used to evaluate investment decisions under fundamental uncertainty while game theory provides the tools for modelling situations in which agents take into account other agents' possible reactions. During the last two decades, numerous studies, mainly anchored in industrial organization, have investigated investment decisions by implementing the ROG approach[1] (for instance see Dixit & Pindyck, 1994; Morreale, Robba, Lo Nigro, & Roma, 2017; Smit & Trigeorgis, 2007).

[1] For a literature review see Azevedo & Paxson, 2014.

Despite its cumulative theoretical contribution to research, the use of ROG decision tools has been limited (Lander & Pinches, 1998; Teach, 2003). One reason for this is that classical theoretical models do not take into account the bounded rationality of decision makers (Posen, Leiblein, & Chen, 2018). According to Thaler and Mullainathan (2008), the canonical real-options valuation tools are "unbehavioral." In other words, the representative investor in such models is not affected by the cognitive limitations and behavioral biases typical of decision-makers who operate in the "real-world" (Trigeorgis, 2014). This causes managers to exercise or terminate options that may be distorted by errors. For instance, Coff and Laverty (2001) state that "managers may erroneously exercise options" or "drop options that would lead to a competitive advantage" (p. 73).

As a consequence, several scholars (e.g., Posen, Leiblein, & Chen, 2018; Smit & Moraitis 2015) have highlighted the importance of combining behavioural economics with corporate finance in order to develop ways of improving decision making under both fundamental uncertainty and strategic uncertainty. The common denominator of this approach is the structure in which players' interconnections are developed. Indeed, several ROG studies have focused on optimal investment policies under fundamental uncertainty in a leader–follower Stackelberg game, where the investment of the leader does not completely eliminate the revenues of its rival (see, e.g. Armada, Kryzanowski, & Pereira, 2009; Siddiqui & Takashima, 2012).

Consider the following well-known example. During the 1980s, the videocassette recorder (VCR) was one of the most important segments of consumer electronics. The first VCRs were built in the early 1970s, and the U-matic format, originally developed by Sony, was the main design intended for professional use. By the mid-1970s, however, different versions of this product had led to similar, but incompatible, formats for home use. Among these were Betamax, introduced in 1975 by Sony, and Video Home System (VHS), originally developed by the Victor Company of Japan (JVC) and later supported by the majority of distributors in Japan, the United States and Europe (Cusumano, Mylonadis, & Rosenbloom, 1992).

Although Sony was the first to introduce VCRs into the domestic market, thereby exploiting its competitive advantage of having a product that gave it the largest market share during the mid-1970s, the Betamax format lost competitiveness to VHS. Indeed, the alliance formed by JVC for production and distribution was the winning factor in the triumph of VHS over Betamax, which gradually disappeared from the market (Cusumano, Mylonadis, & Rosenbloom 1992).

Sony was surely aware that its competitors would try to pre-empt it in launching the new format. This awareness posed a dilemma for Sony. Should the company anticipate its competitors by first launching its format for home videotapes, thereby securely maintaining its leadership – at least in the short run – or should it pass the ball to its competitors by adopting a "wait and see" strategy? In resolving this dilemma, Sony needed to take into account several factors, such as the maintenance or loss of its leadership, the stochastic nature of payoffs due to market uncertainty, and the actions that the second player could have implemented.

The Sony example recalls two-person investment and trust games, where leaders (trusting agents or investors) must decide whether to forgo guaranteed returns in order to trust counterparts (followers) that could provide them with higher (or lower) future returns. A growing body of behavioural literature that focuses on such games has recognized several factors – such as “disutility from loss of control, …, the costs of making incorrect assessments,.., and disutility from earning money due to other people’s kindness” (Aimone & Houser 2012, p. 573) – that influenced leaders’ decisions to pass or not to pass the decision to their counterparts.

However, one important kind of situation that this behavioural literature does not take into account is those where the players’ payoffs are stochastic, which is typical when making investment decisions in a competitive market scenario, such as the Sony story illustrates. This kind of situation is common, and it may modify the psychological/behavioural driven response of the players. Consequently, analysing the “traditional” leader–follower game in the ROG literature from the perspective of behavioural economics would improve our understanding of how individuals make decisions in stochastic and competitive environments.

This paper aims to fill this gap by reporting on a laboratory experiment that empirically tested a ROG and investigated decision making under conditions of fundamental uncertainty and strategic uncertainty – that is, when a decision made by a human being is the source of uncertainty. As O’Keefe (2016) argues, the proposed methodology of laboratory experiments has been used in the context of operations research in order to improve understanding of the modelling process and to push forward the theoretical threshold (see for example Hämäläinen et al., 2013; Lahtinen & Hämäläinen, 2016).

Starting from a normative model, we experimentally examine subjects’ choices and compare the observed behaviour to the normative model. Specifically, this study is based on the theoretical model developed by Lo Nigro, Morreale, Robba, and Roma (2013), who analyse the effect of competition on investment decisions in a stochastic environment.[2] The model is a typical two-stage leader–follower game. Solving the game through backward induction yields different scenarios of equilibrium solutions, depending on the values assumed by some of the model’s input parameters. However, for the purposes of this research, we consider only one possible scenario of equilibrium: under given values of the model’s input parameters, the Nash equilibrium is reached when the first mover – who has the opportunity to exploit the advantage of choosing first – finds it more profitable to pass this choice to the second mover. This decision maximizes the first mover’s expected payoff, assumed that the second mover – who must decide between a sure outcome, which implies the assignment of a risky payoff to the first mover, or the sharing of the risky outcome with the first mover – decides rationally, i.e. opting for the sure outcome. Note that this closely recalls the situation described in the Sony story. It is worth underlining that we concentrate our analysis only on the behaviour of the first movers.

Do individuals behave according to the normative policy suggested by the theoretical model, thus maximizing their expected payoffs? In other words, do first movers reject the sure payoffs and pass the choice to the second movers? In order to address this question, we use data generated by our first experiment. Specifically, in accordance with the theoretical model, we designed a sequential interaction between two players. If the first mover decides to maintain their role, then a sure outcome is achieved, while

[2] In this model, the value of the investment is treated as a stochastic variable that follows a geometric Brownian process.

assigning a risky outcome (due to the presence of fundamental uncertainty) to the counterpart. Otherwise, the first mover can decide to pass the choice to the second mover.

Assuming we find deviation from the optimal theoretical policy (i.e., there are first movers deciding for the sure outcome, thus not passing the choice to the second movers), we investigate whether such “irrational” behaviour could be better explained by empirical research in behavioural economics that focuses on trust and investment games. In other words, is it possible that first movers do not pass the choice to the second movers because they do not want to lose control of the game? Are other-regarding preferences occurring in these decisions?

In order to address these research questions, we designed a set of additional laboratory experiments that differ from the first experiment in: i) relegating second-mover participants to a purely passive role and substituting them with a random device (absence of strategic uncertainty); ii) providing information about the behaviour of second-mover counterparts; and iii) completely removing the second-mover participant. In this way, the experimental set “sterilizes” the psychological impact on the formation of beliefs and economic decisions due to the interaction of human agents.

This study cross-fertilizes two fields of research: ROG theory and the behavioural approach to decision-making under risk and uncertainty. Many real-world decision problems include strategic interactions and fundamental uncertainty. Although this class of problems has been studied extensively from a theoretical perspective, the theory-driven empirical approach that we have adopted has been limited. By augmenting industrial organizational theory with a behavioural approach, the experiments in this study provide insights into decision-making processes involving bounded rationality.

The remainder of the paper is organized as follows: Section 2 reviews the relevant literature and highlights our contribution; Section 3 provides an overview of the theoretical model and presents a simplified version of the model for experimental purposes; Section 4 describes the experimental design; Section 5 introduces the hypotheses; Section 6 presents and discusses the findings; and Section 7 concludes and anticipates future developments.

2. Related literature

This is an interdisciplinary paper that draws on two streams of research: financial real options and behavioural games. Although real-options theory has important implications for making investment decisions under uncertainty, empirical testing of this theory has been rare (Moel & Tufano, 2002; Yavas & Sirmans, 2005). This is primarily due to the problems that researchers encounter in obtaining “key variables” in real options – that is, reliable data on certain components of the real-options approach, such as the current and future value of the underlying asset and the variability of its value (Oprea, Friedman, & Anderson, 2009; Yavas & Sirmans, 2005). Consequently, “laboratory studies are a natural help to fill the empirical gap. All relevant variables are not only observable but also controllable” (Oprea et al., 2009, p. 1103). Accordingly, during the last decade researchers have used laboratory experiments to study the behavioural aspects of managing real options (Anderson, Friedman, & Oprea, 2010; Miller and Shapira (2004); Murphy et al., 2016; Oprea et al., 2009; Yavas & Sirmans, 2005).

In these studies, a decision maker decides, either over discrete periods (Murphy et al., 2016; Yavas & Sirmans, 2005) or continuously (Anderson et al., 2010; Oprea et al., 2009), whether to trade a sure alternative or to choose a risky option with potentially higher value. Specifically, Miller and Shapira (2004) required their participants to indicate the price to sell or to buy a call or put option for binary lotteries. They found that participants tend to underestimate options (as far as the expected payoffs are concerned), while overestimating expected losses to sell a put. Yavas and Sirmans (2005) tested the "wait and see" option in the laboratory. The results of their experiments show that in the majority of cases participants neglect the benefit of waiting and invest too early in comparison to the timing proposed by the normative model.

Oprea et al. (2009) investigated behaviour in risky "wait and see" options governed by Brownian motion. Their results indicate that the time when participants invest approximates the optimal time suggested by the theoretical model only when people can learn from their personal experience. In fact, this near optimal behaviour was only observed in the last rounds of the experiment and not at the beginning of the study. Anderson et al. (2010) extended the study of Oprea et al. (2009) to a competitive environment: a complete pre-emption investment game was utilized, theoretically and empirically. In their experiments, two or more agents have access to the same investment opportunity (whose value is publicly observed and evolves according to a geometric Brownian motion) and the agent who arrives first leaves the investment value unavailable to the others. As predicted by the theoretical model, in competition contexts subjects invest at lower values than in monopolies. Moreover, when the main parameters that influence the stochastic value are altered, subjects are more likely to behave according to the predicted direction in monopoly rather than in competitive environments. In the study by Murphy et al. (2016), a decision maker had to choose how much to invest in a risky environment that evolved over time. Their experimental results differed from the theoretical predictions.

Overall, these experimental findings show that individuals exhibit systematic deviations from the predictions derived from normative models – that is, models assuming that individuals are risk-neutral, expected-value-maximizing agents (Murphy et al., 2016). Indeed, participants neglect the benefit of waiting and invest too early in comparison to the timing proposed by the normative models, or eventually learn to wait until uncertainty is sufficiently resolved (Oprea et al., 2009). Similar to these studies, our experiment captures the central conflict between choosing a less valuable but sure alternative or foregoing it and choosing instead a risky but potentially more valuable one. Unlike most of these studies, however, we include *strategic interactions* in our analysis. The only other study that we are aware of that has done this is Anderson et al. (2010), who utilized a pre-emption game where the participant who arrives first completely eliminates the revenues of their rivals. We depart from this approach, however, since in our tested model the advantage of investing first or second is assumed to be limited, so the investment of the leader or the follower does not completely eliminate the revenues of the rival – that is, we consider a duopoly where both the first mover and the follower share the total market value (the "size of the pie") differently. Consequently, we take into account human interaction that "involves uncertainty about the consequences of one's own actions, and, in particular, the way one's actions impact other's decisions" (Aimone, Houser, & Weber, 2014, p. 1).

In this respect, our study is situated in the behavioural literature on two-person sequential games, such as investment or trust games (e.g., Fehr & Falk, 1999; McCabe, Rigdon, & Smith, 2002). Specifically, trust games design situations where a first mover is endowed with a certain amount of money and must decide whether to keep it or to send it (the entire amount or a part of it), increased by a multiplier factor, to a second mover. The second mover, in turn, can decide to keep all the amount received or to reciprocate by splitting the money received with the first mover (Berg, Joyce, & McCabe, 1995). The Nash equilibrium solution in these games is that the first mover should always accept the guaranteed returns, which means that the first mover should never trust the second mover. However, the empirical evidence shows that first movers often trust second movers. This empirical result opens the door to speculation about the reasons why the first movers deviate from the rational Nash equilibrium prediction. One possible explanation for this behaviour is that first movers bet on the expectation that second movers feel in some way a need to reciprocate trust, and thus choose to split the return. An alternative explanation for the failure of the standard prediction is that other-regarding preferences such as altruism and kindness towards others play a role in influencing the decisions of the first movers. According to this explanation, the first movers are sensible to second movers' welfare, without being worried by the fact that the second movers cannot behave in reciprocal way.

The main difference between our game and the standard trust game is that our first movers are not confronted by the decision of betting or not betting on "reciprocity-based" behaviour by the second movers, but instead they must decide whether to trust or not the second movers' rationality. In other words, the first movers' choice to pass the responsibility for decision making to the second movers is not based on any kind of reciprocity mechanism but on the belief that the second movers will choose to apply a fully rational judgement.

From this point of view, the existing literature on trust games does not offer many examples similar to our approach. In fact, trust games have usually been used by behavioural economists to investigate an array of phenomena linked to the spreading of cooperative behaviours in society (for a literature review see Johnson & Mislin, 2011). Investigating the rise of cooperative behaviour is not appropriate for our game because in our setting there is no room for cooperative strategies. Indeed, as anticipated above, in our game the first player should decide in relation to an expectation of rational behaviour by the second player and no game equilibrium can strictly be considered as the result of some kind of cooperation among the players.

Moreover, while trust games experiments have established that many people trust cooperation, some people do not (see e.g., Berg et al., 1995). The trust game literature leaves room for investigation of such a psychological "no-trust" mechanism, which is consistent with standard economic theory. Recently, several studies have suggested that an explanation for such "no-trust" behaviour could be provided by betrayal aversion (Aimone & Houser, 2012; Bohnet, Grieg, Herrmann, & Zeckhauser, 2008; Bohnet & Zeckhauser, 2004; Hong & Bohnet, 2007). These studies suggest that leaders are reluctant to take risks to get potentially higher payoffs when decisions made by human beings (followers) are the source of uncertainty.

Studies of betrayal aversion have all used a common design proposed by Bohnet and Zeckhauser (2004; see also Bohnet et al., 2008; Hong & Bohnet, 2007). In their pioneering study, Bohnet and Zeckhauser (2004) compared decisions in a two-person trust game and a risky gamble. In the trust game, trusting agents choose between a guaranteed outcome ($10) and a lower or higher outcome determined by their counterparts (trustees): if the trustee reciprocates, then $30 is split equally; if the trustee betrays, then $22 is kept by the trustee and remaining $8 goes to the trusting participant. In the risky gamble, the participants' payoffs are identical but the lower or higher outcome is determined by chance instead by a human participant. Specifically, trusting agents are asked to report the minimum acceptable probability (MAP) to be reciprocated by the trustee at which they would choose a trust or a risk gamble. In the two-person trust game, if the trusting agent reports a MAP lower than the true fraction of reciprocating trustees (p*), then the trusting agent plays the trust gamble and is paid according to the counterpart's decision. Otherwise, if the trusting agent reports a MAP higher than p*, this agent does not play the gamble and both the trusting and the trustee agents each receive $10. Similarly, in the risky gamble, if the reported MAP is lower than an unknown and predetermined p*, then both the players enter the gamble (a random device determines outcomes); otherwise, if the reported MAP is higher than p*, they receive $10.

Consistent with betrayal aversion, decision makers stated higher MAPs in the trust game compared to the risky gamble, suggesting that participants are less willing to take risks when the source of risk is another person. The same finding has been replicated in diverse cultural environments (Bohnet et al., 2008). However, as noted by Bohnet and Zeckhauser (2004), differences in the behaviour between treatments could also be explained by factors other than betrayal aversion, such as disutility from loss of control, loss aversion, or altruism.

In order to check whether betrayal aversion is a robust phenomenon, Aimone and Houser (2012) designed an experiment that allowed them to bypass many of the concerns expressed about the MAP approach. Their design differs since it does not require participants to report probabilities: they infer on revealed preferences. This approach eliminates confounds between loss aversion and betrayal aversion.[3]

Specifically, Aimone and Houser (2012) report data from a two-person trust game in which investors can choose to ignore their counterparts' decisions. Their study compares decisions from a two-person binary trust game and a "computer-mediated" binary trust game where the outcomes are the same as in the two-person trust game. Following Aimone and Houser (2013, p. 2) "The investor, … , is paid based on the decision of a computer programmed to behave the same as a counterpart randomly chosen from that same session (allowing the investor to avoid the knowledge of a specific betrayal while maintaining the same probability of high and low trust outcomes)". Keeping the probability of betrayal fixed across treatments eliminates worries related with investors' beliefs about trustees' behaviour, such as loss-aversion or altruism. Aimone

[3] "Consider, for example, a loss-averse but not-betrayal-averse subject participating in the risky dictator MAP game at her university. Suppose she is willing to engage in the risky gamble when the likelihood of an equitable outcome is a third or greater, and so reports $p = 1/3$. On the other hand, when playing the trust version of the MAP game, she may from previous interactions with her fellow students (playing in the role of trustees) expect 2⁄3 of the trustees will choose to reciprocate. That is, she may plausibly have a reference point at a 2⁄3 chance of an equitable outcome. It follows that she would perceive a 1⁄3 chance of an equitable outcome as an expected loss. Consequently, as she is loss-averse, she would report a MAP that exceeds 1⁄3 in the Trust game, despite the absence of betrayal aversion" (Aimone & Houser, 2012, p:573).

and Houser (2012) found that knowledge by investors of specific trustee's decisions drastically reduced investment, confirming that betrayal aversion is a robust phenomenon.

Our experimental design shares similarities with the experimental settings utilized by Bohnet et al. (2008), Bohnet and Zeckhauser (2004), and Hong and Bohnet (2007) since we adopt a setting where either a human being or a random process determines the outcome of the game. Moreover, following Aimone and Houser (2012), we infer based on preference-revealed choices. However, we depart from the above-mentioned studies mainly in the stochastic nature of the outcomes. Specifically, we contribute to the literature on behavioural two-player sequential games by introducing stochastic payoffs. Indeed, if the first mover forgoes the sure outcome, then their payoff is determined by their counterpart. Unlike the design utilized by Bohnet & Zeckhauser (2004) where the counterpart chooses between two sure payoffs (equal to \$15 and \$8), in our experiment, the counterpart chooses between two stochastic payoffs (log-normal distributed) to be attributed to the first mover. Introducing stochastic outcomes in our experiment transformed the structure of strategic interaction between the first and the second movers. Indeed, since the second movers cannot decide whether to reciprocate the offer in a "deterministic" way, the first movers cannot be betrayed by the second movers.

Moreover, the second movers cannot receive any "trusting offer" (like is the case in trust games and in betrayal aversion games) from the first movers. In fact, the only option left to the second mover to deviate from the behaviour expected by the first mover is to behave "irrationally", i.e. assuming the cost of an inefficient decision. In line with the specific equilibrium solution chosen in our experiment, the second movers know that the first movers have passed them the decision only because the best available choice for the second movers is to take the sure alternative for themselves. In this way, the first movers are sure to obtain from the second movers the entire risky payoff, which is the best outcome assuming risk neutrality. It follows that the second movers cannot reciprocate to any kind of first movers' trusting choice for the trivial reason that any trusting choice was not available to the first movers.

More precisely, our study introduces *disutility from loss of control* as an alternative explanation to betrayal aversion based on the hypothesis that a pure frame effect is actually triggering the different choices observed when first movers play with a human being or with a random device.

3. Theoretical background

As noted previously, we refer to a theoretical model developed by Lo Nigro et al. (2013), who adopted a ROG approach to analyse the effect of competition on alliance decisions. To better understand our experimental design, let us briefly recall the theoretical model.

3.1 Model assumptions

Consider two firms (a and b) existing in the market that are working on the development of two substitute products and are competing to establish a partnership with a third company z. The game unfolds along a time span starting at $t = 0$ and ending at $t = T$ when the two firms enter the market alone or with z if a partnership has been signed. The firms in this example are identical except that one, say a, is able to sign a partnership deal before the other. In fact, the game set-up is such that the first-moving company (*a*) is first offered a mutually exclusive alliance by company *z* (which can only ally with one company), and if an alliance is not contracted, the remaining second company (*b*) is offered the same deal. In addition, the firm that does not partner with company *z* continues the development process and goes to the market on its own, with some spillover benefits from the competitor's alliance.

All the relevant decisions taken by the firms are related to uncertain market dynamics. More precisely, the estimated value of the targeted market in case of no alliance is uncertain and can change during the interval [0, *T*]. Specifically, the model assumes that the targeted market at the generic time *t*, *Vt*, follows a geometric Brownian motion (GBM). Assuming that firms are risk neutral, the value of the targeted market at *t* – i.e., V_t – can be written as:

$$V_t = V_0 e^{(r-1/2\sigma^2)t+\sigma Z^*_t} \quad (1)$$

where V_0 is the estimated value of the market at $t = 0$, *r* is the risk-free interest rate, Z_t^* represents a Brownian process under a risk-neutral probability measure, and σ is the standard deviation of the return of the targeted market value (Merton, 1976).

At time $t = 0$, both firms invest I_0 to reserve the option to launch the product in the market jointly with company *z* or alone. At the same time, firm *z* offers a licensing deal consisting of an upfront sure payment P_0 and royalties ($0 \leq \alpha_0 \leq 1$) upon product commercialization to firm *a*. Firm *a* can decide to accept or reject the deal, depending on its outside option, which is essentially the profit it can make if it does not sign the alliance. In the case of rejection, company *z* offers the same deal to the second firm (*b*), which can accept or reject it based on its outside option. In the event that *b* also rejects the offer at $t = 0$, no alliance is signed and the game moves to time $t = T$ when company *z* offers a licensing deal (consisting of a new upfront payment P_T and new royalties, $0 \leq \alpha_T \leq 1$, upon commercialization); as above, firms are called to decide sequentially. *T* is the time of the market launch of the product, when, upon bearing the commercialization cost I_T, firms *a* and *b* pocket the projected value V_T. It is important to highlight that firm *z* plays a passive role in the contract terms – that is, the payments and the royalties offered to the two competing firms, *a* and *b*, are exogenous decisions.

Fig. 1 reports the timeline of the game. Note that while the estimated value of the targeted market evolves according to a continuous time-modelling setting, firms make their decisions (to ally or not to ally) in only two distinct periods – that is, at time $t = 0$ and at time $t = T$.

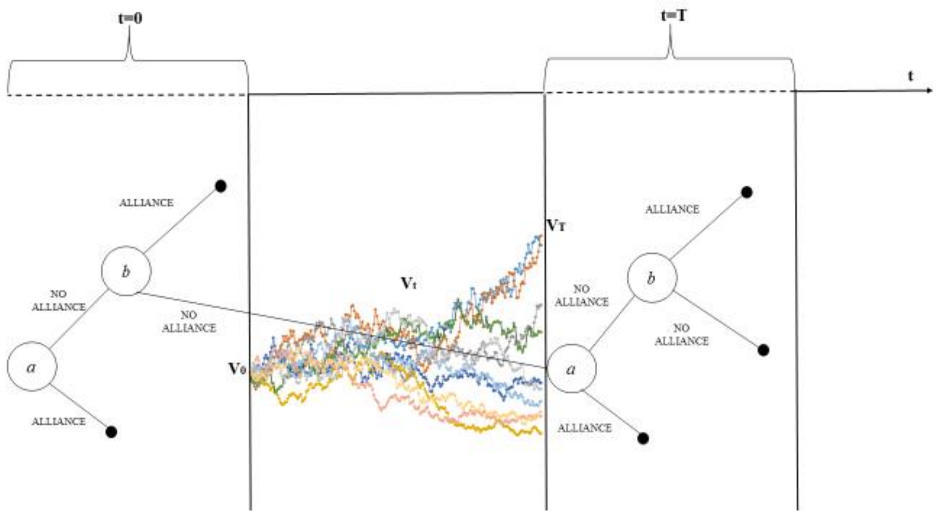

Fig. 1. Game timeline and the evolution of the market value according to a GBM. Adapted from Morreale et al. (2017, p. 1193)

It is worth underscoring that independently of when the alliance is signed ($t = 0$ or $t = T$), the value of the market is determined exogenously through a random process, which means that it is not influenced by the firms' choices. The only impact that the firms' decisions have on the market is on the overall dimension of the market itself. Specifically, in the event that neither firm *a* nor *b* signs the agreement with company *z*, they are still able to reach the final market individually. In this case, they share the (expected) market value V_T according to a parameter γ for *a* and $1 - \gamma$ for *b*. In the case an alliance is signed, it is assumed that the market size increases relatively to that of the no alliance case by an amplification factor $\delta > 1$, thanks to an improved supply effect (Smit & Trigeorgis, 2007). Moreover, it is assumed that the alliance generates spillover effects, which implies that the firm not signing the alliance will receive benefits from its rival's collaboration and enjoy a share in the larger market. However, the two firms will split the total market pie δV_t differently: the firm signing the alliance will appropriate a higher portion – that is, β (higher than γ) – while the stand-alone competitor will appropriate the remaining portion $1 - \beta$.

3.2 Payoff dynamics

The game is solved via a backward induction procedure. Specifically, we start from the final sub-game at $t = T$, where *b* has to decide whether to ally with company *z*, and go back to $t = 0$ by examining all the possible branches of the tree, as illustrated in Fig. 1.

The firms' project payoff at time *T* is max $\{S_{mT} - I_T, 0\}$, where S_{mT} varies according to the scenario considered and $m = a, b$ indicates the firm to which the payoff is referred. For example, in the event an alliance is formed between *a* and company *z* at time $t = 0$, the value S_{aT} is equal to $V_T \delta\beta(1 - \alpha_0)$. Due to the spillover effects, the value S_{bT} is equal to $V_T\, \delta\, (1 - \beta)$. In the event that neither firm *a* nor *b* signs the agreement with company *z*, the value S_{mT} for *a* and *b* is equal to $V_T \gamma$ and $V_T (1 - \gamma)$, respectively.[4]

[4] Note that because of linearity, S_{mT} follows the same distribution as that of V_T.

Let $C(S_m,t)$ denote the value at time t of this investment opportunity. Then, if the value of S_{mT} is greater than I_T, the option will be exercised by firms a and b (i.e., the product will be marketed). Otherwise, for values of S_{mT} lower than I_T, the option will be abandoned.

The value of this investment opportunity at $t = 0$ is the expected present value of these cash flows and is given by Penning and Sereno (2011):

$$C(S_m, 0) = e^{-rT} E_0^* [\max\{S_{mT} - I_T, 0\}]. \quad (2)$$

Therefore, substituting (1) in (2), equation 2 can be written as:

$$C(S_m, 0) = e^{-rT} E_0^* \left[\max\left\{S_{m0}\, e^{\left(r-\frac{1}{2}\sigma^2\right)T+\sigma Z_T^*} - I_T, 0\right\}\right] \quad (3)$$

where S_{m0} is the m's portion of the estimated value of the market at $t = 0$.

Once the firms' payoff expressions have been derived at either $t = 0$ or $t = T$, the model is solved via a backward induction procedure.

3.3 Equilibria

Solving the game yields different equilibrium solutions, depending on the value of δ as well as on the contract terms offered by company z.[5]

For the sake of completeness and simplicity, the possible outcomes are briefly described. The firm a can sign the deal with z at $t = 0$ or at $t = T$ and these outcomes are named E_1 and E_3 respectively; the same applies to firm b and the corresponding outcomes are named E_2 and E_4, while E_5 refers to the game's outcome where no agreement is signed, meaning that a and b reach the market on their own. The achievement of an equilibrium consisting in one of the five outcomes is contingent upon the value of δ, as well as the payments offered by z to firm a (first) and firm b (if a declines the offer) in the subgames unfolding at $t = 0$ and at $t = T$, namely P_0 and P_T. The backward induction procedure allows for the identification of the specific domains for δ, P_0 and P_T where the five equilibria hold. In particular for the levering factor δ, the threshold $\delta^* = (1-\gamma)/(1-\beta)$ is identified; a greater value of δ^* allows, for certain values of the payments discussed below, to enlarge the market enough to make the a's payoff coming from the spillover effects greater than the a's payoff coming from the alliance between a and z. In other words, the leader lets the follower sign the agreement with z only if the alliance between z and b allows the leader to enjoy spillover and, consequently, to achieve a Nash equilibrium in E_2 or E_4. As far as payments are concerned, these domains are delimited by the threshold function of the market value achieved by each of the two firms in the five possible outcomes.

Therefore, implementing the backward induction procedure, we start from the final sub-game at $t = T$, where b has to decide whether to ally or not with company z and go back to time t=0 involving company a's decision, examining all the possible branches of the tree illustrated in Fig. 1. Specifically, at t=T, decision makers compare two payoffs (alliance payoff and no-alliance payoff); the payoffs ranking (and, as a

[5] It is important to recall that company z is not a profit-maximizing player.

consequence, the subgame equilibria at t=T) depends of the upfront payment P_T offered by z in the deal. In particular the payoffs' comparison allows for the calculation of two thresholds for P_T: a low one ($P_T^L P_T^{\mathrm{L}}$) and a high one ($P_T^H P_T^{\mathrm{H}}$). These thresholds, in turn, limit three ranges of values for the payment itself – $P_T^L < P_T$, $P_T^L \leq P_T \leq P_T^H$, and $P_T > P_T^H$ – and for each of these ranges different branches of the tree in Fig. 1 are selected in the backward procedure. Similarly, at t = 0, for each of these ranges, low (L) and a high (H) thresholds are calculated for P_0, respectively $P_0^{L1} P_0^{\mathrm{L1}}$, $P_0^{L2} P_0^{\mathrm{L2}}$ and $P_0^{L3} P_0^{\mathrm{L,3}}$ and $P_0^{H1} P_0^{\mathrm{H1}}$, P_0^{H2} P_0^{H2} and $P_0^{H3} P_0^{\mathrm{H,3}}$. As indicated above, the payment's ranges vary between the two ranges of δ: $\delta < \delta^*$ and $\delta > \delta^*$; in particular, when $\delta < \delta^*$, the low and high values of the thresholds collapse into the low one ($P_T^L P_T^{\mathrm{L}}$ and $P_0^L P_0^{\mathrm{L}}$). Depending on the values assumed by δ, P_T and P_0, five possible equilibrium solutions are obtained (see Table 1). The reader can refer to Lo Nigro et al. (2013) for the exact values of the thresholds.

Table 1
Conditions (in terms of parameters δ, P_T and P_0) for each equilibrium.

Equilibrium	$\delta < \delta^* = (1-\gamma)/(1-\beta)$	$\delta \geq \delta^* = (1-\gamma)/(1-\beta)$
E_1	$(\forall P_T, P_0 > P_0^L)$	$(P_T < P_T^L, P_0 > P_0^{H1})$; $(P_T^L \leq P_T \leq P_T^H, P_0 > P_0^{H2})$; $(P_T > P_T^H, P_0 > P_0^{H3} = P_0^{L3})$
E_2	*Not allowed*	$(P_T < P_T^L, P_0^{L1} \leq P_0 \leq P_0^{H1})$; $(P_T^L \leq P_T \leq P_T^H, P_0^{L2} \leq P_0 \leq$ $P0H2$
E_3	$(P_T > P_T^L, P_0 < P_0^L)$	$(P_T > P_T^H, P_0 < P_0^{L3} = P_0^{H3})$
E_4	*Not allowed*	$(P_T^L \leq P_T \leq P_T^H, P_0 < P_0^{L2})$
E_5	$(P_T < P_T^L, P_0 < P_0^L)$	$(P_T < P_T^L, P_0 < P_0^{L1})$

For the purposes of this research, we consider only one possible scenario of equilibrium, which is described in detail in the next section.

3.4 Laboratory implementation of the model

In order to empirically test the model, we make several assumptions and introduce some simplifications. First, as anticipated, we consider a particular path of the tree that offers a less intuitive kind of strategic solution for the game and is closer to the real case discussed in the introduction. In order to keep our option-pricing problem as simple as possible, we make several additional assumptions. Following Miller and Shapira (2004), we consider options with zero exercise prices (i.e., I_T = 0). In this way, the market value is risky, but the associated real options value can assume only positive values. Moreover, we set $I_0 = 0$, $\alpha_0 = 1$, $\delta(1-\beta) = 1$ and $\gamma = 0.5$.[6] Specifically, we consider: $\delta > \delta^*$, $P_T < P_T^L = \gamma V_T - (1-\alpha_T)\beta\delta V_T$ and $C_{where\ S=\gamma V0} = P_0^{L1} < P_0 < P_0^{H1} = C_{where\ S=(1-\beta)\delta V0}$. As can be seen in Table 1, under these conditions, the leader firm a finds it more profitable that the other company – that is, firm b – partners with company z at

[6] Note that imposing $a_0 = 1$, $\delta(1\text{-}\beta) = 1$ and $\gamma = 0.5$ still respects the condition $\delta > (1-\gamma)/(1-\beta)$.

t = 0 (E_2 is the Nash equilibrium suggested by the model).[7] In other words, the leader should reject the alliance at t = 0 and allow the follower to make the alliance. With the selected input parameters, the threshold for P_T is the difference between the a's market value in case of no alliance and a's market value in case of alliance signed at $t = T$; while P_0 can vary in a range where the lower bound is a's market present value when no alliance is signed and the upper bound is the a's market present value when the alliance is signed by b at t = 0 (both present values are expressed as real options).

It is noteworthy that, considering $P_T < \gamma V_T - (1-\alpha_T)\beta\delta V_T$, the solution of the sub-game at $t = T$ is always “no agreement signed” for any value assumed by V_T.[8] Therefore, backtracking to time t = 0, the first mover is confronted with the options of signing an alliance at t = 0 and allowing the second mover to make the decision. If the latter is chosen, the second mover is in turn confronted with two alternatives: choosing to sign an alliance at t = 0 or rejecting it, which would imply that both the first and the second movers will share the market size according to γ. It is also worth remembering that company z plays a purely passive role and for this reason has not been included in the experimental setting. Finally, during the game we used neutral terminology and avoided the terms “alliance” and “first/second mover.”

By making these additional assumptions and simplifications, the structure of payoffs going to both the first and the second mover is the same as that which is illustrated in Fig. 2[9]. This figure shows how each branch is named (H, K, X or Y) in the experiment’s instructions.

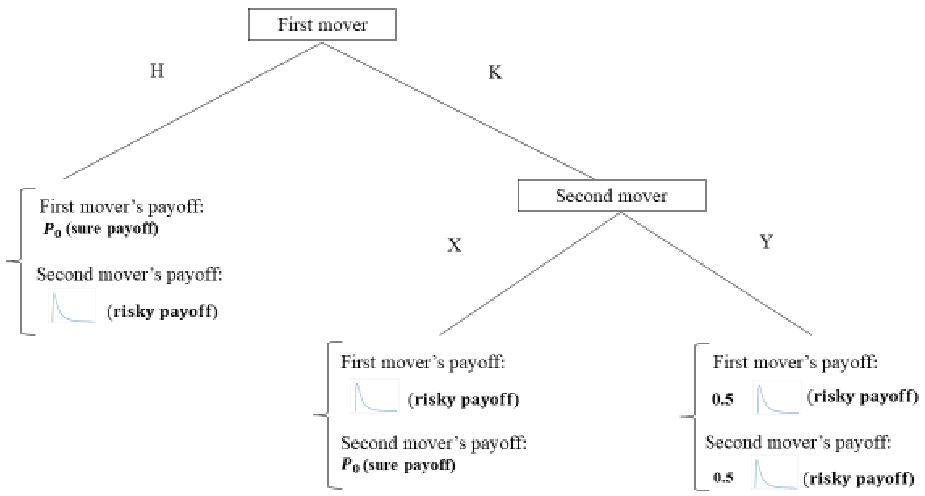

Fig. 2. Payoff structure in the laboratory.

As shown in Fig. 2, the first mover is provided with a sure outcome, P_0. If the sure payoff is accepted (implying the alliance is signed at t = 0; let this be referred to as alternative H), the second mover has a risky

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[7] See Lo Nigro et al. (2013) for a complete derivation of the possible scenarios of equilibrium.

[8] In fact, the NO alliance payoff is $\gamma V_T - I_T$, which is always greater than the alliance payoff $((1-\alpha_T)\beta\delta V_T + P_T - I_T)$, when $P_T < \gamma V_T - (1-\alpha_T)\beta\delta V_T$.

[9] We discounted the values of the distribution at t = 0 to make them comparable with the certain payoff P_0, computed at t = 0.

outcome, particularly in terms of a log-normal distribution, and this outcome has the expected value, computed at time = 0, equal to $E_0[V_T]$, which is higher than the sure outcome. Conversely, if the first mover rejects the sure outcome (alternative K), this mover will go alone to the market and this mover's payoff will be risky with an expected value depending on the decision made by the follower. If the second mover accepts the sure payoff (implying the alliance is signed at t = 0; let this be referred to as alternative X), the first mover has a risky outcome; with the expected value, computed at t = 0, equal to $E_0[V_T]$, which is higher than the sure outcome. Otherwise, if the follower rejects the sure outcome, both players share the risky outcome equally (alternative Y) and it is expected that they both will get a lower payoff than the sure outcome – that is, $0.5E_0[V_T] < P_0 < E_0[V_T]$. Consequently, in considering decision makers to be risk-neutral profit maximizers, the first mover should always reject the sure payoff and allow the follower to choose the same sure payoff.

Moreover, according to the theoretical model, which assumes that V_t follows a GBM, we present the risky outcome V_t in the format of a log-normal distribution. In Fig. 3, we truncated the distribution at a preselected value; specifically, the truncation was made at the value of the 99th percentile of the distribution.

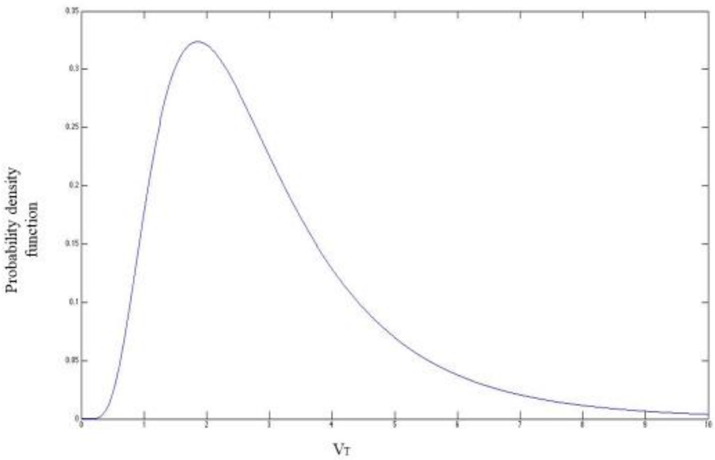

Fig. 3. Log-normal distribution of V_t truncated at the 99th percentile

In order to increase the subjects' understanding, we also provided them with a discrete distribution that approximates the precise payoff (see Table 2). We considered the 99 percentiles of the distribution (first column of Table 2) and the values assumed by the random variable *V* in correspondence to the percentiles (second column); for instance, considering the distribution in Fig. 3, the value of the 1st percentile is 0.68, while the value of the 17th percentile is 1.49. We provided participants with a ten-sided dice (numbered from 0 to 9). They had to roll this dice twice in order to produce a number between 00 and 99 (representing the 99 percentiles). For example, if the realization of the two rolls of the dice was 01 (i.e., the 1st percentile), the outcome would be 0.68. If the realization of the two rolls of the dice was 17 (i.e., the 17th percentile), the outcome would be 1.49. Otherwise, if the realization was between 01 and 17, then the outcome would be a random value between 0.68 and 1.49.

Table 2
Discrete approximate distribution of the log-normal distribution.

Percentiles	More precise payoff (V)	Approximate payoff (V)
00	0	0
01–17	0.68–1.49	1
18–48	1.52–2.48	2
49–71	2.52–3.49	3
72–84	3.55–4.48	4
85–91	4.59–5.46	5
92–95	5.66–6.48	6
96–97	6.88–7.41	7
98	8.17	8
99	9.53	10

In order to make our setting as simple as possible, we rounded the percentiles' values to the nearest integer number (third column), and we provided participants with only the first and the third columns of Table 2. For instance, they were told that in the event that their dice rolls yielded a number from 01 to 17 inclusively, then their outcome would be 1.

Note that the procedure adequately approximates the continuous distribution. For instance, from Table 1, the probability of getting 2 is 31% – that is, in the event that the dice numbered from 18 to 48 inclusively. According to Fig. 4, the probability that the random variable V is in the interval (2, 2+dV) is around 31%. This easily implementable procedure can be generalized to any continuous distribution (and not only to those that are log-normal).

4. The Experiment

This section describes the treatments implemented (4.1), the protocol for each treatment (4.2), and the participants and procedures (4.3).

4.1 Experimental design and task

Participants in the experiment complete two tasks: one main task and a Bomb Risk Elicitation Task (BRET; Crosetto & Filippin, 2013). The main task relies on four between-subjects treatments. In order to test the descriptive accuracy of the normative model, the main task was first designed and conducted using the investment problem described in Section 3.4 (two-person game). Then, we varied the main task in the following ways: i) substituting the second-mover participants with a random device (absence of strategic uncertainty) in the chance-based game, ii) providing information about followers' behaviour in the two-person game with info, and iii) eliminating the second participant (absence of both strategic uncertainty and other-regarding preferences effect) in the leader-risky game. We examine whether such changes affect the behaviour of first movers. In each of the four games, subjects repeated the decision task five times (rounds). The games are described below.

Two-person game

In the two-person game (TPG), half of the participants play the role of the first mover (or leader) and they are randomly paired with others who play the role of the second mover (or follower). First movers are confronted with two alternatives: H and K. H provides the first mover with a sure payoff and the paired follower with a risky outcome whose expected value is higher than the sure payoff. K provides both players with payoffs that depend on the behaviour of the second mover. The second mover is confronted by two alternatives: X and Y. X provides the second mover with a sure payoff and the paired leader with a risky outcome whose expected value is higher than the sure payoff. Y provides both players with the same risky outcome whose expected value is lower than the sure payoff (see Fig. 4). Moreover, and consistent with Bohnet and Zeckhauser (2004) and Aimone and Hauser (2012), in this treatment we ask the second movers whether they would choose the sure payoff if given the opportunity (strategy method). We use the followers' responses to determine the proportion of participants (p) who choose X and (1 – p) Y in each of the five rounds.

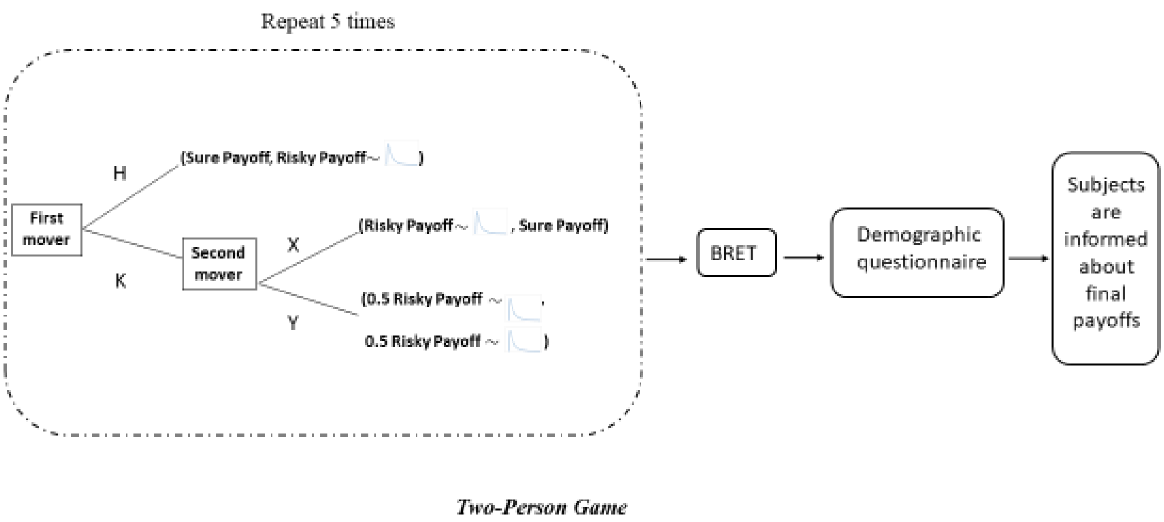

Fig. 4. Experimental tasks timeline in the TPG (in brackets the payoffs to the first mover and second mover respectively).

Chance-based game

In the chance-based game (CBG), risky choices are considered in the absence of strategic uncertainty. In this treatment, the payoffs are the same as in the TPG, but the treatment differs in that a random device, rather than the human follower, determines the payoffs going to both the first and second movers. Thus, the first mover's choice affects the follower's payoffs as well as the first mover's own. The follower simply makes no decision, which means that p and (1 – p) is computed in the first treatment to determine the "random device behaviour". In other words, if the first mover chooses K, in contrast to the TPG in which the follower makes choices, in this treatment alternative X is realized with probability p and alternative Y with probability (1 – p). In the experiment, any lotteries are resolved by drawing a ball from an urn with 100 balls numbered from 1 to 100, which represent percentages. If the selected ball is lower than p (in %), then the first mover receives the risky payoff while the second mover the sure outcome; otherwise, both players share the risky payoff equally (see Fig. 5). Participants are informed that the value of p was determined prior to the experiment and how. In addition, at the beginning of this game, the five pairs of values p and (1 – p) are shown in advance and in a randomized order to all the participants. However, they are not told which pair (p, [1 – p]) is related to that specific round.

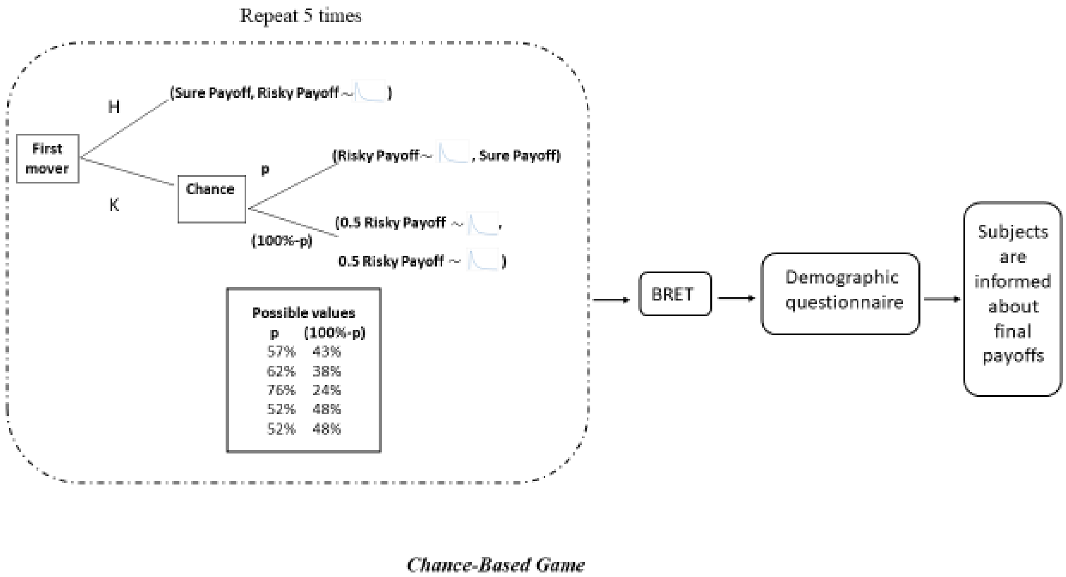

Fig. 5. Experimental tasks timeline in the CBG (in brackets the payoffs to the first mover and second mover respectively).

Two-person with info game

The two-person with info game (TPIG) differs from the TPG in that participants are informed of the behaviour of the second movers and from the CBG in the active role played by followers. In this treatment, the payoffs are the same as in the TPG, but participants are informed of the percentage of the second movers (p and [1 – p]) who chose alternative X and alternative Y in previous similar experiments. However, strategic uncertainty still exists since first movers are aware that the outcomes depend on their behaviour as well as on the choices made by the second movers in this treatment (see Fig. 6). Participants are also told that the value of p was determined prior to the experiment and how. As in the CBG, in the TPIG, before each round the five pairs of values p and (1 – p) are shown to all participants in advance and in a randomized order, but no information is provided regarding which pair is involved in that specific round.

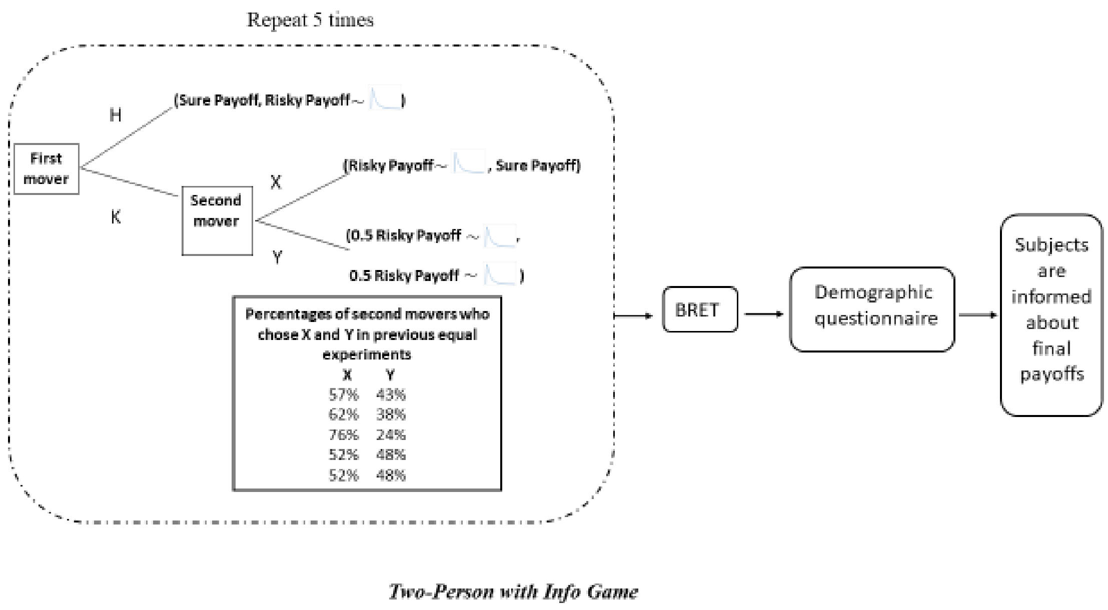

Fig. 6. Experimental tasks timeline in the TPIG (in brackets the payoffs to the first mover and second mover respectively).

Leader risky game

The leader risky game (LRG) differs from the CBG only in that there is no second mover involved – that is, when a leader makes choices there are no payoffs going to a follower (see Fig. 7).

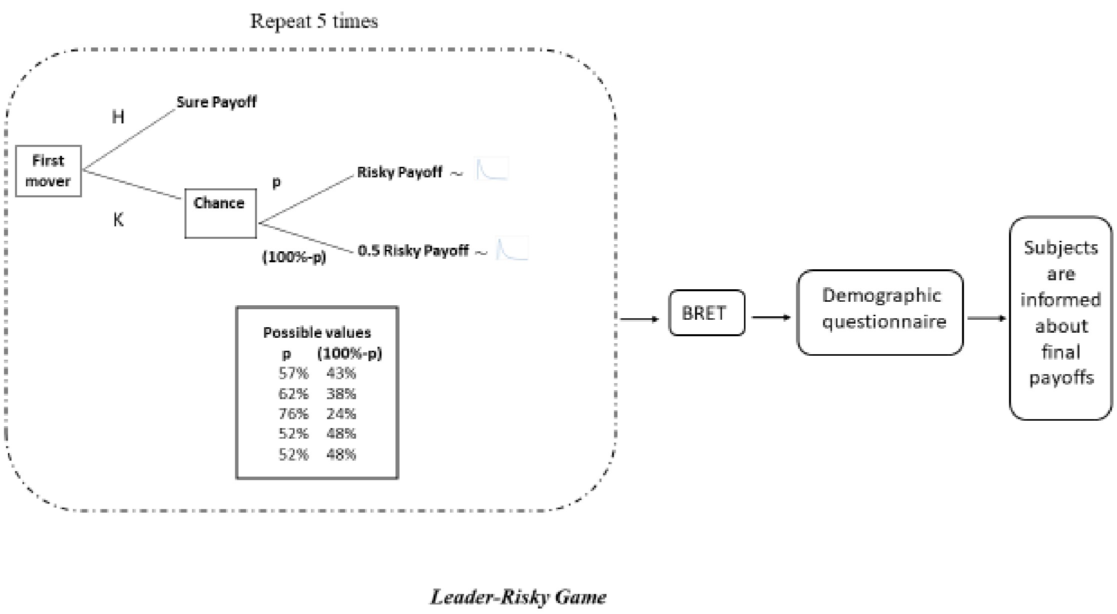

Fig. 7. Experimental tasks timeline in the leader-risky game.

During the second and final part of the experiment, we elicited the subjects' risk preferences through the BRET. In this task, subjects are shown 100 boxes and informed that 99 boxes contain €0.03 each, while the remaining one contains a bomb that will explode and nullify the earnings for the second part of the experiment. Each subject is then asked to collect as many boxes as they like. The boxes are then opened. If the box with the bomb has not been selected, the subject's earnings depend on the number of collected boxes; but if the bomb is among the boxes, the earnings are equal to zero.

4.2 The protocol

During each treatment, subjects repeat the decision task five times with a different value of P_0 and E_0 (V_T). Table 3 shows the value of P_0 and the expected value of the risky outcome E_0 (V_T) used in the five rounds. In each round, the subjects were presented with the log-normal distribution and the discrete one as information about the risky outcome. To avoid creating a particular order of importance, instead of using numbers to identify the distributions, we used the names of Italian cities (Napoli, Roma, Palermo, Milano and Torino). Additionally, to enhance the inter-item comparability of the respondents' answers to the rounds, we set the following parameters: $r = 0.05$; $\sigma = 0.40$; $T = 2$; and E_0 (V_T) expressed in points varying between 300 and 700. It is important to emphasize that given our input parameters, the five decision tasks are identical and only the values vary, but there is no change in the form of the log-normal distribution. Indeed, the geometric Brownian motion presents the same standard deviation, which is 0.40 in each round. Moreover, according to the particular path of the tree chosen, the value of P_0 ranges between 0.5 E_0 (V_T) and E_0 (V_T). Again, in order to enhance the inter-item comparability of the respondents' answers to the rounds, in each round we chose the 40th approximated percentile of the corresponding log-normal distribution (note that this value respects the condition 0.5 E_0 (V_T) < P_0< E_0 (V_T); see Table 3). Thus, all the items can be considered different representations of the same decision problem. Finally, to check for order effects, the distributions were presented to the participants in random order.

Table 3
Values of P_0 and E_0 (V_T).

P_0 values according to the theoretical model	**Used values at approximately the 40th percentile (sure outcomes)**	**Expected value of the log-normal distribution (risky outcomes)**	**Distributions**
$150 < P_0 < 300$	$P_0 = 200$	$E_0 (V_T) = 300$	Napoli
$200 < P_0 < 400$	$P_0 = 300$	$E_0 (V_T) = 400$	Roma
$250 < P_0 < 500$	$P_0 = 400$	$E_0 (V_T) = 500$	Palermo
$300 < P_0 < 600$	$P_0 = 400$	$E_0 (V_T) = 600$	Milano
$350 < P_0 < 700$	$P_0 = 500$	$E_0 (V_T) = 700$	Torino

The value of p used in the TPIG and CBG during each round was established by the proportion of followers who chose alternative X in the first two sessions of the TPG. Specifically, 12 out of 21 chose X ($p = 0.5714$) in Napoli; 13 out of 21 chose X ($p = 0.6190$) in Roma; 16 out of 21 chose X ($p = 0.7619$) in Palermo; 11 out of 21 chose X ($p = 0.5238$) in Torino, and 11 out of 21 chose X ($p = 0.5238$) in Milano. In practice, we set p (Napoli) = 0.57; p (Roma) = 0.62; p (Palermo) = 0.76; p (Torino) = 0.52; and p (Milano) = 0.52. As indicated previously, these values were presented to the participants before playing the five tasks and in random order. They were not told that a specific pair of values corresponded to a specific distribution.

Moreover, during the experiment we named the sure payoff and the risky payoff as PC and V respectively (see Appendix A for the instructions provided during the game).

4.3 Participants and procedures

Our experiment was conducted at the Cognitive and Experimental Economics Laboratory (CEEL) at the University of Trento. We ran the TPG in three sessions, the TPIG game in three sessions, the CBG in three sessions, and the LRG in two sessions. Participants were undergraduate and graduate students at the University of Trento who were recruited via a customized software developed at CEEL. In total, 198 people participated (97 females and 99 males):[10] 27 pairs in the TPG, 29 pairs in the TPIG, 28 pairs in CBG, and 30 individuals in the LRG. The average age was 23.06 (s.d. = 2.693), and most of the participants were students of economics (55.61%) and the others were students of law (16.84%), the social sciences (12.75%), engineering (6.12%), the humanities (6.12%), or mathematics and other natural sciences (2.55%).

Upon arrival at the laboratory, participants were randomly assigned to a computer. It was common knowledge that the experiment was composed of two tasks. Specifically, subjects were told that they would participate in the second part of the experiment, but none of them was informed in advance about the purpose of the second part. First, each participant visualized instructions for the first part of the experiment, which were first read by the subjects individually. Then, the instructions were read aloud by one of the researchers. After the completion of the first part, the instructions for the second part were distributed and read aloud. Following the two parts of the experiment, subjects were asked to fill out a brief demographic questionnaire.

The experiment used a between-subjects design, where each subject participated in one treatment only. In all the treatments, except for the LRG, participants were randomly assigned a role and randomly matched with a counterpart. Specifically, they were told that they would maintain their role throughout the rounds, but the counterpart would change randomly in each round. In the LRG, all subjects were leaders.

Subjects received 3 Euros as a show-up fee, plus a sum of experimental currency units (ECU) that varied according to the choices made in the first and second parts of the experiment; the ECUs were converted to Euros (100 ECU = 1 Euros) at the end of the experiment. During the first part of the experiment, participants were aware that a random draw at the end would determine which of the five distributions would be applied to calculate the payment. The average payment including the participation fee was €7.80, with a maximum of €20.50 and a minimum of € 3 for an experiment that took approximately 40–50 minutes.

5. Hypotheses

The first game – that is, the TPG – was designed and conducted using the ROG problem described in Section 3.4. According to our theoretical model, which assumes decision makers to be risk-neutral profit maximizers, the first mover should always prefer alternative K and reject the sure payoff of alternative H. This reasoning allows us to make the following theory-based hypothesis:

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[10] Two participants did not declare their gender in the questionnaire.

***Hypothesis 1**: If players are risk-neutral profit maximizers, in the TPG first movers will choose alternative K.*

This hypothesis assumes the (full) rationality of subjects, even though the literature on decision-making behaviour has long identified behaviours that are not aligned with fully rational decision-making. In this vein, we argue that when making their choices first movers are influenced by strategic uncertainty. Indeed, in the TPG, if the first mover rejects the sure payoff, then a second human agent makes decisions (strategic uncertainty is present).

A growing body of literature on trust games, which share the same leader–follower structure proposed in our model, has observed that leaders are more prone to reject the sure payoff and take the opportunity to have a potentially higher payoff when chance (i.e., a random device), instead of a human being, determines outcomes (Bohnet et al. 2008; Bohnet & Zeckhauser 2004; Hong & Bohnet 2007). Following this stream of research, we would expect a lower number of leaders to accept the sure payoff (alternative H) in the CBG than in the TPG. However, in the setting used in these experiments, payoffs are deterministic, while in our setting payoffs are stochastic. Moreover, a role could be also played by so-called ambiguity-aversion bias, which refers to the preferences towards choosing events with known probabilities for potential outcomes (risky events) over those with unknown probabilities (uncertain events). Daniel Ellsberg (1961) originally studied ambiguity aversion in a well-known study – the Ellsberg paradox experiment – providing evidence that the participants systematically preferred the risky gamble, avoiding the ambiguous one.

In our experiment, first movers have to choose between a sure outcome (alternative H) and a lottery (alternative K) that could yield either a higher or a lower risky payoff (i.e., in terms of expected value) than the sure outcome. Depending on the way in which alternative K is framed, our treatments can be considered more or less ambiguous. In fact, alternative K is designed to be an uncertain situation (TPG) or a risky situation (CBG). According to ambiguity aversion, we expect that, other things being equal (in particular assuming equal distributions of risk attitudes across samples), a higher percentage of participants should choose alternative K (or a lower percentage of participants should choose alternative H) in the CBG (lower ambiguity) rather than in the TPG (higher ambiguity).

Combining these two lines of argument, we derive the following behavioural hypothesis:

***Hypothesis 2**: The number of first movers choosing alternative H will be lower in CBG than in TPG.*

Betrayal aversion has been recognized as the main factor contributing to differences between these two treatments in previous similar settings using deterministic outcomes (Aimone & Hauser, 2012, Bohnet et al., 2008; Bohnet & Zeckhauser, 2004; Hong & Bohnet, 2007). However, as has been acknowledged in these studies, other factors such as disutility from loss of control and other-regarding preferences may drive this outcome.

The introduction of risky outcomes in our experiment profoundly transforms the structure of strategic interaction and allows for the disentangling of the betrayal aversion mechanism by breaking the sure payoffs structure that links the first mover to the second one. More exactly, the room for the rise of some form of betrayal aversion is completely absent. As anticipated, the only way left to the second mover to deviate from the behaviour expected by the first mover is to assume the cost of an inefficient choice. It is useful to remember that this type of behaviour moves us in the field of the study of a psychological mechanisms alternative to betrayal aversion. Specifically, our study introduces an alternative explanation to betrayal aversion based on the hypothesis that a factor involving disutility from loss of control is actually triggering the different choices observed when the first mover must decide when is playing with a human (TPG) or with a random device (CBG).

In order to verify the robustness of this argument, we need to rule out alternative explanations for this difference. One consideration regards the role played by the probabilities used in the CBG. Recall that these probabilities are calibrated using the frequencies of the second movers' choices, as reported in the TPG sessions previously conducted. In the CBG, subjects are informed about this procedure, which means that they could have decided what to do by aligning their expectations with the data provided. With this aim, we designed the TPIG, which differs from the TPG only in that we inform participants of the behaviour of the second movers in otherwise identical experiments. If statistically significant differences between choices in the TPG and in the TPIG are not observed, we can conclude that knowledge of the probabilities used in the CBG does not matter.

It could be argued, however, that also other-regarding concerns could play a role in determining that a lower number of first movers choose alternative H in the CBG than in the TPG treatment. To test this proposition, we designed the LRG, which differs from the CBG in that it provides no payoffs to the follower. In the CBG, first movers may care about the payoffs going to their counterparts, such that other-regarding concerns may be at work. These other-regarding preferences could affect first movers' decisions (Fehr & Schmidt, 2004) in choosing alternative H or alternative K. Following the approach proposed by Bohnet et al. (2008), we have compared first movers' choices in the CBG and LRG, and this provides us with a measure of other-regarding preferences. If a statistically significant difference between choices in the CBG and the LRG is not observed, we can conclude that other-regarding concerns do not play a role. Based on these considerations, we derive our final behavioural hypothesis:

Hypothesis 3*: The lower number of first movers choosing alternative H in the CBG than in TPG is due to a psychological factor involving disutility from loss of control only if we do not observe a statistically significant difference between choices in the TPG* and the *TPIG and between choices in the CBG and the LRG.*

6. Results and discussion

In this section, we present the findings of our experiment. As already indicated, we focus on the behaviour of first movers. First, we analyse our results across treatments (subsection 6.1). Then we aggregate our data and analyse the impact of several determinants on the behaviour of first movers with multivariate analysis (subsection 6.2). Finally, in section 6.3, we employ a bootstrap analysis to examine whether our results are robust.

6.1 Main results across treatments

We first analyse subjects' risk preferences across treatments using the BRET. Table 4 shows the average number of boxes collected in each treatment.

Table 4
Boxes collected in each treatment.

TPG	**TPIG**	**CBG**	**LRG**
47	41	47	42

As shown in Table 5, the subjects do not show significant differences in terms of risk preferences across treatments. Consequently, if differences in subjects' responses exist across treatments, these differences were not triggered by non-homogenous risk attitudes distributions.

Table 5
Comparison of average boxes collected across treatments (p values computed with a Mann–Whitney U test)

$47_{(TPG)} \leftrightarrow 41_{(TPIG)}$ p value = 0.2214	$47_{(TPG)} \leftrightarrow 47_{(CBG)}$ p value = 0.9328	$47_{(TPG)} \leftrightarrow 42_{(LRG)}$ p value = 0.2064
$41_{(TPIG)} \leftrightarrow 47_{(CBG)}$ p value = 0.2214	$41_{(TPIG)} \leftrightarrow 42_{(LRG)}$ p value = 0.8974	$47_{(CBG)} \leftrightarrow 42_{(LRG)}$ p value = 0.2155

We now present the results of non-parametric tests. Table 6 reports the percentage of first movers who chose the sure outcome (alternative H) in each treatment.[11] We have 27 independent observations and 135 decisions for the TPG, 29 independent observations and 145 decisions for the TPIG, 28 independent observations and 140 decisions for the CBG, and 30 independent observations 150 decisions for the LRG.

Table 6
Percentage of first movers who chose alternative H (sure outcome) in each treatment.

TPG (N = 135)	**TPIG** (N = 145)	**CBG** (N = 140)	**LRG** (N=150)
42.96%	37.93%	29.28%	30.00%

The aim of this analysis is to determine whether subjects made decisions consistent with the normative solution of our theoretical model. We have argued that the TPG precisely represents the ROG problem of the model. Assuming decision makers to be risk-neutral profit maximizers, the first mover should always reject the sure payoff of alternative H. However, our first experiment found that about 57% of investment decisions corresponded with this prediction.

Result 1*: In the TPG, nearly 43% of subjects chose alternative H, thus exhibiting behaviour that departs from fully rational decision-making.*

Guided by the behavioural literature, we argue that this result is due to the human interaction in the TPG, and in the absence of such human interaction subjects should be more prone to reject the sure payoff

[11] We focus on the behaviour of first movers. For the sake of completeness, we also report the percentage of second movers who chose alternative X in the TPG (62.22%, N = 135) and TPIG (64.82%, N = 145).

(alternative H) in order to receive a potentially higher payoff. In the CBG, which differs from the TPG only in that there is no human interaction, the percentage of subjects choosing alternative H is significantly lower than it is in the TPG treatment condition (two-sided Fisher's exact p = 0.024).[12] Due to the design of our study, this difference must be attributed to the absence of human interaction in the CBG. The statistically significant difference between the numbers of H choices in the two treatments supports our second hypothesis.

Result 2*: Significantly fewer first movers chose alternative H in the CBG than in the TPG.*

Our third hypothesis is that if we observed a dissimilar composition of the first movers' choice between the two treatments, this difference is due to disutility from loss of control. However, this hypothesis is premised on the assumption that the role of information and other regarding preferences are not at work. By comparing choices in the TPG and the TPIG, we found that information about the behaviour of second movers in these otherwise identical experiments does not matter. Indeed, there is no statistically significant difference between the numbers of H choices selected in these two treatments (two-sided Fisher's exact p = 0.397).[13]

Result 3a: *The number of first movers who chose alternative H in the TPIG is not statistically different from the number of first movers who chose the same alternative in the TPG.*

Moreover, we observe there is no statistically significant difference between the number of H choices selected in the CBG and the LRG (two-sided Fisher's exact p = 0.898).[14] Therefore, other regarding preferences do not play a role.

Result 3b: *The number of first movers choosing alternative H in the CBG is not statistically different from the number of first movers choosing the same alternative in the LRG.*

Taken together, these results confirm our last hypothesis:

Result 3: *The lower number of first movers choosing alternative H in the CBG than in TPG is due to a psychological factor involving disutility from loss of control.*

To investigate these results in detail, we conducted two logit-regression analyses (see Table 7). A Logit Model was adopted due to the nature of our dependent variable (1 = if the first mover chooses alternative H, 0 = if the first mover chooses alternative K), and we introduce random effects to check for repeated decisions.

Specifically two models were used, namely Model 1 and Model 2. In Model 1, we wanted to compare first movers' choices in the TPG (which represents the baseline category) with the same choices in the TPIG and the CBG. In model 2, we wanted to compare first movers' choices in the CBG (which in this model represents the baseline category) with the same choices in the LRG.

[12] The result is robust to a Pearson Test (p = 0.018).
[13] The result is robust to a Pearson Test (p = 0.391).
[14] The result is robust to a Pearson Test (p = 0.894).

Table 7
Regression estimates. First movers' choices among treatments

	Model 1 (baseline category: TPG)	**Model 2** (baseline category: CBG)
TPIG	–0.218	
	(0.278)	
CBG	–0.626**	
	(0.289)	
LRG		0.375
		(0.305)
Constant	–0.297	–0.945***
	(0.199)	(0.226)
Observations	420	290

Note: This is a random effects logit model, where the dependent variable (choice) takes value 1 if the first mover chooses alternative H and zero otherwise.
* significant at 10% ** significant at 5% *** significant at 1%

The regression results exactly reproduce the findings of our nonparametric tests. Indeed, significantly fewer first movers chose alternative H in the CBG than in the TPG (Result 2). Conversely, there is no statistically significant difference between the number of first movers who chose alternative H in the TPG and the TPIG (Result 3a) and between the number of first movers who chose alternative H in the CBG and the LRG (Result 3b).

6.2 Multivariate analysis

In order to analyse the effect of several factors on first movers' choices, we ran multivariate logit regressions on the entire sample of data (N = 570, that is, 5 choices of 114 first movers). The dependent variable was the first movers' choices across treatments. The independent variables were the four decision scenarios (*TPG, TPGI, CBG,* and *LRG*), the five distributions submitted to the participants (*Napoli*, *Roma*, *Palermo*, *Milano*, and *Torino*), *BRET*, *Age* (in years), a dummy variable for those studying economics (*Economic Concentration*), and a dummy variable for female subjects (*Women*). The *TPG* scenario, *Napoli*, and *Men* are our baseline categories (see Table 8). We decided to test a model including *Women* because there are literature evidences that show that female subjects are on average more financially risk averse (Charness & Gneezy, 2012), and less trustworthy (Alesina & La Ferrara, 2002) than males. The variables *Economic Concentration* and *Age* have been included to the sixth model to control for possible idiosyncratic effects triggered by broad demographic differences among the participants. Finally, BRET has been included to monitor for the impact induced on the observed choices by risk attitudes.

Table 8
Determinants of alternative H (sure outcome).

	(3)	**(4)**	**(5)**	**(6)**

TPIG	-0.219		-0.241	-0.371
	(0.279)		(0.308)	(0.318)
CBG	-0.628**		-0.689**	-0.700**
	(0.291)		(0.319)	(0.321)
LRG	-0.592**		-0.649**	-0.684**
	(0.285)		(0.313)	(0.337)
Roma		0.588*	0.588*	0.608*
		(0.331)	(0.331)	(0.337)
Palermo		1.696***	1.697***	1.774***
		(0.325)	(0.325)	(0.33)
Milano		0.771**	0.771**	0.84**
		(0.327)	(0.327)	(0.332)
Torino		1.387***	1.387***	1.421***
		(0.323)	(0.323)	(0.328)
BRET				-0.013**
				(0.007)
Women				0.174
				(0.241)
Age				0.0192
				(0.453)
Economic Concentration				-0.766
				(0.243)
Constant	-0.298	-1.619***	-1.218***	-1.139
	(0.200)	(0.265)	(0.319)	(1.19)
Observations	570	570	570	565

Note: This is a random effects logit model, where the dependent variable (choice) takes value 1 if the first mover chooses alternative H and zero otherwise.
* significant at 10% ** significant at 5% *** significant at 1%

As Model 3 in Table 8 indicates, the CBG and the LRG are statistically different than the TPG and lead to significantly fewer sure H choices than the TPG. These findings support our hypothesis about the role played by the presence or absence of an “active” second player. Conversely, the TPG and the TPIG are not statistically different. This result confirms, all else being equal, the conjecture about the absence of a significant effect induced on the first movers’ choices by the provision of “statistical” information about the behaviour of second movers.

In Model 4, we control for the distributions. Note that the coefficients of the distributions (*Roma, Palermo*, *Milano*, and *Torino)* are significant and positively correlated with the sure alternative choice. Recall that the values of the risky choices in the distributions gradually increase (from *Napoli* to *Torino*), as do the value of the sure outcome alternative, so these results provide further support for the interpretation that the participants tended to choose alternative H (the sure outcome) more frequently when the distribution offered higher sure payoff, independent from the values of the risky payoffs. This finding implies that people are sensitive to the relationship between the values of the sure and the risky payoffs.

In Model 5, we control for both treatments and the distributions, and it is apparent that there are no changes in the significance or the sign of the coefficients.

Finally, in Model 6[15] we control for all the variables already used in the previous models, including *BRET*, *Age*, *Economic Concentration*, and *Women*. We notice that the *BRET* coefficient is negatively, and significantly, correlated with the sure choice alternative. Given that higher values of *BRET* are associated with a higher degree of risk taking, it is intuitive to expect a negative correlation between the *BRET* score and the number of sure alternative choices. In other words, we have confirmation that the greater the risk aversion, the higher the number of sure outcome alternatives chosen by the participants. It should be noted that this result can be interpreted as an indirect confirmation that the participants did not make their decisions randomly and that they were driven by concerns about the relationship between outcomes and probabilities. On the other hand, it is important to stress that the coefficient of the *CBG* and *LRG* are again negative and statistically significant. In other words, the introduction of a measure of risk propensity does not change the role and the sign of *CBG* and *LRG* in explaining our dependent variable. The coefficients of the variables *Age*, *Women* and *Economic Concentration* are not statistically significant; therefore, they do not contribute to the explanation of our dependent variable.

6.3 Bootstrap analysis

In order to determine whether our results differ from our sample statistics, we employed bootstrap methods. From our complete sample, we drew 1,326 random samples of the same size as our observed sample for each subject in each treatment, [16] and then we re-ran regressions models 1 and 2 to check the robustness of the main results across treatments (Table 9), and models 3 to 6 to get the multivariate analysis (Table 10).

Table 9
Bootstrap analysis of regression estimates.

	Model 1 (baseline category: TPG)	**Model 2** (baseline category: CBG)
TPIG	–0.218 (0.305)	
CBG	–0.627** (0.299)	
LRG		0.375 (0.309)

[15] Note that in total 114 participants played the role of first mover; one of them, however, did not declare their gender in the questionnaire, so there are 565 observations in Model 4 rather than 570.

[16] This number has been computed using the “bsize” command provided as an added package in Stata software. For an overview of this command see Andrews and Buchinsky (2000).

Constant	–0.297	–0.945***
	(0.218)	(0.250)

Note: This is a random effects logit model, where the dependent variable (choice) takes value 1 if the first mover chooses alternative H and zero otherwise.
* significant at 10% ** significant at 5% *** significant at 1%

Table 10
Bootstrap analysis of the determinants of alternative H (sure outcome).

	(3)	**(4)**	**(5)**	**(6)**
TPIG	-0.219		-0.241	-0.371
	(0.300)		(0.324)	(0.349)
CBG	-0.628*		-0.689**	-0.700**
	(0.336)		(0.356)	(0.341)
LRG	-0.592**		-0.649**	-0.684**
	(0.297)		(0.313)	(0.381)
Roma		0.588	0.588	0.608
		(0.409)	(0.418)	(0.432)
Palermo		1.697***	1.697***	1.774***
		(0.439)	(0.448)	(0.448)
Milano		0.771**	0.771*	0.84*
		(0.392)	(0.413)	(0.441)
Torino		1.387***	1.387***	1.421***
		(0.413)	(0.419)	(0.449)
BRET				-0.013*
				(0.008)
Women				0.174
				(0.266)
Age				0.0192
				(0.049)
Economic Concentration				-0.766
				(0.256)
Constant	-0.298	-1.619***	-1.218***	-1.139
	(0.220)	(0.334)	(0.399)	(1.31)

Note: This is a random effects logit model, where the dependent variable (choice) takes value 1 if the first mover chooses alternative H and zero otherwise.
* significant at 10% ** significant at 5% *** significant at 1%

The findings in Tables 9 and 10 confirm the results observed and discussed in the previous sections, and consequently we can conclude that our results are robust. The only exception is the coefficient of the distribution *Roma*, which is no longer statistically significant, while the coefficients of *Palermo*, *Milano*, and *Torino* are still significant and positively correlated with the sure alternative choice (Models 4, 5, and 6). The lack of statistical significance of the distribution *Roma* is due to the fact that the values of the sure choice and of the expected payoff of this distribution are closest to those of the distribution *Napoli*, which is the baseline variable used for the comparison. Therefore, this analysis provides additional support that alternative H is more frequently chosen when the sure payoffs are higher – that is, when participants are playing the distributions *Palermo, Milano*, and *Torino***.**

7. Conclusions

The number of game and real options theory-based frameworks has grown over the last two decades. However, without experimental studies, it is difficult to appreciate the value added by the ROG approach. Moreover, it is even more difficult to assess the contributions of new theoretical contributions (Azevedo & Paxson, 2014).

This study demonstrates the importance of empirically testing the available theoretical models. Accordingly, the first goal of this paper was to investigate decision-making processes in a ROG setting that allows human behaviour to be investigated in a controlled laboratory environment while simultaneously preserving the main characteristics of the decisional problem embodied in the theoretical model (TPG). Our experimental results show that subjects do not behave in a rational way – that is, according to the normative policy derived from the theoretical model under study. Specifically, guided by the behavioural literature that focuses on two-person trust games, our findings highlight that decision makers seem reluctant to behave as the normative model suggests because of the presence of strategic uncertainty – that is, when the source of uncertainty is a human subject. These results suggest that participants experience a psychological cost when interacting with another human being. Given our empirical results, the theoretical model we have experimentally tested needs to be revisited. Specifically, the model must take into account the behaviour of decision makers who, instead of maximizing their monetary payoff, maximize a utility function that, in addition of the monetary payoff, includes this psychological cost. In other words, this behavioural regularity can be conceived as disutility from loss of control.

In addition, we add to the behavioural literature in that our approach introduces a crucial difference with respect to the behavioural literature on trust games and betrayal aversion mainly because we use probabilistic rather than deterministic payoffs. This difference is of crucial importance because it means that first movers must make decisions trusting on the perfect rationality of the second player, removing at the same time the influence of betrayal aversion, because the choice of the second mover cannot be intentionally aimed to damage the first mover. Our main finding that first movers are more likely to choose to pass the choice to the second movers when a random device, rather than a human subject, determines their final outcomes is thus due to disutility from loss of control instead than to betrayal aversion.

Several additional directions can be taken to build upon this work. First, the theoretical model we have investigated offers several scenarios of equilibrium solutions, depending on the values assumed by the model's input parameters. For the purposes of this study, we have considered only one possible scenario of equilibrium, and further research could extend the empirical analysis to other possible solutions.

Second, for the sake of parsimony, our analysis focuses on the behaviour of first movers. It would be interesting to extend the analysis to the behaviour of second movers. For instance, a non-trivial number of second movers chose alternative Y instead of alternative X, as the theoretical model predicts, and this finding implies that both players equally share the risky payoff. Thus, it could be that their choices are in some way influenced by other-regarding concern, and further research could examine the role played by concern for others, including the so-called "inequity aversion" (Fehr & Schmidt, 1999).

Finally, Morreale et al. (2017) theoretically extended the model we have utilized (Lo Nigro et al., 2013) in order to study the optimal alliance timing in a setting where firms compete to sign a research and development agreement with a third company. They significantly depart from the original approach since they incorporate the active role of the third company by considering the contract terms – that is, payments offered to the two competing firms – as endogenous decisions. In our experimental setting, the third company is a passive player, but it would be useful to experimentally analyse the more complete setting offered by Morreale et al. (2017) – that is, a setting that simultaneously encompasses the role of flexibility (implied by the use of real options in the presence of fundamental uncertainty), the role of competition and the roles of the three active players.

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