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Professionals and students in a lobbying experiment Professional rules of conduct and subject surrogacy

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Abstract

Lobbying is studied in a series of signaling game experiments. Students as well as professional lobbyists are used as subjects. In contrast with some earlier studies, comparing students and professionals, we find significant differences in the behavior of the two subject pools. Professional subjects appear to behave more in line with the game–theoretic predictions, display a higher degree of separation, and earn more money. We show that professional rules of conduct and professionalization can explain these differences. Although our results suggest that subject surrogacy is a relevant issue in this field of research, arguments are provided why experimentation with student subjects remains useful to study lobbying. © 2000 Elsevier Science B.V. All rights reserved.

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1. Introduction

Lobbying has become a growth industry in many countries. For example, for the US, there are estimates listing as many as 100,000 persons that are now involved in 'legislative advocacy' nationwide. [1] In Brussels, more than 10,000 are estimated to be involved in interest representation, and the number of lobbyists in individual European countries is rapidly growing. [2] The new profession of 'political manager' or 'public affairs consultant'

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[1] See National Lobbyist Directory, http://www.lobbyist.net.

[2] See, e.g. The Economist, 'The Brussels lobbyist and the struggle for ear time', 15 August 1998, and 'Time for reform', 24 September 1994 (speaking of an 'explosive growth' in the UK).

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is on its way, with its own associations, registers, standards of conduct, and educational training.[3] At the same time, the potential inroad of lobbying on representative democracy has become a regular issue in the media.

The strategic transmission of information, whereby a lobbyist is interested in persuading a policymaker to choose a particular type of action, is generally considered to be at the heart of lobbying (see, e.g. Potters and van Winden, 1992; Strauch, 1993; Austen-Smith, 1994). Although by now there is a large general literature on lobbying and related issues,[4] there exists only a small number of rigorous theoretical studies of the information transmission game between lobbyists and policymakers (for a review, see Austen-Smith, 1997), and hardly any empirical tests thereof. The latter is not so surprising given the obvious difficulty of obtaining adequate data (cf. Potters and Sloof, 1996). In this respect, laboratory experimentation seems to offer an interesting alternative method of research in this important area.

In Potters and van Winden (1996), we used this method to investigate the behavior of student subjects in a signaling game experiment designed to capture the basic features of lobbying. In the setup chosen for this experiment, the sender (lobbyist) has private information on the realization of some stochastic state variable which affects the payoffs that the sender and the responder (policymaker) can get. Actual payoffs are determined by the choice the responder has to make, given the value of the state variable. On the basis of her prior incomplete information about this variable, the responder is inclined to take a decision which is unfavorable for the sender. However, the sender can opt to send a (costly) message to the responder in an attempt to change the responder's beliefs about the state variable, so that the responder will take a decision which is preferred by the sender. As Section 2 will show, game theory provides sharp predictions for the outcome of this game. Our experimental results clearly deviated from these predictions.

Although it is common in experimental economics to use students as subjects, in this case a legitimate point of critique could be that, in order to learn something about lobbying, one should use professionals as subjects. First, because students may not be familiar with the intricacies of, and have no training in, the strategic transmission of information. The experimental game described above is a relatively complex one. It involves incomplete information, interaction, a relatively large strategy set, and the absence of dominated strategies. Since public affairs officials may be expected to feel more familiar with such an environment, which mimics some basic features of their 'natural habitat', it provides an opportunity to them to behave differently. Experimental evidence suggests that more complex situations are indeed necessary for professionals to show different behavior (see Abdolmohammadi and Wright, 1987). Second, and perhaps more importantly, in reality professional rules of conduct may play a significant role. The latter point links up with the growing interest in economics for the relevance of conventions and norms. Indeed, in publications distributed by professional lobbyists and their associations, standards of conduct are frequently mentioned, as will be further discussed below.

[3] See, e.g. American Society for Association Executives (http://www.asaenet.org/PublicPolicy), and The Graduate School of Political Management of The George Washington University (http://www.gwu.edu/∼gspm).

[4] See, for instance, the literature mentioned under the course requirements of The Graduate School of Political Management.

The main goals of this paper are to investigate whether indeed professional rules of conduct have an impact and whether there are any significant differences in the behavior of professionals and students. To that purpose we replicated the signaling game experiment, using experienced public affairs officials (and additional students) as subjects.

Although in general it may not be easy to find out what the rules of conduct are in a particular profession, in our case we are helped by explicit statements in professional publications. [5] We only reproduce the rules that are of potential relevance for our study. In this respect, an important rule — present in all the publications referred to in footnote 5 — holds that the lobbyist should 'never cheat or misinform'. A related rule frequently mentioned is that one should 'avoid conflicts of interest', inducing a focus on 'win-win' situations. To indicate the relevance of these rules for the signaling game studied in this paper it suffices to note here that this game exhibits a dilemma structure: the equilibrium outcome of the game is payoff dominated by a 'cooperative' (non-equilibrium) outcome. In the equilibrium outcome, the responder is sometimes misinformed by the sender, and the responder's choices accommodate to that fact. In terms of expected payoffs, this equilibrium outcome is worse for both the sender and the responder than the 'cooperative' outcome. In case of the latter, a sender reveals his private information to the responder, enabling the responder always to make the choice which is best for her. This outcome is only attainable, however, if the sender can somehow resist the temptation to cheat. Another potentially important rule of conduct put forward in this literature is that lobbyists should 'closely monitor the feedback from policymakers', [6] since this rule could lead to a different form of behavioral adjustment over time by the professionals in the experiment.

Finally, we note here that our second goal — searching for differences between student and professional subjects — is also relevant for the nagging issue of 'subject surrogacy', which is often raised by those who are sceptical about the results of experimental inquiry in the social sciences. Experimentalists usually rebut the argument by pointing out that it is not a criticism of the experimental method per se, but a plea for 'selective replications' with professionals from the field (Plott, 1982; Kinder and Palfrey, 1993). Up till now, remarkably few differences between students and professionals have been documented in the experimental literature (see, e.g. Davis and Holt, 1993, p. 17). However, this evidence shows a number of limitations. First of all, it is based on relatively few studies, with small numbers of professionals. [7] Secondly, in contrast with standard procedures for student subjects, subjects in these studies are sometimes not paid at all (e.g. Burns, 1985), or not in the usual fashion (e.g. DeJong et al., 1988). Moreover, many of these studies involve markets, which do not represent situations most likely to elicit differences in subject-pools (Roth et al., 1991). On the basis of an extensive survey Ball and Cech (1996, p. 266)

[5] See, e.g. the 'Guidelines for Association Lobbyists' and 'Standards of Conduct' of the American Society of Association Executives (http://www.asaenet.org/PublicPolicy), the 'Lobbyist Ethics Pledge' of the Council for Ethics in Legislative Advocacy, to become a certified lobbyist (http://www.lobbyist.net), 'Some Lobbying Tactics' by long time Australian-based political campaigner Aldis Ozols (http://www.zeta.au/∼aldis). See also Schlozman and Tierney (1986) and Strauch (1993).

[6] Strauch (1993, p. 45) refers to this rule as one of the 'methods of lobbying' (Genaue Beachtung des Feedback von den Entscheidungsträgern).

[7] For instance, there are only nine professionals in Burns (1985), seven in DeJong et al. (1988), and 8–10 in Dyer et al. (1989).

conclude: "The combined evidence (. . .) seems to suggest that, in environments where probability and risk play a major role, research using students subjects has the potential to produce anomalous results and efforts should be made to replicate results using subjects from more representative populations."[8] For all these reasons, replications with professionals, particularly concerning institutional settings other than markets, are called for. To the best of our knowledge, this is the first study of subject surrogacy involving a signaling game. Moreover, this study differs from earlier investigations by using a relatively large number of professionals (30) and 'adequate' monetary rewards (see Section 3).

The paper is organized as follows. Section 2 describes the signaling game and its theoretical properties. It also presents the hypotheses to be tested. Experimental procedures are discussed in Section 3. Section 4 contains the experimental results and the tests of the hypotheses. Section 5 concludes.

2. Game and hypotheses

The signaling game underlying our experiment has two players: a sender S and a responder R (in the experiment neutrally labeled as participant A and B, respectively). The latter has to decide between two choices, labeled B1 and B2. The payoffs received by R and S are determined by the choice selected by R and the realization of a stochastic state variable k (in the experiment, the color of a disk), which can take the value white or black. The realization of this variable is private information to S; R only knows the prior odds that the value of k is white (2/3) or black (1/3). After the realization of k, but before R makes her decision, S has to decide whether or not to send a message to R. Sending a message bears a fixed cost, c, to S only. Sending no message is costless. In case S decides to send a message, he has to select the content of the message, which can be either 'the color is white' or 'the color is black'. After R is informed about the choice of S (no message, 'white', or 'black'), R chooses between B1 and B2.

We decided to focus on two parameter configurations (treatments). In treatment L, the cost c of sending a message is relatively low (0.5 Dutch guilders), whereas in treatment H this cost is relatively high (1.5 guilders). The game–theoretic predictions of the effect of the cost differential are clear-cut and will be explained below. The payoffs in guilders to R and S as a function of the choice by R (B1 or B2) and the value of k (white or black) are shown in Table 1. The sender's payoffs differ per treatment. If we had only changed the cost to S of sending a message, the expected equilibrium payoffs to S would have been different in the two treatments, and also different from R's expected payoff in at least one treatment. To give the game–theoretic predictions their best chance, by minimizing the potential effects of motivations like fairness or altruism, we chose to have a design where the expected equilibrium payoffs are the same over the two players' roles and over the two treatments.

Note from the payoffs in Table 1 that the players have a common interest when the color is black: both prefer B2. However, if the color is white their interests conflict: R prefers to

[8] Anderson and Sunder (1995) found professional traders in an experimental auction to be more risk-averse and more accurate in estimating probabilities than students. On the other hand, Dyer et al. (1989) report that the professionals in their auction showed risk-neutrality, whereas the students exhibited risk-aversion.

Table 1
Payoffs (in Dutch guilders) to sender and receiver in the two treatments depending on the color of the disk (white or black) and the choice of the receiver (B1 or B2)

Payoff to S, R		R's choice	
		B1	B2
Low message cost (c=0.5)			
State	White (P=2/3)	2, 3	4,1
k	Black (P=1/3)	1, 0	7, 1
High message cost (c=1.5)			
State	White (P=2/3)	1.5, 3	3.5, 1
k	Black (P=1/3)	1.5, 0	5.5, 1

play B1, but S (again) prefers R to play B2. This illustrates the basic dilemma faced by the players. When the color is black S would like to send a message to R to reveal his private information, and it would be in R's interest to believe S's message. However, given that R is inclined to believe messages, S will be tempted to send a message to R — saying that the color is black — even when the color is in fact white. Realizing this, R is likely to maintain some healthy skepticism towards S's messages.

To indicate the game–theoretic predictions for the signaling game, we introduce some notation first. Let S's strategy be denoted by $\sigma_k(s)$. It represents the probability that S sends signal s, with $s\in\{$n,'w','b'$\}$, given the color $k\in\{$w(hite),b(lack)$\}$, where the signals n, 'w', and 'b', stand for no message, 'the color is white', and 'the color is black', respectively. Furthermore, let R's strategy be denoted by $\rho(s)$, which represents the probability that the responder chooses B2 given R's signal s.

Focusing on sequential equilibria, it is first noted that every message ('w' or 'b') sent with positive probability should have the same effect on the responders choice, that is, ρ('w')=ρ('b'). As the decision to send a message implies the fixed cost c, no matter its content, the content itself ('w' or 'b') is equivalent to cheap talk. For the equilibrium analysis of the game this implies that we can disregard the content of the message and proceed as if the signal space contains only two elements: no message (n) and a message (m), with $\sigma_k:=\sigma_k(\text{m})=1-\sigma_k(\text{n})$, k=b,w.

There is a sequential equilibrium for this game in which the sender never sends a message, $\sigma_k=0$, k=w,b. Since the two 'types' of the sender pool in this equilibrium, the responder will base her choice on her prior beliefs and opt for choice B1: $\rho(\text{n})=0$. As the sequential equilibrium concept does not restrict R's belief in case she might receive an off-equilibrium message, R is allowed to play B1 in that event, that is, $\rho(\text{m})=0$. However, the beliefs that support this equilibrium fail to pass the stronger refinements tests — like Universal Divinity (Banks and Sobel, 1987) — that have been proposed to exclude implausible off-equilibrium beliefs. The intuition for this failure is that R should realize that a costly message is more likely to be sent by S when the color is black, because this sender type has a larger stake in trying to persuade S to choose B2.

It turns out that there is a unique sequential equilibrium that satisfies all of the refinements. For expositional reasons, we derive this equilibrium in an informal way (for formal proofs, see Potters and van Winden, 1992). First note that in both treatments S has a larger stake in trying to persuade R, by sending a costly message, when the color is black. Hence we have that $\sigma_b>\sigma_w$, as perfect pooling via $\sigma_w=\sigma_b=1$ cannot be optimal (for in that case a message would reveal no information to R, and thus, lead to ρ(m)=0). Realizing that a message is more likely to be sent when the color is black, and no message when the color is white, R will respond with $\rho(m)>\rho(n)=0$. Obviously, $\rho(m)=1$ cannot hold, because then each sender type would like to send a costly message for sure. Hence, we must have that $0<\rho(m)<1$, which requires that the responder is indifferent between choosing B1 or B2 when a message is received. Now, given a positive response by R to a costly message, it should follow that $\sigma_b=1$ whenever $\sigma_w>0$, because S has a strictly larger stake in sending a message when k=b. Hence, we must have that $\sigma_b=1>\sigma_w>0$, since $\sigma_w>0$ is required to make the responder indifferent. But this implies that the sender should be indifferent between sending a message and not sending a message, when k=w. It is straightforward to verify that these two conditions — the sender being indifferent when the color is white, and the responder when she receives a message — can be satisfied simultaneously if and only if $\sigma_w=1/4$, in both treatments, and $\rho(m)=1/4$ in treatment L and $\rho(m)=3/4$ in treatment H. It follows that increasing the cost to the sender of sending a message should (only) have a positive impact on the receiver's response to a costly message. Summarizing we have the following equilibrium predictions:

$$\text{Treatment L (Low message cost)}: \sigma_w=\tfrac{1}{4},\quad \sigma_b=1,\quad \rho(n)=0,\quad \rho(m)=\tfrac{1}{4}$$

$$\text{Treatment H (High message cost)}: \sigma_w=\tfrac{1}{4},\quad \sigma_b=1,\quad \rho(n)=0,\quad \rho(m)=\tfrac{3}{4}$$

Having described the nature of the game and its equilibrium outcomes, we are now in the position to formulate our hypotheses. We start out with some general hypotheses concerning the qualitative (comparative) aspects of the sequential equilibrium predictions. These hypotheses are of interest in themselves, and they prepare the ground for the remaining hypotheses pertaining to differences between student and professional subjects. We do not merely focus on the quantitative 'point predictions' because these values are derived under the assumption of strict selfishness and risk neutrality. The qualitative aspects of these predictions are not, or at least to a much lesser extent, dependent on the actual pay-off and risk attitudes of the players. Since strategies cannot be directly observed, we will use the empirical frequency with which a message is sent in case of color k (denoted by s_k), and the empirical frequency with which B2 is chosen in case of signal s (denoted by $r(s)$).

Hypothesis 1 (Signaling). (i) Senders engage in costly signaling, with $s_b>s_w$; (ii) responders are influenced by costly signals: $r(m)>r(n)$.

Hypothesis 2 (Treatment effect). Only the impact of a message increases with its cost: $r(m)_{\text{treat H}}>r(m)_{\text{treat L}}$.

In this study, we are particularly interested in the extent to which these hypotheses apply differently to students and professionals. For the formulation of our hypotheses pertaining to this issue an important observation in the literature is that differences between the behavior of students and professionals are typically small and insignificant. Therefore, we take as our null-hypothesis:

Hypothesis 3.0. There are no significant differences in the observed behavior of the two subject pools.

As mentioned in Section 1, counterevidence has been mainly attributed to the following factors: *familiarity* (field-experience) with the environment, a different *risk-attitude*, and *rules of conduct* developed in the field. It is important to note that these factors mainly pertain to the professionals in their role as senders. Public affairs managers are professionally skilled to transmit information and to influence the beliefs and behavior of policymakers or other relevant audiences. They are not necessarily more skilled in processing and evaluating information. Therefore, if at all, we would expect differences between the two subject pools mainly to show up in the behavior of the senders in our signaling experiments.

The first factor, in particular the familiarity with strategic information transmission, could show up in behavior by professionals that is more closely in line with the game–theoretic predictions than the behavior of students. The second factor could reinforce this behavioral difference to the extent that professionals are less risk-averse (more risk-neutral), as suggested by Dyer et al. (1989). Therefore, we take as our first alternative hypothesis:

Hypothesis 3.1 (Gamesmenship). The behavior of the professionals in their role as senders is closer to the (risk-neutral) game–theoretic predictions than that of the student subjects.

Another way in which familiarity with strategic information transmission could unfold is that the professional senders are able to achieve higher earnings. By playing the refined sequential equilibrium, in each treatment both senders and responders can expect to earn two guilders. If, however, the senders can somehow achieve full separation ($\sigma_b=1$, $\sigma_w=0$ and $r(n)=0$, $r(m)=1$) their expected earnings are 3.5 guilders in treatment L and 2.33 guilders in treatment H. In that case the expected earnings of the responders will rise to 2.33 guilders. Hence, there are substantial incentives to arrive at this, 'cooperative', separating outcome. However, this requires that the senders succeed in establishing a higher degree of information disclosure ($\sigma_b-\sigma_w=1$) than in the equilibrium outcome ($\sigma_b-\sigma_w=3/4$). Senders should, in particular, refrain from sending deceitful messages when the color is white. Perhaps, professional experience with information transmission or professional rules of conduct, such as the 'never cheat or misinform' rule discussed in Section 1, could make the cooperative outcome a greater attractor for the professionals than the students. Therefore, we formulate:

Hypothesis 3.2 (Separation and earnings). Professional senders show a higher degree of separation (s_b-s_w) and/or achieve higher earnings than student subjects in the same role.

Our final hypothesis concerns patterns of learning and adaptation. Professional rules of conduct could have two opposite effects here. On the one hand, the emphasis in the professional lobbying literature on monitoring the reactions of opponents ('closely monitor the feedback from policymakers', see Section 1) could show up in a stronger response to experience in the experiment than the response exhibited by students. Given the lack of transparency of the institutional environment in which lobbying takes place, such a rule is quite understandable. On the other hand, more static rules of conduct, like the 'never cheat or misinform' rule, could manifest themselves in a weaker response to experience by the professionals. If professionals apply such rules of thumb in the experiment they may be less inclined to adapt their behavior in response to observations in the experiment (Burns, 1985; Mestelman and Feeny, 1988). Hence, a priori it is not clear in which direction any effects of professional rules of conduct are likely to go. This leads to the following hypothesis:

Hypothesis 3.3 (Learning and adaptation). In comparison with student subjects, professionals display different patterns of learning and adaptation in response to experience in the experiment.

Note that the alternative Hypotheses 3.1–3.3 are neither mutually exclusive nor necessarily compatible. For example, gamesmenship is perfectly compatible with a weak response to experience, but (in a strict sense) it conflicts with achieving a cooperative outcome. This reflects the scarcity of evidence on subject-pools effects, which calls for a more exploratory approach concerning the locus and direction of such effects.

3. Experimental design and procedures

In total, 15 experimental sessions were conducted, 12 of which with students as subjects and three with professionals. In total, 142 students (S) and 30 professionals (P) participated. Six of the student sessions concerned the low cost (L) treatment (denoted by SL1–SL6), and the other six the high cost (H) treatment (SH1–SH6). Two of the sessions with professionals were conducted under the low cost treatment (PL1 and PL2), and one under the high cost treatment (PH1). Each session had 10 or 12 subjects as players and one subject to perform the role of monitor (observer). Table 2 provides some information on the characteristics of the sessions. All the sessions with professionals — which took place at a conference center — were of the 'pen and paper' type. In case of the students, four of the six sessions under each treatment were also conducted with pen and paper. The main reason was that we did not have a computerized laboratory at the time of the first sessions. The remaining two student sessions under each treatment were conducted by computer in the CREED-laboratory in Amsterdam. Of course, procedures were duplicated as much as possible. This appeared to be successful, since no significant differences were found between the two modes of conducting the experiment (if anything, slightly more 'cheating' messages were sent in the computer mode).

Student subjects were recruited among the undergraduate student population of the University of Amsterdam. In announcements, they were requested to participate in a 2 h experimental study of decision making which would earn them money. The majority of the participants were economics majors.

After a short introduction in the reception room, the student subjects were randomly assigned a role (sender, responder, or monitor) by picking an envelope which contained the role. Once the subjects and the monitor were seated in the laboratory — at tables with

Table 2
Session characteristics

Session	Treat[a]	Pool[b]	No. of subjects	Mode[c]	Date
PL1	Low	Prof	10	p&p	5/92
PL2	Low	Prof	10	p&p	1/93
PH1	High	Prof	10	p&p	1/93
SL1	Low	Stud	12	p&p	1/92
SL2	Low	Stud	12	p&p	3/92
SL3	Low	Stud	12	p&p	4/94
SL4	Low	Stud	12	p&p	4/94
SL5	Low	Stud	12	comp	4/94
SL6	Low	Stud	12	comp	4/94
SH1	High	Stud	12	p&p	1/92
SH2	High	Stud	12	p&p	3/92
SH3	High	Stud	12	p&p	4/93
SH4	High	Stud	10	p&p	4/93
SH5	High	Stud	12	comp	4/93
SH6	High	Stud	12	comp	4/93

[a] Low: low cost; high: high cost (parameter treatment).
[b] Prof: professionals; stud: students.
[c] p&p: pen and paper; comp: computerized.

partitions — the instructions were distributed and read aloud (an English translation of the instructions is provided in Appendix A). In the instructions, senders and responders were, respectively, called A and B participants. The message character of the sender's signal was retained in the experiment. Senders had to decide to send a message ('white' or 'black') or to send 'no message'. Responder's choices were labeled B1 and B2. The student sessions consisted of two parts, each part beginning with one practice period followed by 10 periods (rounds) of play. Instructions for the second part were read only after the first part was finished. The only difference between the parts was that subjects changed roles (senders became responders, and vice versa). Because of the time constraint at the conferences where the sessions with the professionals were held (see below), we were not able to conduct this second part with the professionals. Therefore, only the data of part 1 of the student sessions will be used for comparison with the professionals.

The professional subjects were recruited among the subscribers to two different conferences on public affairs, one held in Amsterdam (PL1) and the other in The Hague (PL2 and PH1). They were informed by mail that participation in a 1 h experimental study of decision making, which would earn them money, was optional. Participants were executives and, in particular, public affairs and public relations officers from the private and the public sector. Experimental procedures were identical to those of the student sessions, except that the sessions with professionals were held in a conference room. Moreover, we quadrupled their payoffs and message costs. The payoffs in Table 1 are based on the convention in experimental economics that subjects should, on average, at least be paid the opportunity cost of participation. The expected equilibrium earnings of 20 guilders in 1 h (10 rounds of play, with an expected payoff of two guilders per play) are above the 15 guilders that a student would typically earn per hour in an odd job, such as working in a bar or restaurant.

For the professionals, comparable monetary incentives were less easy to determine in view of the fact they had already subscribed to participate in the conference before signing up for the experiment. After consulting a public affairs consultancy firm, that was involved in the organization of one of the conferences, it was decided to multiply the payoffs and message cost by four. As a result, their expected equilibrium earnings were 80 guilders for 1 h. [9]

All subjects (students and professionals) participated only once, and no one had previously taken part in a similar experiment.

After the instructions were read the procedures were as follows. In every period, each sender was matched with a responder. Subjects could not know whom they were matched with in any period. The matching scheme was determined randomly before the experiment under the constraint that no match would stay the same for two (or more) consecutive periods, nor occur more than twice during the 10 periods. [10] Subjects were informed about this constraint. Moreover, the matching scheme was handed out to the monitor, so that (s)he could check the procedure if (s)he so desired. In each period the game presented in the previous section was played according to the following rules. At the start of the period a white or black disk (checker) was drawn by the monitor from an urn. Whereas all subjects knew that the urn contained one black and two white disks, the disk drawn by the monitor was only revealed to the subjects playing the role of sender. After seeing the color of the disk a sender decided whether or not to send a message to the unknown responder he or she was paired with in that period. In case of a message, the sender had to choose between the announcement (the disk is) ‘white’ or ‘black’. Sending a message involved a fixed cost for the sender (as indicated in Table 1). After the sender had recorded her or his decision, it was privately communicated to the responder, and the responder had to choose between B1 and B2. This choice was then communicated privately to the sender and the color of the disk was revealed to the responder (by the monitor). Results for the other pairs of subjects were not revealed.

Possible payoffs were presented in tables (see Appendix A) and expressed in guilders. They were also projected on a wall, for all to see. Earnings were privately paid in cash at the end of the experiment, after the completion of a questionnaire.

The only difference between the sessions with professionals and students was that, after 10 periods of play, students changed roles and proceeded with another 10 rounds of play (without knowing that they would do so until after the completion of the first 10 periods). The only difference between the computer sessions and the pen and paper sessions was that messages and choices were communicated to the other player by the computer in the former, and by us in the latter (by marking the appropriate columns on subjects’ record sheets). In both types of sessions the monitor could check, using the matching scheme, whether the decisions made by the subjects corresponded with those communicated.

[9] Taking an estimated yearly net income of 150,000 guilders as point of departure, the net salary of professionals is about 80 guilders per hour.

[10] The main reason to have randomly matched pairs instead of fixed pairs, is that we wanted to mimic the one-shot character of the game-theoretic equilibrium predictions as closely as possible while at the same time allowing the subjects to learn and adapt their behavior in response to experience.

Table 3
Frequencies of messages when the color was white (s_w, first entry) or black (s_b, second entry), averaged over the session aggregates[a]

	s_w, s_b		
	L (low cost)	H (high cost)	Row average
Professionals	0.515 (0.243), 0.834 (0.047)	0.343 (0.000), 0.933 (0.000)	0.457 (0.198), 0.867 (0.067)
Students	0.549 (0.110), 0.688 (0.117)	0.373 (0.166), 0.710 (0.108)	0.461 (0.163), 0.699 (0.108)
Column average	0.540 (0.132), 0.724 (0.121)	0.368 (0.152), 0.742 (0.130)	0.460 (0.162), 0.732 (0.121)

[a] Standard deviations in parentheses.

4. Results

In this section, we discuss the experimental results in relation to the hypotheses formulated in Section 2. A principal question to be addressed first, is what to take as an observation. There are three possibilities: each play of the game, each individual subject, or each session. In total, we have 15 sessions, with in each session 10 rounds of a particular game, played by either five (sessions PL1, PL2, PH1 and SH4) or six pairs of subjects (SL1–SL6, SH1–SH3, SH5 and SH6). Hence, there are 15 session level observations, 172 subject level observations, and 860 game level observations. Because of dependency of observations at the individual level, we will use the observations at the session level for the tests of our hypotheses.[11] Furthermore, throughout we will base our statistical conclusions on non-parametric tests (Wilcoxon's signed-ranks test for matched pairs, and the Mann–Whitney U-test for independent pairs), using $P<0.10$ (one-tailed) as threshold for significant effects.

We start with testing the general Hypotheses 1 and 2 presented in Section 2, concerning the occurrence of signaling and treatment effects. Table 3 presents the observed frequencies with which costly messages were sent by senders when the color was white (s_w) or black (s_b). The figures represent averages over the sessions, where we distinguish between subject pools (students and professionals) and treatments (low message cost and high message cost). Table 4 gives similar figures for the responders: the frequency of B2 choices when no message was received, r(n), and when a message was received, r(m).[12]

[11] Analysis of the data reveals that taking individual subject data as unit of observation would disregard significant dependencies within sessions. In particular, analyses of variance as well as Kruskal–Wallis tests show significant differences between sessions with the same experimental design (i.e. with the same treatment and subject pool) for several of the main variables of interest. Also, Mann–Whitney tests reveal significant differences between some of the sessions with the same design in pairwise comparisons. Therefore, we take the more conservative approach and use session aggregates as units of observation.

[12] In the analysis reported in the text, we decided to pool 'white' and 'black' messages: $s_k=s_k(\mathrm{w})+s_k(\mathrm{b})$. This is not because the content makes no difference. It turns out that 'black' messages are reacted to more strongly by responders than 'white' messages: $r(\mathrm{b})>r(\mathrm{w})>r(\mathrm{n})$. However, the frequency of 'white' messages is too low to allow for a powerful statistical analysis. For example, in one session only one 'white' message occurred. Furthermore, none of the results depends in any way on this pooling over message content. Also, there are no signs in the data that the content of the message plays a different role for the student subjects than for the professional subjects.

Table 4
Frequencies of B2 choices after no message (r(n), first entry) and after a message (r(m), second entry), averaged over the session aggregates[a]

	r(n), r(m)		
	L (low cost)	H (high cost)	Row average
Professionals	0.076 (0.006), 0.267 (0.094)	0.250 (0.000), 0.654 (0.000)	0.134 (0.101), 0.396 (0.233)
Students	0.061 (0.104), 0.273 (0.106)	0.042 (0.038), 0.501 (0.105)	0.052 (0.076), 0.387 (0.156)
Column average	0.065 (0.088), 0.272 (0.096)	0.071 (0.086), 0.523 (0.112)	0.068 (0.084), 0.389 (0.164)

[a] Standard deviations in parentheses.

Hypothesis 1 (Signaling). From Tables 3 and 4 it is evident that senders engaged in costly signaling ($s_b > s_w$), and that responders reacted in the expected direction to these signals (r(m)>r(n)). The average frequency of messages was 0.732 in case the color of the disk was black (s_b), and 0.460 when the color was white (s_w). The difference is significant at P=0.001, using a one-tailed Wilcoxon test with session aggregates as observations. Responders chose B2 at a rate of 0.389 when they received a message (r(m)), and at a rate of 0.068 when they received no message (r(n)). Using the same test, the difference is again highly significant (P=0.001). [13]

Hypothesis 2 (Treatment effect). According to the predictions of the refined sequential equilibrium, the higher cost of a message for the sender in treatment H, in comparison with treatment L, should (only) increase the impact of a message. This leads to the expectation that $r(\mathrm{m})_H > r(\mathrm{m})_L$, which is supported by the data. As Table 4 shows, responders chose B2 at a rate of 0.523 after a message in treatment H, and at a rate of 0.272 in treatment L (averaged over the sessions). The difference is significant at P=0.001, using a one-tailed Mann–Whitney test.

Also in line with the hypothesis is that no significant treatment effect on the frequencies of s_b and r(n) is found, using the same test. However, with respect to messages in case the color was white (s_w) it turns out that these were sent at a significantly higher rate when the cost of doing so was relatively low. As Table 3 shows, this frequency is 0.540 in treatment L and 0.368 in treatment H (averaged over sessions). The difference is significant at P=0.029. Apparently, senders send more messages when the cost c of doing so, relative to the potential benefit, is low. This relative cost equals 0.25=0.5/(4–2) in treatment L and 0.75=1.5/(3.5–1.5) in treatment H (see Table 1). Although this outcome may seem rather intuitive, it was not to be expected on the basis of the game–theoretic analysis. [14]

[13] The value of r(n)=0.250 for the professionals in the high cost treatment is remarkably high. As a consequence, taking all session data, the value of r(n) is significantly higher for professionals than for students at P=0.10 with a two-tailed test. This, however, is entirely due to one professional subject who chose B2 in each of the 10 periods, irrespective of whether a message was sent or not. Possibly this subject wanted to maximize the sum of the payoffs to sender and receiver. In any case, if this subject were excluded as an outlier, then we would have r(n)=0.050 (and r(m)=0.570) for this session and there would be no difference between professionals and students.

[14] A general property of mixed strategy equilibria is that the players keep one another indifferent. As a consequence, the change of a payoff parameter of player S has an impact on the strategy of player R but not on player S's own strategy.

The results presented so far are in line with our earlier experimental findings, which showed that the sequential equilibrium outcomes fail as accurate predictions of the experimental results. A further discussion of the extent to which the results do support the equilibrium predictions is beyond the scope of the present paper. For a detailed analysis, the reader is referred to our earlier paper (Potters and van Winden, 1996). We turn now to an investigation of the Hypotheses 3.0–3.3 which are the main focus of this paper. [15] These hypotheses relate to potential differences between students and professionals in terms of gamesmen-like behavior (Hypothesis 3.1), cooperative behavior and earnings (Hypothesis 3.2), and the behavioral response to experience (Hypothesis 3.3). Our null-hypothesis (Hypothesis 3.0) is that there are no significant differences in the behavior of the two subject pools.

Hypothesis 3.1 (Gamesmenship). The refined sequential equilibrium prescribes for senders (under strict selfishness and risk-neutrality) that a costly message should always be sent when the color is black, $s_b=1$, and at a rate of $s_w=1/4$ when the color is white. One way of defining the prediction error for the senders is provided by the following measure: $1/2|\sigma_b - s_b| + 1/2|\sigma_w - s_w|$. From Table 3, it can be derived that this prediction error is smaller for the professionals (0.170) than for the students (0.278). The difference between the pools is significant at $P=0.090$ with a one-tailed Mann–Whitney test. [16] Closer inspection reveals that the difference in prediction error depends entirely on s_b, which is significantly larger (at $P=0.015$, one-tailed) for professionals (0.867) than for students (0.699), whereas s_w is almost identical for professionals (0.457) and students (0.461).

Hence, we find weak evidence that the professionals' behavior is more in line with the game–theoretic predictions (refined sequential equilibrium) than that of the students. The difference rests on s_b which is closer to the theoretically predicted value of $\sigma_b=1$ for professionals than for students. In other words, if the state of the world is such that both sender and receiver would benefit from the choice of B2 by the receiver (no conflict of interest), professional subjects are more likely than students to send a message in order to sway the beliefs of the receiver, as they should according to the theoretical prediction.

Hypothesis 3.2 (Separation and earnings). In case professional subjects would behave according to the rules of conduct mentioned in Section 1 ('never cheat or misinform' and 'avoid conflicts of interest'), one would expect more cooperative behavior — that is, s_b closer to 1 and s_w closer to 0 — for the professionals. At any rate, one would expect the professional senders to achieve a higher degree of separation and information disclosure. We have already seen that s_b meets this expectation, whereas s_w does not. As a result, the degree of separation ($s_b - s_w$) is higher for professionals (0.410) than for students (0.238), but the difference is not significant ($P=0.145$ with a one-tailed Mann–Whitney test).

[15] The significance levels found with respect to signaling and the treatment effect show that 15 (session) observations are sufficient for nonparametric tests to pick up effects that are strong and systematic.

[16] If we weigh the prediction errors $|\sigma_b - s_b|$ and $|\sigma_w - s_w|$ with the expected prior probability of the relevant color (1/3 and 2/3, respectively) then the difference between the two pools is somewhat smaller (0.182 for professionals and 0.256 for students) and no longer significant ($P=0.147$).

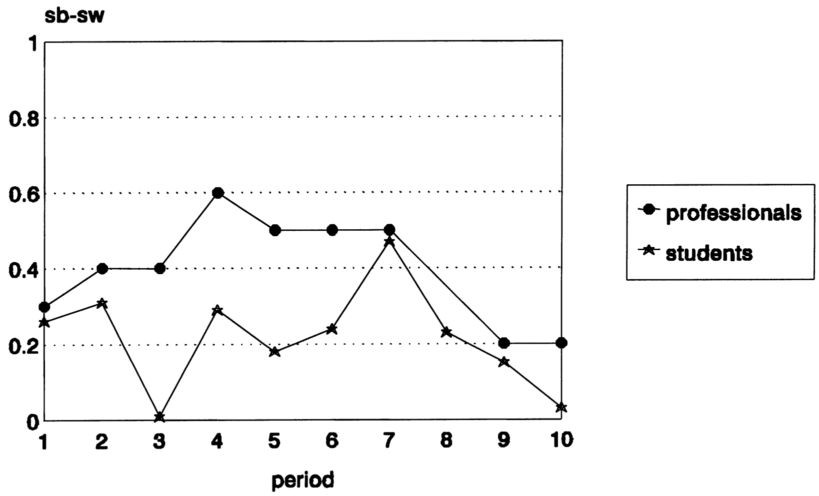

Fig. 1. Development of the degree of separation.

A look at the development of separation corroborates this picture. Fig. 1 presents the degree of separation ($s_{b,t}-s_{w,t}$) for each of the 10 periods, averaged over sessions for each subject pool. It appears that in all periods the professionals achieve a larger degree of separation, but that the differences are not very pronounced, especially not in the final periods. In the last two periods, the professionals achieve a degree of separation of only 0.20, much below the level of $\sigma_b-\sigma_w=0.75$ that is theoretically predicted.

With respect to earnings, it turns out that the professionals earned more money in their role of senders than the students. Using the student payoff parameters for comparison, average earnings per period are 2.10 guilders for professionals and 1.86 guilders for students. The difference is not significant though ($P=0.147$, one-tailed). This is partly due to the fact that the colors drawn were a little less favorable for the professional senders (30% black disks) than for the student senders (38% black disks). If we take the prior probability that a black disk (33%) or a white disk (67%) is drawn to calculate expected earnings, given the behavior of the subjects, then the difference between the two pools becomes significant ($P=0.090$, one-tailed).

Finally, it is interesting to take a closer look at individual behavior. In particular, we examine the fraction of subjects engaging in perfect separation ($s_b=1$ and $s_w=0$), since this kind of behavior would be in line with the rule of conduct of ‘never cheat or misinform’. It turns out that the average fraction of subjects adhering to this rule is larger for the professionals (0.20) than for the students (0.04). The difference is significant at $P=0.068$ (one-tailed Mann–Whitney test, with session averages as observations). Interestingly, the three (out of 15) professionals which strictly adhere to this rule of perfect separation are responsible for most of the differences between the two subject pools. Excluding the subjects engaged in perfect separation (three professionals and three students) from the data and rerunning the tests, the differences between the two subject pools in terms of separation and earnings all but disappear. The only difference remaining concerns s_b, which stays significantly larger for the professionals (P=0.035).

Summarizing, we find evidence that the professionals disclose more information, and that (as a result) they earn more money. Also, the fraction of subjects that perfectly separate (s_b=1 and s_w=0) is significantly larger for professionals than for students. In fact, it is this small fraction of perfectly separating professionals (20%) that is responsible for most of the differences between the two subject pools.

Hypothesis 3.3 (Learning and adaptation). Subjects received full feedback information on the choices and earnings in the plays of the game they themselves participated in (thus, not regarding sender–responder pairs that they were no party to). We relate the behavior of the senders to this information in the following two ways, which distinguish between belief learning and rote learning (Selten, 1991). Under the former type of learning subjects adapt their behavior to experience via its impact on their beliefs. More specifically, subjects are here hypothesized to increase the frequency of choices which are a best response to the aggregate history of play, using the observed frequencies of choices by opponents to update their beliefs about these choices. [17] In case of rote learning, a simple stimulus-response process is assumed. In that case subjects are hypothesized to increase (decrease) the frequency of choices which turned out to be successful (unsuccessful) in the previous period of play, where success is defined by higher earnings than in the immediately preceding round of play (t−2). [18]

We start with the cognitively more demanding learning model of 'best response to history' (BRTH), which is closest in spirit to gamesmen behavior. Averaged over the sessions we find that a message was sent at a rate of 0.756 when it was BRTH to do so, and at a rate of 0.455 when it was BRTH not to do so. The difference is highly significant (P=0.001 with one-tailed Wilcoxon test using the 15 session aggregates as observations). Furthermore, the magnitude of the effect [s_k(BRTH=m)−s_k(BRTH=n)] is somewhat smaller for professionals (0.232) than for students (0.320), but the difference between the two subject pools is not significant (P=0.448 with a two-tailed Mann–Whitney test). [19] The conclusion is that there is significant evidence of belief learning, in the direction of BRTH, but no support for a significant subject-pool effect.

So what about rote learning? It turns out that, indeed, relatively more messages occurred when sending a message had been successful, or sending no message had been unsuccessful, in the previous round (0.651 versus 0.522). The difference is significant at P=0.039 (one-tailed Wilcoxon test). Furthermore, the magnitude of the effect is a bit larger for pro-

[17] For each sender in a particular period t we calculate the cumulative frequencies of $r(\mathrm{m})_{t-1}$ and $r(\mathrm{n})_{t-1}$ up to that period. Then, we determine whether the value of $r(\mathrm{m})_{t-1}-r(\mathrm{n})_{t-1}$ multiplied by the gain for the sender when B2 instead of B1 is chosen exceeds the cost of sending a message. If this is (not) the case, then (not) sending a message is the best response to history.

[18] Thus, for senders, if sending a message (no message) in the previous period earned them more (less) than in the preceding period ($t-2$) this should increase the frequency with which messages are sent in period t; otherwise, this frequency should decrease.

[19] We take a two-tailed test here because Hypothesis 3.3 does not indicate whether we should expect a stronger or weaker response to experience by the professionals.

fessionals (0.156) than for students (0.122), but this difference is not significant (P=0.820 with a two-tailed Mann–Whitney test). Thus, there are also signs of rote learning, but the evidence is weaker than for belief learning. Furthermore, again no support for a significant subject-pool effect is found.[20]

Summarizing, we find support for learning — mainly in the direction of best response to history, as in our earlier study (Potters and van Winden, 1996) — but no evidence of significant or systematic differences between the two subject pools. Consequently, we find no evidence for a differential impact of the professional rule of conduct ‘closely monitor the feedback from policymakers’, mentioned in Section 1.

5. Concluding discussion

Our experimental study of lobbying shows that professional lobbyists behave more in line with game–theoretic predictions, show a higher degree of information disclosure (separation), and earn more money.[21] These results are driven by two findings. First, compared with student subjects, professionals send more frequently costly messages when there is no conflict of interest with the responder (i.e. s_b is larger). Second, a small group of professionals (20%) always sends a message if there is no conflict of interest with the responder, and they never send a message if such a conflict exists; in other words, they perfectly separate (i.e. s_b=1 and s_w=0). Among the student subjects only 4% behave in this way. Except for s_b, all differences between students and professionals disappear if these perfect separators are excluded from the data. Our conjecture is that both findings can best be explained by professional rules of conduct, as discussed in Section 1. One frequently mentioned rule — ‘avoid conflicts of interests’, focusing attention on ‘win-win’ situations — may induce professional lobbyists to be relatively keener on taking costly action when the responder is likely to make a decision which runs counter to the interests of both. This may explain the larger value of s_b. On the other hand, the most frequently mentioned rule that we found — ‘never cheat or misinform’ — is strikingly in line with the behavior of the perfect separators. However, the fact that only a small group behaved according to this important rule of conduct would then suggest a limited extent of professionalization. We will return to this issue below.

A natural question that can be raised in this context is to what extent our signaling game resembles ‘real-life lobbying’. In this respect it is noted that providing policymakers with information is often considered to be one of the most important means to influence the

[20] This conclusion does not change when, for the definition of success, earnings obtained with a particular choice (action) in the previous period are compared with its *average* earnings up to that period.

[21] Apart from the above analyses we also compared the two subject pools in other respects, such as the behavior in the first and last period(s), and the variance in behavior within the two pools. With respect to variance in behavior, it is noted that DeJong et al. (1988) found that businessmen exhibited greater variance in market behavior than students. However, we found no differences between the two pools in any other respect than the ones reported in the main text. We also looked at the differences between the two subject pools in their role of responders. We did not find any significant or systematic differences. For example, the professional responders do not behave more in line with the game-theoretic predictions, do not respond to messages more strongly (r(m)−r(n)) (see, however, footnote 13), do not earn more money, and display neither a stronger nor a weaker response to experience.

policymaking process. As Ornstein and Elder (1978, p. 75) put it: “the ability of a group to command facts, figures, and technical information in support of its positions is another key organizational resource. (. . .) a group that can provide persuasive data to support its case has an important advantage”. Although policymakers recognize that lobbyists may have valuable private information, they are quite aware of the strategic incentives for lobbyists in presenting or withholding this information (see, e.g. Zeigler and Baer, 1969, p. 109; Schlozman and Tierney, 1986, p. 298). According to Appels (1985, p. 308): “This need not imply outright lie or dishonest manipulation, although these cannot be excluded with certainty”. Thus, it seems that basic features of real-life lobbying are represented by our game: there is asymmetric information, with S (the sender) holding information which is valuable for R (the responder); there is mutual dependence, because S is affected by the action of R; there is interaction, when S tries to influence R by sending messages; and there are partially conflicting interests, since S sometimes has an incentive to misinform or to withhold information from R. Furthermore, our game echoes the fact that lobbying (unlike, for example, market exchange) typically takes place in an environment with a ‘thin’ institutional structure. In combination, these features also make the game strategically interesting, and not at all trivial.

Of course, one could question whether the subjects took the experiment seriously. Also, as a study of lobbying, one could argue that the experimental design perhaps eliminated essential aspects of the lobbying environment in the field (cf. in this context Dyer and Kagel, 1996, on the winner’s curse). Although it is impossible to provide a definitive answer, we have the following arguments in support of our study. Regarding the first issue, there is no indication from observations during or discussions after the experimental sessions, nor from responses to a debriefing questionnaire, that one should be worried about the seriousness with which both types of subjects determined their decisions.[22] With respect to the experimental design we have the following arguments, in addition to the ones mentioned above. First, comments collected during a seminar presented at a public affairs consultancy firm (before the experiment, but with different professionals) gave support to the relevance of the design. Of course, other issues of interest were mentioned — such as the use of intermediate agents — but these related more to adding complexity to the experiment. Second, it is noted that there is at present a strongly felt need of enhancing the degree of professionalization in this field (this was also a central issue at the conferences where the experiments with the professionals were run). This makes it perhaps less surprising why few, but nevertheless interesting, subject-pool effects were detected in the experiment.

In view of the differences found between the two subject pools, an important methodological question is whether one can use student subjects as a surrogate for professionals in this field of research (lobbying or public affairs). We believe that an affirmative answer is justified. First, although we found some statistically significant differences, the size of these differential effects is generally small. Second, for by far most of the professional subjects (80%) there are no differences with the students at all (with the exception of a somewhat larger value of s_b). Third, if the differences turn out to be robust and systematic one could adjust the data obtained with student subjects in these respects. Fourth, and most

[22] For instance, the responses to the questionnaire showed that only two professional subjects found the experiment (a little) boring, and only three of them thought the possible earnings were low.

importantly, for the analysis of comparative-statics effects and the effects of alternative institutions student subjects remain useful, because the direction of the effects is generally the same.

With respect to the last argument, it is noted that in case of lobbying, as in other fields of interest, the more interesting questions are rather of a qualitative (comparative-statics) nature than of the point-estimate type. As Kagel and Roth (1992, p. 1389) put it: “We think that much of the potential contribution of experimental methods lies in their ability to provide serious tests of the basic comparative-static implications of hypotheses of economic interest”. Thus, instead of questions like: What is the precise frequency with which influence attempts are undertaken by lobbyists? or: How often exactly are policymakers persuaded by the messages of lobbyists?, the more interesting questions would seem to be: How does information disclosure interact with (partial) conflicts of interests?, How is the degree of information disclosure related to the stakes involved in informing and misinforming? or: How does the influence on policymakers depend on the ease of access and the cost of messages?

It is these types of questions that we addressed in our earlier theoretical paper (Potters and van Winden, 1992), and the subsequent experimental test (Potters and van Winden, 1996). The main conclusion from the latter paper is that game theory (sequential equilibrium) is helpful in organizing the experimental data, but fails as a good predictor of the comparative-statics. It turned out that a decision–theoretic model, supplemented with belief learning, served best in organizing the comparative-statics properties of the data. This conclusion is mainly based on the findings that the student subjects responded strongly to changes in own payoff parameters (contrary to the prediction of mixed strategy sequential equilibrium), and that the impact of experience was in the direction of best response to history. For these two main results of our earlier paper, we do *not* find a difference between students and professionals.[23] Thus, it appears that, even though we find some differences between the precise point estimates of the behavior of students and professionals, these differences do not affect the more interesting comparative-statics properties of their behavior, that is, the extent and the direction of behavioral changes in response to environmental parameters.

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[23] With respect to response to experience we have already seen that the direction and the extent of learning is similar for the two subject pools (Hypothesis 3.3). Regarding the treatment effect recall that (in contrast with the equilibrium predictions) the frequency of ‘deceitful’ messages decreases with the cost of a message: s_w(Treat L)−s_w(Treat H)=0.540−0.368=0.172. The size of this effect is similar for the student senders (0.549−0.373=0.176) and the professional senders (0.514−0.343=0.171).

Choice Society Meeting in New Orleans (1992), the workshop on experimental economics in Stony Brook (1992), the Amsterdam Workshop on Experimental Economics (1992), the IAREP Conference in Rotterdam (1994), the Competition and Cooperation seminar at CentER in Tilburg (1996), and the European Public Choice Society Meeting in Tiberias (1996). In particular, we are thankful for the comments by Uri Gneezy, Werner Güth, and Avi Weisz. We, finally, acknowledge the very helpful comments by the editor and an anonymous referee.

Appendix A. Instructions

The following is an exact translation from Dutch of the instructions for the pen and paper sessions.

A.1. Introduction ⟨Reception Room⟩

You are about to participate in an experimental study of decision making. The experiment consists of two parts. In total the experiment will last about 2 h. Before you will be invited to the laboratory, we ask you to draw one envelope from this box.

In the envelope you will find your 'registration number', which will be used throughout the experiment, and an indication of your role in the first part of the experiment. There are two roles: 'participant A' and 'participant B'. In the envelope, it is announced whether you have the role of participant A or B. One envelope is an exception to this rule. Instead of 'participant A' or 'participant B' this envelope contains the announcement 'monitor'. The monitor will watch us while we carry out the experiment and assist us from time to time. The monitor receives a payment of 40 guilders.

After you have taken an envelope, you are invited to enter the laboratory and take a seat behind a table reserved for an A or B participant. As you will see clearly indicated, participants A sit together in one part and participants B in another part of the room. A separate table is reserved for the monitor.

From the moment you have drawn an envelope you are no longer allowed to talk or communicate to the other participants. If you have a question, please raise your hand and one of us will come to your table. As soon as everyone has taken his or her seat in the laboratory, we will distribute further instructions and read them aloud.

Are there any questions about what has been said up till now? If not, then the person on the left of me is now requested to first pick an envelope, open it and go the laboratory.

A.2. Instructions

A.2.1. Introduction

This is an experimental study of decision making. Various research institutions have provided funds for this study. The instructions are simple and if you follow them carefully you may earn a considerable amount of money. All the money you earn is yours to keep. Your payoffs will be paid to you in cash, privately and confidentially, after the experiment.

We will begin by reading these instructions. Thereafter, you will have the opportunity to ask questions.

A.2.2. Decisions and earnings

The experiment will consist of two separate parts, and each part will consist of a number of periods. In each period a participant A will be matched with a participant B. Both participants will have to take one decision during the period. The earnings for A and B depend upon the decisions made, but are codetermined by the color of a disk. The color of the disk can be either *black* or *white*. At the beginning of each period the color of the disk will be determined by a drawing. The *probability* that a white or a black disk is drawn will be announced to both participants. The *outcome* of the drawing, however, will only be announced to participant A. Hence, participant A knows the color, whereas B only knows the probabilities of a white and a black disk. At the beginning of each period participant A decides whether or not to send a *message* to participant B. In case of a message, there is a choice between the announcement (the disk is) *'white'* and the announcement (the disk is) *'black'*. The announcement is allowed to differ from the real color. Sending a message bears a cost to participant A; the costs of a message will be stated in guilders.

Thus, the decision by participant A can either be a message, stating *'white'* or *'black'*, or *no* message. This decision will then be communicated to participant B. Each B participant will be notified of only one decision, namely, the decision of the A participant to whom he or she has been matched in that period. After taking notice of A's decision, participant B decides whether to react with *choice B*1 or *choice B*2. This decision (choice) will then be communicated to participant A and, finally, the color of the disk will be announced to B.

The earnings of A and B are (apart form the costs of a message) determined by the disk's color (white or black) and B's decision. These earnings are presented in *tables* on the sheet called DESCRIPTION, which has already been distributed ⟨see below⟩. You are now requested to take this sheet. In order to demonstrate that all of you have the same information, this sheet will also be projected on the wall.

First, you see the probabilities that the disk's color is white or black. Next, it is again indicated that participant A takes a decision whether or not to send a message, before participant B determines her or his choice. In case of a message, a choice must be made between the announcements 'white' and 'black'. The costs of a message are posted in guilders.

Finally you see two tables. The left table presents A's earnings and the right one presents B's. If you want to know a participant's earnings with a particular color of the disk and a particular choice by B, you first move to the table indicating *'earnings participant A'* or *'earnings participant B'*. Then you look up the color of the disk (white or black) and you move to the right to the column indicating the choice of B (choice B1 or choice B2). The figures are stated in guilders. For A participants, the costs of a message must be subtracted if a message was sent.

A.2.3. The monitor

At the beginning of each period the monitor will perform the drawing that determines the color of the disk. This color will then *privately* be communicated to the A participants.

From time to time we will ask the monitor whether we are actually conducting the experiment in the manner specified in the instructions. The monitor will be expected to answer these questions with a simple yes or no. The monitor will also assist us with the experiment. The monitor will not be permitted to communicate with the rest of you in any way.

The monitor is now requested to show the disks to the participants, in order to check whether the probability of a white and a black disk is in accordance with the probabilities stated on the sheet DESCRIPTION ⟨There always was one black disk and either one or two white disks. We also asked the monitor to show the urn and filmcases for the disks and to turn them upside done⟩.

A.2.4. Recording of the results

Now both A and B participants are requested to pick one envelope from the appropriate box and to open the envelope.

Take the enclosed sheet, called RECORD SHEET, and put your registration number in the upper left corner. The registration number is needed to make sure that the right payments are made to the right person. On the next line you see your role (A or B) in this part of the experiment. The table is also projected on the wall.

RECORD SHEET (sample)

Your registration number:
Your role is participant **A**

period	codeletter	the color is		**Your decision** (message)			B's decision		**Your payoff** (earnings minus costs)	B's payoff (earnings)
		white	black	"white"	"black"	no	choice B1	choice B2		
0	a									
1	b									
2	c									
3	d									
4	e									
5	h									
6	f									
7	g									
8	i									
9	j									
10	n									
								your total payoffs for this part:		

Now, first look at the first (left) column of the table. The figures in this column indicate the period, starting with period 0 and ending with period 10. Period 0 is a practice period. The results (earnings) of this period are not included in the payments at the end of the experiment.

The next column (the second from the left) contains a codeletter. This codeletter makes it easier for us to register the results. The codeletter determines the participant in the other role you are matched with in a particular period. Each period you are matched with a different person; furthermore, in periods 1–10 you are matched with the same person at most twice. We have determined the sequence before the experiment in an arbitrary way, so you cannot know whom you are matched with in a any period.

Each period will proceed as follows. After the color has been determined by the monitor's draw, this color will be communicated to the A participants. They will mark this color in the column called 'the color is' of their RECORD SHEET. Then participant A takes her or his decision to send or not to send a message ('white' or 'black') to B. Participant A will mark this decision in the column called 'A's decision (message)'. We will then note this decision on the RECORD SHEET of the appropriate B participant. Participant B will then make her or his decision (choice B1 or choice B2) and mark it in the column 'B's decision'. We will then note this decision on A's sheet and, simultaneously, the monitor will mark the color of the disk in the column 'the color is' of the B participants.

The last two columns concern the payoffs. At the end of each period you will use the tables of the sheet DESCRIPTION to determine your payoffs for that period. These payoffs must be noted in the column called 'your payoffs'. Finally, for the sake of completeness, a column is included in which you are supposed to register the payoffs of the participant with whom you are matched in a particular period. For A participants account must be taken of the costs of a message in case A has decided to send a message in that period.

We will also register all the information.

A.2.5. Summary

Each period begins with the monitor drawing the color of the disk. After the outcome has been communicated to participant A, he or she decides whether or not to send a message to participant B. After this decision has been communicated, it is B's turn to take a decision (choice B1 or choice B2), not knowing the result of the draw (the disk's color). Finally, B's decision is communicated to A and the disk's color to B. With this information the participants determine the payoffs for that period, on the basis of the tables on the sheet DESCRIPTION. The next periods will proceed in exactly the same way until and including period 10.

Thereafter, the sheets for part 2 of the experiment will be distributed, and the new instructions will be read.

A.2.6. Final remarks

At the end of today's session you will be asked to answer some questions for the evaluation of the experiment. After that you will be called by your registration number to privately collect your payoffs in cash at the secretariat. Your payoffs are your own business: you do not have to discuss them with anyone.

It is not allowed to talk or communicate with other participants during the experiment. If you have any questions please raise your hand and one of us will come to your table.

Description ⟨sheet referred to in A.2.2.⟩

(Sample treatment L)

- The urn contains one white and one black disk. Hence, the probabilities that a white is drawn is *2/3* and the probability that a black disk is drawn is *1/3*.
- Participant A decides whether or not to send a message — 'white' or 'black' — before participant B makes her or his choice. A message bears a cost of *f 0.50* to participant A.
- The earnings to participant A (excluding any message cost) and participant B are presented in the tables below. The earnings are dependent upon the disk's color (the color is white or the color is black) and B's decision (choice B1 or choice B2).

Earnings Participant A	Choice B1	Choice B2
The color is white	2	4
The color is black	2	7

Earnings Participant B	Choice B1	Choice B2
The color is white	3	1
The color is black	0	1

A.3. Computerized sessions

The instructions and design of the computerized sessions are identical to those of the pen and paper sessions except for the following:

- All choices were made on, and communicated by, the computer.
- The Description sheet with the payoff tables (see above) was projected on the computer screen throughout the experiment (and on the wall, as in the pen and paper sessions).
- The Record Sheet (see above) was projected on the computer screen.
- In the pen and paper sessions subjects had to calculate their own and their opponent's payoffs. We checked these calculations, which gave us information on whether they understood the game situation. In the computer sessions, the computer calculated the payoffs. To have a check on whether they understood the calculation, subjects had to answer four simple questions concerning the calculation of the payoffs before playing the game. For the same reason — forcing the subjects to look through the payoff tables — two practice periods were held.
- The debriefing questionnaire was per computer.

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