Journal of Economic Behavior & Organization 82 (2012) 39–55

Contents lists available at SciVerse ScienceDirect

Journal of Economic Behavior & Organization

journal homepage: www.elsevier.com/locate/jebo

# Signaling in deterministic and stochastic settings☆

Thomas D. Jeitschko[a,*], Hans-Theo Normann[b,1]

[a] Department of Economics, Michigan State University, United States
[b] Duesseldorf Institute for Competition Economics (DICE), Universitaetsstr. 1, 40225 Duesseldorf, Germany

ARTICLE INFO

*Article history:*
Received 20 June 2011
Received in revised form 2 November 2011
Accepted 1 December 2011
Available online 11 December 2011

*JEL classification:*
C7
C9
D8

*Keywords:*
Experiments
Learning
Noise
Signaling
Stochastic environments

ABSTRACT

We contrast a standard deterministic signaling game with one where the signal-generating mechanism is stochastic. With stochastic signals a unique equilibrium emerges that involves separation and has intuitive comparative-static properties as the degree of signaling depends on the prior type distribution. With deterministic signals both pooling and separating configurations occur. Laboratory data support the theory: in the stochastic variant, there is more signaling behavior than with deterministic signals, and less frequent types distort their signals relatively more. Moreover, the degree of congruence between equilibrium and subject behavior is greater in stochastic settings compared to deterministic treatments.

Published by Elsevier B.V.

## 1. Introduction

Since the seminal work by Spence (1973, 1974), signaling—that is, the costly undertaking of actions in order to either convey or hide private information from others—has become the focus of much research within and beyond economics. In addition to the original work by Spence focusing on education, applications and variations have been seen in industrial organization (for example, entry deterrence through limit pricing (Milgrom and Roberts, 1982) or signaling of product quality (Milgrom and Roberts, 1986; Bagwell and Riordan, 1991)), monetary policy (Backus and Driffill, 1985) and the economics of litigation (Reinganum and Wilde, 1986), to name just a few; and outside of economics insights from signaling have found applications in biology (for example, Zahavi, 1975) and anthropology (Sosis and Ruffle, 2003).

Although these models greatly differ in their approaches and applications, there is one thing they have in common. In the vast majority of signaling games, the signal-generating mechanism is deterministic. The sender is able to perfectly control

☆ The views expressed in this paper are those of the authors and are not purported to reflect the views of the U.S. Department of Justice. We would like to thank Tony Creane, Johannes Hörner, John Kagel, audience members at Seminars at the University of York, Royal Holloway College (University of London), University of Frankfurt, and Max-Planck-Institute Jena, DIW Berlin, Wissenschaftszentrum Berlin; as well as participants at the Midwest Economic Theory Meetings at Michigan State University, the Midwest Mathematical Economics Meetings at Ohio State University, and the Economic Science Association Meetings in Innsbruck for comments. We are grateful to the Max-Planck Institute for Research on Collective Goods (Bonn) for financial support.

* Corresponding author. Present address: Antitrust Division, U.S. Department of Justice, 450 Fifth Street, NW, Washington, DC 20530, United States. Tel.: +1 202 532 4826; fax: +1 202 514 8862.

*E-mail addresses:* thomas.jeitschko@usdoj.gov (T.D. Jeitschko), normann@dice.uni-duesseldorf.de (H.-T. Normann).

[1] Tel.: +49 211 81 15297; fax: +49 211 81 15499.

0167-2681/$ – see front matter. Published by Elsevier B.V.
doi:10.1016/j.jebo.2011.12.002

the signal, and the receiver precisely observes the signal that is sent. The receiver has no trouble interpreting the signal and can therefore correctly infer its cost. That is, there are no inaccuracies in sending or receiving the signal.

Even though standard, the assumption of deterministic signaling is not always plausible. For illustrative purposes, consider a Spence-type education-signaling game in which students signal their (unobservable) ability to potential employers through their choice of an (observable) level of education attained, and suppose that the signal that employers observe is a student's grade-point-average. Problems at the signal-generating stage may occur if, for instance, a student has a "bad day" (or a "good day," for that matter) during an exam. In this case, the sender is only imperfectly able to control the signal. Problems at the receiving end may occur if the employer cannot assess whether the classes taken by the student were particularly easy or hard. Similarly, education will also be a noisy signal for the receiver in a scenario where education is measured by the observable number of years of school attendance but where the education choice is affected by an intrinsic unobservable (dis)utility for education. As the receiver will be unaware of the utility of education, perfect inference of the cost of the signal is no longer possible. In these examples, the signal-generating process is, in effect, stochastic.

Matthews and Mirman (1983) were the first to study such a stochastic setting. They consider a variation of the limit-pricing game introduced by Milgrom and Roberts (1982). In particular, they suppose that the incumbent monopolist chooses an unobservable output level before stochastic demand for the product is realized. This results in an observable but stochastic price which only imperfectly reveals the underlying output choice and hence the type of the incumbent. Another study, Hertzendorf (1993), adds noise to the Milgrom and Roberts (1986) advertising model. Hertzendorf argues that the recipients of advertising signals will only rarely be informed about the exact advertising budget of a company. Instead, people receive a noisy signal of the budget when observing advertisements.

Technically, what happens in stochastic signaling games is that any signal realization is consistent with any action taken by any type whenever the noise perturbing the signal has full support. Thus, signals are no longer invertible and therefore do not allow complete information about the underlying actions of the sender, even when agents of different types undertake different actions in equilibrium (that is, a separating equilibrium). The observable signals only allow incomplete inferences about the sender's true (unobservable) type. In other words, Bayesian updating leads to incremental information dissemination when agents undertake distinct actions, rather than immediate and complete learning.

A main result in noisy signaling games is that often a unique separating equilibrium emerges (Matthews and Mirman, 1983), instead of the large number of possible equilibrium configurations that emerge without noise and that differ quantitatively and qualitatively in deterministic games.[2] A second significant deviation of the equilibrium in noisy signaling games compared to deterministic versions is that the former admit a much richer comparative-statics analysis. In particular, in deterministic games actions are generally independent of the underlying distribution of types within a class of equilibrium configurations so that there are no meaningful comparative statics within a class with respect to prior beliefs. In contrast, in a noisy signaling model the unique equilibrium is sensitive to variations in the underlying distributions, which yields smooth comparative statics with respect to prior beliefs.

In the present paper we consider a sender–receiver signaling game similar to Matthews and Mirman (1983) and Carlsson and Dasgupta (1997) in which a sender chooses from a continuum of actions while the receiver only has two actions. However, in contrast to the former, we follow the latter in assuming a binary type-space rather than a continuum of types. We compare the equilibrium configurations in the deterministic and the stochastic signal-generating mechanisms and provide experimental data for this setup.

The theoretical analysis shows that there are many perfect Bayesian equilibrium constellations in the variant without noise. After the application of equilibrium refinements, one obtains a unique perfect Bayesian equilibrium. Depending on the prior, this unique equilibrium is either pooling or separating. For the stochastic case, even without resorting to refinements, one obtains a unique equilibrium which is separating. Thus, a first hypothesis (testable in the experiments) is that for certain priors pooling behavior should occur without noise as opposed to separating behavior with noise. A second implication of the noisy signaling framework is that players should always signal, that is, always choose actions that differ from their myopically optimal actions. This is in contrast to the deterministic case in which one type always chooses the myopic best action and does not engage in signaling.[3] Often, the impact of noise on players' decisions is ambiguous and depends on the prior beliefs and players' types. This sometimes leads to intriguing comparative-statics predictions that can be tested experimentally. For example, it is the less frequent type who chooses a message that is more strongly distorted away from the sender's myopic best action.

We complement the theoretical analysis with experimental data. Experimental research on signaling games has proven useful is assessing the relevance of the theory. For early contributions see for example Miller and Plott (1985), Brandts and Holt (1992), Potters and van Winden (1996) and Cooper et al. (1997a,b). More recent studies include Cooper and Kagel (2005, 2009) and Kübler et al. (2008). The case for studying a noisy signaling game seems particularly strong given the various propositions that differ markedly from the deterministic environment.

---

[2] Carlsson and Dasgupta (1997) use the stochastic game to suggest an equilibrium-selection criterion for deterministic signaling games when noise vanishes—demonstrating conditions for a unique noise-proof equilibrium to exist in deterministic games.

[3] In a separating equilibrium the "low" type chooses his myopic first-best action, and in a (refined) pooling equilibrium it is the "high" type who chooses his myopic first-best action.

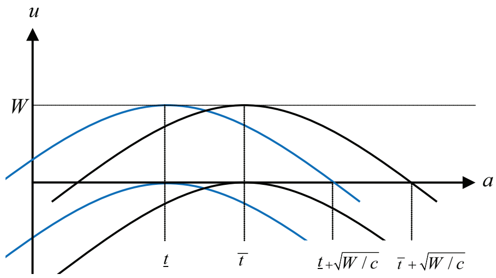


**Fig. 1.** Sender's Payoff (with and without $W$; low type, left ■; high type, right ■).

In our experiments we study treatments with two different priors (a "high" and "low" prior belief for the sender's type). For each of the two priors, we implement a deterministic and a noisy variant. We find that, as in previous experiments, our sessions do not completely converge to equilibrium.[4] Nevertheless, our experimental results provide some clear confirmation of the theory. Regarding the key variables of our experiment (sender and receiver actions), the theory has predictive power. In addition, the hypotheses mentioned in the previous paragraph are supported by the data. Thus, for the high prior, there is more pooling behavior in the deterministic variant; we find indeed that there is significantly more signaling with noise. While there is no support for the hypothesis that the less frequent type signals more,[5] in relative terms, this prediction is confirmed. Overall, the empirical data are closer to their equilibrium counterparts in the stochastic variant compared to the deterministic setting. We attribute this to the fact that noise in the model is similar to imperfect play by subjects leading to a greater congruence between equilibrium observations and subject behavior.

While our study is among the first experiments to analyze a noisy signaling game, de Haan et al. (2011) also construct a model with noisy signals and run experiments. Their model differs from ours in that a pooling equilibrium may exist with noise, because the two sender types have the same first-best preferred action and the marginal cost of signaling is strictly positive. Their main focus is on varying levels of noise, whereas we examine how prior beliefs affect play in noisy and deterministic games. In particular, they find in their data that signaling expenditures increase with the level of noise. For low levels of noise, a separating equilibrium ceases to exist, however, subjects still coordinate on separation in the experiment.

## 2. The model and equilibrium behavior

There are two players who act in sequence. The first player to move is referred to as the sender (of male gender), and the second player is referred to as the receiver (of female gender). Before play begins, nature draws the sender's type. With probability $\rho_0 \in (0, 1)$ the sender is the "high" type, denoted by $\bar{t}$ and with complementary probability of $1 - \rho_0$ he is the "low" type denoted by $\underline{t}$ $(< \bar{t})$.

The sender observes his type and then chooses a hidden/unobservable action $a$ that affects his payoffs both directly and indirectly. The indirect effect comes about because the unobservable action $a$ generates a (possibly noisy) signal $s$ that triggers a payoff-relevant reaction $r$ by the other player, the receiver. Specifically, the sender's (type-dependent) payoff is given by

$$u(a, r) = U - c(a - t)^2 + Wr(s), \quad t \in \{\underline{t}, \bar{t}\}, \tag{1}$$

where $U$ is a normalization parameter, $r \in \{0, 1\}$ is the receiver's response (based on the observed signal $s$), $c > 0$ is a scaling parameter, and $W > 0$ is a windfall gain that the sender obtains when $r = 1$.

The agent's most preferred (myopic best) action is thus $a = t$, and deviations from this (that is, $a \neq t$) entail signaling. Signaling may be undertaken in order to induce the receiver to take a response of $r = 1$, rather than a response of $r = 0$, as $r = 1$ results in the agent obtaining the added windfall payment of $W$. The sender's type-dependent payoff as a function of the action is depicted in Fig. 1, where, in order to observe signaling behavior in the equilibrium of the deterministic setting, we have restricted parameters such that $\bar{t} - \underline{t} < \sqrt{W/c}$.

The receiver does not know the type of the sender. Her prior beliefs are captured by $\rho_0$. These prior beliefs are updated to $\rho_1$ upon observing the signal $s$ on the basis of the relationship between the sender's actions $a$ and the resulting signal $s$, given beliefs about how a sender's type $t$ determines his action.

[4] See Cooper and Kagel (2005, 2009), who convincingly argue that previous experimental signaling games do not immediately converge. Without repetitions or other mechanisms facilitating learning, equilibrium play emerges only gradually, if at all. They show that teams play dramatically more strategically than individuals.

[5] The reason for this failure is that, as is common in signaling-game experiments, the "low" types are much more prone to signaling than are the "high" types.

The receiver's payoffs are affected by her response $r \in \{0, 1\}$ and are given by

$$v(r,t) = \begin{cases} V + Br, & \text{if} \quad t = \bar{t} \\ V + B(1-r), & \text{if} \quad t = \underline{t}, \end{cases} \tag{2}$$

where $V$ is some base-utility and $B > 0$ is a bonus that increases the weight of the decision variable $r$ on the inference that the receiver has drawn about the agent's type. Given posterior belief $\rho_1(s)$, the receiver chooses $r \in \{0, 1\}$ in order to maximize

$$Ev(r) = V + B[\rho_1 r + (1-\rho_1)(1-r)]. \tag{3}$$

Hence, the receiver responds with $r = 0$ whenever $\rho_1 \leq (1/2)$, and chooses the response $r = 1$ otherwise.

We analyze the receiver's decision with switching strategies. That is, the receiver's response is determined by a critical threshold value of the signal $\tilde{s}$ for which

$$r = \begin{cases} 1 & \text{if} \quad s \geq \tilde{s} \\ 0 & \text{if} \quad s < \tilde{s}. \end{cases} \tag{4}$$

Using a switching point makes sense if the receiver thinks facing the higher type is more likely as the signal increases. This indeed occurs in the unique equilibrium of the noisy game, when noise has the monotone-likelihood-ratio property. The deterministic game can also be analyzed with switching strategies, although the game can also be studied without them, in which case the results change only marginally (as discussed below).

It is worth mentioning that compared to the model in Matthews and Mirman (1983) the sender's payoff itself is not subjected to noise in our model, only the signal is. Our model is similar to Carlsson and Dasgupta (1997), but differs in that the receiver's payoff is strictly increasing in properly identifying the sender's type. That is, the receiver obtains $B$ whenever she correctly identifies the sender's type. This is in contrast to many signaling games in which one response can be viewed as a risk-free alternative in that it gives a constant payoff independent of the sender's type (for example, not hiring the worker in Spence's model leads to a reservation payoff that is independent of the true type of the job applicant). Our setup corresponds to a scenario where a manager has to assign a worker to specific tasks within the firm (one requiring higher skills and therefore yielding greater compensation), and where the worker's subsequent performance correctly reveals his or her type in either case and thus may serve as a basis for the payment of the manager.

In summary, the sequence of events is:

1. after nature chooses the sender's hidden type $t \in \{\bar{t}, \underline{t}\}$ with $Pr(t = \bar{t}) = \rho_0$, the sender chooses an unobservable action, denoted by $\underline{a}$ for $\underline{t}$, and $\bar{a}$ for $\bar{t}$;
2. the unobservable action $a$ generates the observable signal $s$, which the receiver uses to update her beliefs about the sender's type upon which she chooses a response $r \in \{0, 1\}$;
3. both players' payoffs are realized according to Eqs. (1) and (2).

The relationship between the sender's action $a$ and the observed signal $s$ depends on whether the signal-generating technology is deterministic or noisy. We analyze the two distinct environments in turn, concentrating on perfect Bayesian equilibrium solutions. In the deterministic setting the set of solutions is further refined, whereas in the noisy setting the perfect Bayesian equilibrium is unique and therefore does not require further refinement arguments.

### 2.1. Equilibrium with deterministic signals

In deterministic models the signal allows a perfect inference about the actions that were taken (that is, the signal-generating mechanism is invertible). Indeed, this is informationally equivalent to a setting in which the action itself is observable. Thus, we consider them to be identical,

$$s \equiv a. \tag{5}$$

Restricting attention to pure-strategy equilibrium configurations, the action taken by the sender is either type-dependent and distinct for the two types (a separating equilibrium), or is independent of his type (a pooling equilibrium).

**Separating configurations**. In a separating equilibrium the receiver infers the sender's type: the low type faces the unfavorable response of $r = 0$, whereas the high type achieves the favorable response of $r = 1$. As $\underline{t}$ faces the unfavorable response he chooses his myopic first best action, that is, $\underline{a}^* = \underline{t}$, since any other action yields a lower payoff when his type is revealed (see Fig. 2 for illustration). Incentive compatibility for the low type requires that he does not find it advantageous to trigger the favorable response of $r = 1$ by choosing the high type's equilibrium action. This implies that $\bar{a}^* \notin [\underline{t} - \sqrt{W/c}, \underline{t} + \sqrt{W/c}]$. Moreover, individual rationality for the high type dictates that he does not prefer to accept the unfavorable response $r = 0$ over taking the equilibrium action in order to obtain the favorable response of $r = 1$. That is,

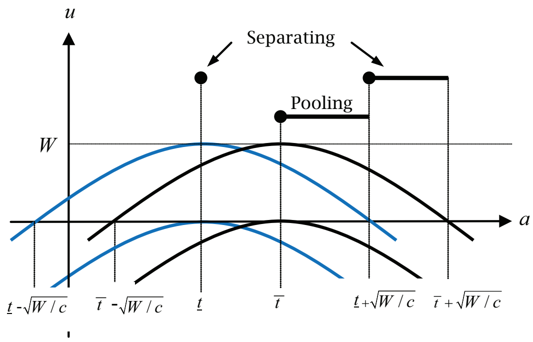


**Fig. 2.** Separating and pooling equilibrium configurations.

$\bar{a}^* \in [\bar{t} - \sqrt{W/c}, \bar{t} + \sqrt{W/c}]$. Taking these considerations together yields a continuum of separating equilibrium constellations $\underline{a}^* = \underline{t}$ and $\bar{a}^* \in [\underline{t} + \sqrt{W/c}, \bar{t} + \sqrt{W/c}]$, with switching point $\tilde{s}^* = \bar{a}^*$.[6]

Using Cho and Kreps' (1987) intuitive criterion, any separating equilibrium with $\tilde{s}^* > \underline{t} + \sqrt{W/c}$ can be upset. Specifically, suppose $\tilde{s}^* = \bar{a}^* > \underline{t} + \sqrt{W/c}$ and consider an out-of-equilibrium action $a \in [\underline{t} + \sqrt{W/c}, \tilde{s}^*)$. Such an action is dominated for $\underline{t}$. Even if the receiver responded with $r = 1$ to such an action, $\underline{t}$ would still be strictly better off choosing $a = \underline{t}$ and getting the $r = 0$ response. Thus, the receiver should believe $\rho_1 = 1$ after such a deviation and then $\bar{t}$ can profitably deviate to $\bar{a}' = \underline{t} + \sqrt{W/c}$. This leaves a unique separating equilibrium, the least-cost separating equilibrium, with $\underline{a}^* = \underline{t}$, $\bar{a}^* = \underline{t} + \sqrt{W/c}$ and $\tilde{s}^* = \bar{a}^*$.

**Pooling configurations**. Note first that when $\rho_0 < (1/2)$ there cannot be a pooling equilibrium. If types pool their actions with $\rho_0 < (1/2)$, the receiver chooses the unfavorable response of $r = 0$. But then both types are no worse off by choosing their myopic best actions, which precludes them taking the same action so that a pooling equilibrium does not exist. With $\rho_0 \geq (1/2)$ and pooling, the receiver chooses the favorable response of $r = 1$, that is, both types get $W$ in equilibrium. Because the receiver employs a switching point, there does not exist a pooling equilibrium with $\tilde{s}^* = \underline{a}^* = \bar{a}^* < \bar{t}$ as $\bar{t}$ could profitably deviate to the myopically optimal $a = \bar{t}$ and still trigger $r = 1$. There is a continuum of equilibrium pooling configurations with $\underline{a}^* = \bar{a}^* \in [\bar{t}, \underline{t} + \sqrt{W/c}]$ (having assumed that $\bar{t} - \underline{t} < \sqrt{W/c}$).[7]

The equilibrium pooling configurations are strictly Pareto-rankable with lower actions strictly preferred by both types of sender. For that reason, the intuitive criterion does not refine the set of equilibrium configurations. (Whenever $\underline{a}^* = \bar{a}^* > \bar{t}$, both types would be better off choosing an out-of-equilibrium action of $a^* = \bar{t}$ if this triggered $r = 1$, thus, the out-of-equilibrium action is not equilibrium dominated and hence configurations with $a^* > \bar{t}$ survive.) However, one can select the efficient pooling equilibrium by applying Grossman and Perry's (1986) perfect sequential equilibrium or Mailath et al.'s (1993) undefeated equilibrium. Thus we obtain a unique pooling equilibrium (which is Pareto efficient) in which $\underline{a}^* = \bar{a}^* = \bar{t}$.[8]

Note finally that when $\rho \geq (1/2)$ the efficient pooling equilibrium with $\underline{a}^* = \bar{a}^* = \bar{t}$ also Pareto dominates the least-cost separating equilibrium from the sender's point of view. Specifically, in this pooling equilibrium $\bar{t}$ has no incentive to separate himself by choosing some action $a > \bar{t}$ since $\bar{t}$ already gets the maximum payoff in the pooling equilibrium. Applying the same equilibrium selection arguments as in footnote 8 leaves the efficient pooling equilibrium as the unique equilibrium if $\rho_0 \geq 1/2$. If $p < (1/2)$, the least-cost separating equilibrium does survive the application of these additional refinements.

We summarize the refined equilibrium constellation for the deterministic case in Proposition 1 and Table 1.

**Proposition 1** (Equilibrium in deterministic settings). *If $\rho_0 < (1/2)$, the unique perfect Bayesian equilibrium surviving equilibrium refinements is the least-cost separating equilibrium with $\underline{a}^* = \underline{t}$ and $\bar{a}^* = \underline{t} + \sqrt{W/c}$. If $\rho_0 \geq 1/2$, the unique perfect Bayesian equilibrium surviving equilibrium refinements is the pooling equilibrium with $a^* = \bar{t}$.*

[6] Note that, because we assume a switching point, all actions $a > \tilde{s}^* = \bar{a}^*$ trigger $r = 1$. Without a switching-point strategy, these signals may induce the response $r = 0$. However, neither type has an incentive to choose $a > \tilde{s}^*$, because this reduces the sender's payoff without affecting the receiver. Hence, the set of separating equilibrium actions is the same with and without a switching-point strategy.

[7] Analyzing the game without switching strategies, the set of equilibrium pooling configurations is larger, namely $\underline{a}^* = \bar{a}^* \in [\bar{t} - \sqrt{W/c}, \underline{t} + \sqrt{W/c}]$, which includes actions below the low-type's most preferred action. However, these additional configurations do not pass the intuitive criterion. We also note already at this point that subjects of either type in the experiment only rarely chose actions $a \in (\underline{t}, \bar{t})$ in the deterministic framework.

[8] Perfect sequential equilibrium (Grossman and Perry, 1986) requires that, for each out-of-equilibrium message, the receiver hypothesizes that the message was sent by some set of types of the sender and revises her beliefs conditional accordingly. If precisely the set of hypothesized players best responds by choosing the out-of-equilibrium message, the original equilibrium is upset. A similar rationale underlies Mailath et al.'s (1993) undefeated equilibrium except that the out-of-equilibrium message must be chosen with positive weight by some sender type in another perfect Bayesian equilibrium. To see the application of these refinements to our game, consider a pooling equilibrium with $\underline{a}^* = \bar{a}^* > \bar{t}$ (given $\rho_0 > (1/2)$). Now assume an out-of-equilibrium action $a \in [\bar{t}, a^*)$. This is profitable for both $\underline{t}$ and $\bar{t}$ whenever the response is $r = 1$. While the equilibrium requires $\rho_1 = 0$ after the deviation, the refinements imply $\rho_1 = \rho_0 > (1/2)$ since the deviation is profitable for both types provided $r = 1$ and both types have an incentive to deviate.

**Table 1**
Refined equilibrium configurations with deterministic signals.

| Equilibrium type | Prior beliefs | |
|---|---|---|
| | $\rho_0 < 1/2$ | $\rho_0 \geq 1/2$ |
| Pooling | – | $\underline{a}^* = \overline{a}^* = \overline{t}$ |
| Separating | $\underline{a}^* = \underline{t}$<br>$\overline{a}^* = \underline{t} + \sqrt{W/c}$ | – |

The predictions in Proposition 1 are based on the application of equilibrium refinements. The literature on the relevance of refinements in experiments (starting with Brandts and Holt, 1992, 1993) has not been conclusive and has not always found support for refinements. Thus, *ex ante*, it appears demanding to consider the above theoretical results as benchmarks for an experiment. Note, however, that in our setting the implications of the refinements are rather modest. They merely give preference to the least-cost separating equilibrium over Pareto inferior separating equilibria, and they select the Pareto efficient pooling equilibrium. The experimental results show that there is support for Proposition 1.

### 2.2. Equilibrium with noisy signals

Consider now the case where the signal that the receiver observes is not invertible and therefore does not reveal the agent's action perfectly. As indicated in Section 1 such noise may result because the sender does not have perfect control over the signal, or it may be that the receiver cannot clearly observe the action. In either case, the receiver must use statistical inference in order to update her beliefs about the action taken, and thus learn about the agent's type.

After we formalize the signal-generating mechanism, we consider the receiver's best response conditioned on her conjectures about the actions taken by the two types of sender. In anticipation of this response the optimal action of the sender is derived. The equilibrium is found by restricting beliefs of the receiver so that they are consistent with the actions taken by the sender.

The signal-generating mechanism is given by

$$s \equiv a + \epsilon, \tag{6}$$

where $\epsilon$ is an unobservable noise term that is distributed independently of $a$ (that is, homoskedastically) according to a normal distribution (which has the monotone likelihood ratio property) with zero-mean and standard deviation of $\sigma$, i.e., $\epsilon \sim N(0, \sigma^2)$, $\forall a$. We assume that the noise term is realized only after the sender has taken the action $a$. Thus, the sender is unable to adjust his actions in light of the realization of noise and, hence,

$$s \sim N(a, \sigma^2).$$

We consider first the receiver's inference problem and best response. Let $\underline{a}^c$ and $\overline{a}^c$, with $\underline{a}^c \neq \overline{a}^c$, denote the receiver's conjectures about which (unobservable) type-dependent actions are taken. As before $\rho_1$ denotes updated (posterior) beliefs. That is, $\rho_1$ is the receiver's subjective probability-assessment that the sender is a $\overline{t}$-type sender, conditional upon having observed the signal $s$, given the conjectures $\overline{a}^c$ and $\underline{a}^c$.

Then, with $f(s|a) = (1/\sigma\sqrt{2\pi})\exp(-((s-a)^2/2\sigma^2))$ denoting the normal density of the distribution of $s$ with mean $a$, Bayes' Rule yields:

$$\rho_1(s|\underline{a}^c, \overline{a}^c) = \frac{\rho_0 f(s|a = \overline{a}^c)}{\rho_0 f(s|a = \overline{a}^c) + (1-\rho_0) f(s|a = \underline{a}^c)} = \frac{LR_0}{LR_0 + \exp((\overline{a}^c - \underline{a}^c)(\overline{a}^c + \underline{a}^c - 2s)/2\sigma^2)}, \tag{7}$$

where $LR_0$ denotes the likelihood ratio of prior beliefs, $\rho_0/(1-\rho_0)$.

Combining (7) with (3) yields the receiver's best response.

**Lemma 1** (Best response). *Given the conjecture $\overline{a}^c$ and $\underline{a}^c$, the receiver's best response is determined by a critical threshold value of $s$ denoted by $\tilde{s}^c$. That is, $r^* = 1$ if and only if $s \geq \tilde{s}^c$ with*

$$\tilde{s}^c = \frac{\overline{a}^c + \underline{a}^c}{2} - \ln(LR_0)\frac{\sigma^2}{\overline{a}^c - \underline{a}^c}.$$

Notice that $\tilde{s}^c$ has several intuitive properties. If $\rho_0 = 1/2$, then $\ln(LR_0) = 0$ and the critical threshold is simply the average of the conjectured actions of the two types of agent. As prior beliefs become strongly biased in favor of one or the other type of sender (that is, $\ln(LR_0) \to \pm\infty$), only extreme signals will lead to updating sufficiently strong to revise prior beliefs to change a response. Similarly, as the sender chooses a similar action regardless of type (that is, we approach a pooling equilibrium, so to speak, and $\overline{a} \approx \underline{a}$), again only extreme signals trigger a response by the receiver that differs from what prior beliefs indicate. Finally, the same holds true for increases in the variance of the noise $\sigma^2$ so that, for given beliefs about the senders' actions, a noisier environment leads to less updating.

Having characterized the receiver's learning and response, consider now the sender's optimal actions. Recall that the receiver's choice of $r$ affects the sender's payoff (see (1)). Since the choice of $r$ is governed by $\rho_1$, which is a function of the sender's action $a$ (see (7)), it is clear that the sender accounts for how $a$ affects $r$. Since $r$ is increasing in $s$, given $\overline{a}^c$ and $\underline{a}^c$, both types of sender have an incentive to increase $s$. That is, both types would like to be identified as being a high type: the high type wants to set himself apart, and the low type wants to deceive.

Thus, given $\overline{a}^c$ and $\underline{a}^c$ (the sender's action affects only the signal $s$, but cannot affect the receiver's conjectures) and given Lemma 1, the sender's (type-dependent) objective is to choose $a$ in order to maximize the value of Eq. (1), *viz.*,

$$U - c(a-t)^2 + WPr(s \geq \tilde{s}^c | a) = U - c(a-t)^2 + W \int_{\tilde{s}^c}^{\infty} \frac{1}{\sigma\sqrt{2\pi}} \exp\left(-\frac{(s-a)^2}{2\sigma^2}\right) ds, \quad t \in \{\underline{t}, \overline{t}\}, \tag{8}$$

where $\tilde{s}^c$ is given in Lemma 1. The (type-dependent) first-order condition is given by

$$2c(a-t) + \frac{W}{\sigma\sqrt{2\pi}} \int_{\tilde{s}^c}^{\infty} \frac{s-a}{\sigma^2} \exp\left(-\frac{(s-a)^2}{2\sigma^2}\right) ds = 0, \quad t \in \{\underline{t}, \overline{t}\}. \tag{9}$$

Hence,

**Lemma 2** (Best action). *Given the receiver's response conditioned on her conjecture $\overline{a}^c$ and $\underline{a}^c$, the sender's best action is implied by*

$$2c(a-t) = \frac{W}{\sigma\sqrt{2\pi}} \exp\left(-\frac{(\tilde{s}^c - a)^2}{2\sigma^2}\right), \quad t \in \{\underline{t}, \overline{t}\},$$

*where $\tilde{s}^c$ is as before, given in Lemma 1.*

**Proof.** Let $g(s, a) = -((s-a)^2/2\sigma^2)$. Then $g_a = -g_s = (s-a)/\sigma$. Hence the term under the integral in the FOC (9) is $-g_s e^{g(s,a)}$ and therefore the integral itself is $e^{g(\tilde{s}^c,a)}$, since $\lim_{s\to\infty} e^{g(s,a)} = 0$. □

Notice that Lemma 2 implies that both types of sender engage in signaling (that is, $a^* > t$ for both types), independent of the receiver's conjectures about the actions taken, provided $\overline{a}^c \neq \underline{a}^c$. This is a reflection of the fact that the marginal gain from signaling is positive, and hence the sender is willing to trade-off deviations of $a$ from $t$ in order to obtain the positive marginal signaling gains. Specifically, the marginal cost of signaling is zero at $t$, whereas the marginal gains are strictly positive. Thus, in the noisy environment, players always signal.

In equilibrium the receiver is aware of the sender's desire to manipulate the flow of information. That is, she is aware that the high type will choose an action in the hopes of distinguishing himself from the low type and, similarly, that the low type will attempt to mimic the high type. As a consequence, she is aware of Lemma 2. This leads to consistent beliefs in which $\overline{a}^c = \overline{a}^*$ and $\underline{a}^c = \underline{a}^*$. Thus,

**Proposition 2** (Equilibrium in stochastic settings). *The equilibrium actions, $\overline{a}^*$ and $\underline{a}^*$, are implied by the equations*

$$2c(\overline{a}^* - \overline{t}) = \frac{W}{\sigma\sqrt{2\pi}} \exp\left(-\frac{1}{2\sigma^2}\left(\frac{\Delta a^*}{2} + \ln(LR_0)\frac{\sigma^2}{\Delta a^*}\right)^2\right), \tag{10}$$

$$2c(\underline{a}^* - \underline{t}) = \frac{W}{\sigma\sqrt{2\pi}} \exp\left(-\frac{1}{2\sigma^2}\left(\frac{\Delta a^*}{2} - \ln(LR_0)\frac{\sigma^2}{\Delta a^*}\right)^2\right); \tag{11}$$

*where $\Delta a^* := \overline{a}^* - \underline{a}^*$. The equilibrium response is given in Lemma 1, with the $\overline{a}^*$ and $\underline{a}^*$ replacing $\overline{a}^c$ and $\underline{a}^c$.*

Notice that if $\rho_0 = 1/2$ so that $\ln(LR_0) = 0$, then both types will deviate from their myopic best actions by exactly the same amount, otherwise the relatively less likely type deviates (that is, signals) more.

## 3. Experimental design and procedures

The experiments were framed as an interaction of a worker and a personnel manager.[9] In the instructions (see online Appendix), subjects were informed about the game as described above. The worker's decision was framed as an effort in a "test" which preceded the employment decision. We made it clear that no real effort had to be invested, and we explained

[9] Because we use labels like manager, worker and effort, our frame is "meaningful." A "generic" frame would have avoided such labels and, instead, presented the game in abstract terms. Cooper and Kagel (2009) analyze cross-game learning in signaling games with meaningful and generic contexts and find that meaningful contexts can strongly support cross-game learning. However, a change in context, together with superficial changes which leave the underlying economic structure isomorphic to the original game leads to reduced learning. Two-person teams show substantially higher levels of strategic play in all of Cooper and Kagel's (2009) treatments.

**Table 2**
Treatments.

| Prior | Noise | |
|---|---|---|
| | $\sigma = 0$ | $\sigma = 5$ |
| $\rho_0 = 1/3$ | *NoNoise.33* | *Noise.33* |
| $\rho_0 = 2/3$ | *NoNoise.67* | *Noise.67* |

how the effort level chosen affected the worker's profit. The payoff information regarding the effort choice was given in a table.

As in the model, personnel managers then received the (deterministic or stochastic) signal derived from workers' effort levels in the test. Next, they had to decide whether or not to employ the worker for some (not specified) task. The descriptions of the payoffs explained that the manager is paid *B* if he or she employs a worker "suitable for the task" or if he or she does not employ a worker who is not suitable for the task. The suitability of the workers was randomly determined by the computer individually and in every period. Workers were paid *W* only if they were employed, in addition to the payoff from the effort choices.

The parameters we used for the experiment were $U = 100$, $V = 0$, $W = B = 100$, $c = 1/2$, $\bar{t} = 50$ and $\underline{t} = 40$. Effort levels had to be chosen from the interval [25, 65]. These parameters yield the payoff table in the instructions (see Appendix).

Our treatment variables are the noise parameter and the prior belief. Specifically, we compare games without noise to those with noise. In the sessions with noise, the noise was normally distributed with $\varepsilon \sim N(0, 5^2)$. Following Ashenfelter et al. (1992), subjects were not given the specific formal details of the normal distribution. Instead, they were given 100 "past realizations" of the noise term and were told that they should expect "similar distortions today" (see Appendix). As for the prior belief, we deliberately ruled out a treatment in which both types are equally likely (which is the traditional set-up in signaling experiments) for two reasons. First, in the deterministic variant of the game a prior of one-half is exactly the threshold for switching from a pooling to a separating equilibrium and we wanted to have unambiguous predictions on behavior; and second, in the noisy variant we were specifically interested in the observation that the less-frequent type signals more, ruling out a prior under which types are equally likely. However, we also wanted to assure that neither type occurs too infrequently in any of the treatments. Thus, it was natural to settle on one prior in which the low type was twice as likely to occur as the high type and the other treatment with the reverse, that is, priors $Pr(t = \bar{t}) = \rho_0$ of 1/3 and 2/3, respectively.

We use the labels *NoNoise.33*, *NoNoise.67*, *Noise.33* and *Noise.67* for the corresponding treatments. Table 2 summarizes the treatment design.

At the end of each period, subjects were given the following feedback: they were informed about the worker's type and the actual effort decision. In sessions with noise, they were also told the noisy signal that the personnel manager received. Further, they were reminded of the personnel manager's decision and were given the resulting payoffs of both players.

We decided to allow for many repetitions because learning is necessary in such complex situations—a supposition that is borne out by the data. Our experiments had a length of 40 periods.

Subjects were randomly rematched in every period in order to create an environment as close as possible to a single-period interaction between subjects. In each session, 20 subjects participated. The matching scheme was such that subjects interacted within a group of ten subjects. The rationale for this matching scheme is to generate more group-level observations that are independent; each session of 20 subjects thus consists of two independent groups.[10] We have two sessions (40 participants) for each treatment, generating four independent groups per treatment. We used the same matching protocol in all sessions.

We applied role switching in this experiment. That is, participants acted both in the role of the worker and in the role of the personnel manager. Roles were switched every five periods, so all participants played either role four times for five periods. Many signaling experiments employ role switching; see Brandts and Holt (1992, 1993), Cooper et al. (1997a,b), Cooper and Kagel (2005, 2009), Kübler et al. (2008) and Potters and van Winden (1996). One motive sometimes given in favor of role switching is that it may enhance learning because subjects may better understand the decision problem of the other players and therefore the overall game if they play in both roles.

Experiments were computerized. We used *z-Tree*, developed by Fischbacher (2007). Sessions were conducted at *BonnEconLab*, the University of Bonn's Experimental Economics Laboratory. In total, 160 subjects participated in eight sessions.

Sessions lasted between 60 and 75 min, including the time for reading the instructions, playing the experiment, answering a post-experimental questionnaire and paying the subjects. Earnings were denoted in "points." The exchange rate of one euro for 500 points was known. Subjects also received a show-up fee of four euros. Average earnings were about 13 euros, including the show-up fee.

[10] It should be noted, however, that this can have the undesirable impact of making the within-group correlation greater since individuals within a matching group interact more frequently than if groups were formed from all session participants.

**Table 3**
Equilibrium constellations given the experiment parameters (figures are rounded).

| | *NoNoise* | *Noise* |
|---|---|---|
| $\rho_0 = 1/3$ | $\underline{a}^* = 40.0$, $\overline{a}^* = 54.1$, $\Delta a^* = 14.1$, $\underline{e}^* = 0$, $\overline{e}^* = 1$, $e^* = 0.33$, $\tilde{s}^* = 54.1$ | $\underline{a}^* = 42.5$, $\overline{a}^* = 55.0$, $\Delta a^* = 12.5$, $\underline{e}^* = 0.06$, $\overline{e}^* = 0.83$, $e^* = 0.32$, $\tilde{s}^* = 50.2$ |
| $\rho_0 = 2/3$ | $\underline{a}^* = 50.0$, $\overline{a}^* = 50.0$, $\Delta a^* = 0.0$, $\underline{e}^* = 1$, $\overline{e}^* = 1$, $e^* = 1$, $\tilde{s}^* = 50.0$ | $\underline{a}^* = 48.0$, $\overline{a}^* = 54.0$, $\Delta a^* = 6.0$, $\underline{e}^* = 0.49$, $\overline{e}^* = 0.88$, $e^* = 0.75$, $\tilde{s}^* = 48.1$ |

## 4. Hypotheses

Given the experimental parameters ($U = 100$, $V = 0$, $W = B = 100$, $c = 1/2$, $\overline{t} = 50$ and $\underline{t} = 40$), we obtain the equilibrium benchmarks given in Table 3. Recall that $\overline{a}$ and $\underline{a}$ refer to $\overline{t}$'s and $\underline{t}$'s effort choices, respectively, and $\Delta a := \overline{a} - \underline{a}$ is the effort difference. Employment rates are denoted by $\overline{e}$ and $\underline{e}$, and the average employment rate is $e := \rho_0\overline{e} + (1 - \rho_0)\underline{e}$. Finally, $\tilde{s}$ denotes the switching point, that is, the signal above which employers choose $r = 1$. Equilibrium values are indicated by an asterisk ($^*$) throughout.

Table 3 reveals that the predictions about the effects of our treatments are not always unambiguous. Consider, for example, the impact of noise on $\underline{a}$. In *Noise.33*, $\underline{a}$ should be higher than in *NoNoise.33*, but it is exactly the other way round in *Noise.67* and *NoNoise.67*. For $\overline{a}$, the impact of noise is different again. Instead of unambiguous hypotheses, the impact of noise often depends on the prior (or *vice versa*) in theses cases. In what follows, we accordingly present hypotheses in the form of ordinal rankings of the relevant variable across all four treatments. We then use the non-parametric Jonckheere–Terpstra test,[11] testing the null hypothesis that all treatments come from the same distribution against the predicted ranking of treatments.

We start with sender behavior. The first hypotheses are on the effort levels chosen by the low type $\underline{a}$, by the high type $\overline{a}$, and on the effort difference $\Delta a = \overline{a} - \underline{a}$. All of the hypotheses are obtained from Table 3.

**Hypothesis 1** *(Sender's effort choice).* Concerning the senders' type-dependent equilibrium effort choices, the following rankings hold:

(a) for the low-type's action $\underline{a}^*$: *NoNoise.33 < Noise.33 < Noise.67 < NoNoise.67*;
(b) for the high-type's action $\overline{a}^*$: *NoNoise.67 < Noise.67 < NoNoise.33 < Noise.33*;
(c) for the difference in actions $\Delta a^*$: *NoNoise.67 < Noise.67 < Noise.33 < NoNoise.33*.

A general implication of noise is as follows.

**Hypothesis 2** *(Signaling with noise).* In the noisy treatments, senders should always signal, that is, they should always choose $a > t$.

For the treatments with noisy signaling, we have an intriguing hypothesis which we already noted following Proposition 2.

**Hypothesis 3** *(Signaling distortions with noise).* The sender whose type is less likely under the prior beliefs engages in more costly signaling efforts, that is, $\underline{a} - \underline{t} < \overline{a} - \overline{t}$ in Noise.33, and $\underline{a} - \underline{t} > \overline{a} - \overline{t}$ in Noise.67.

We now turn to the receiver's behavior. Table 3 contains the data for the type-dependent employment rates and also for the average employment rates per treatment.

**Hypothesis 4** *(Employment rates).* Concerning the senders' type-dependent equilibrium employment probabilities, the following rankings hold:

(a) for the low type's employment rate $\underline{e}^*$: *NoNoise.33 < Noise.33 < Noise.67 < NoNoise.67*;
(b) for the high type's employment rate $\overline{e}^*$: *Noise.33 < Noise.67 < NoNoise.67 = NoNoise.33*;
(c) for the average employment rate $e^*$: *Noise.33 < NoNoise.33 < Noise.67 < NoNoise.67*.

We finally turn to the receiver's equilibrium choice of switching points.

**Hypothesis 5** *(Switching points).* Regarding the receiver's switching points, $\tilde{s}^*$, the following ranking holds: *Noise.67 < NoNoise.67 < Noise.33 < NoNoise.33*.

[11] The Jonckheere–Terpstra test is a non-parametric test for more than two independent samples, like the Kruskal–Wallis test. Unlike Kruskal–Wallis, Jonckheere–Terpstra tests for ordered differences between treatments and hence requires an ordinal ranking of the test variable. See, for example, Hollander and Wolfe (1999).

**Table 4**
Effort levels (equilibrium values in italics, standard deviation in parenthesis).

| Prior | NoNoise | | Noise | |
|---|---|---|---|---|
| | $\underline{t}$ | $\bar{t}$ | $\underline{t}$ | $\bar{t}$ |
| 1/3 | *40.00* | *54.14* | *42.50* | *55.00* |
| | 44.34 | 51.57 | 45.19 | 52.77 |
| | (1.41) | (1.40) | (0.91) | (0.49) |
| 2/3 | *50.00* | *50.00* | *48.00* | *54.00* |
| | 46.93 | 50.96 | 45.67 | 51.50 |
| | (1.96) | (0.92) | (1.72) | (1.19) |

## 5. Results of the experiments

Section 5 is structured as follows. We begin with an analysis of the worker (sender) behavior. Then we move on to the managers (receivers), before analyzing workers and managers jointly to see how they respond to one another's actual behavior.

We usually employ non-parametric tests where we (conservatively) count one group of randomly matched participants as one observation. That is, we usually take the average action of all participants in one group as one observation (recall that we have four entirely independent observations per treatment). Whenever we depart from this, we indicate how we deal with the possible non-independence of observations. Thus, we generally test directed hypotheses and report one-sided $p$-values, accordingly—except for the (unpredicted) time trends where we report two-sided $p$-values.

In signaling game experiments, it is common for learning to take place among subjects (see, for example, Cooper and Kagel, 2005, 2009). Therefore, our main data analysis is based on data from periods after learning has settled, that is, the second half of the experiment. We report on learning effects (which justify this selection) below.

### 5.1. Worker (sender) behavior

Table 4 and Fig. 3 summarize the effort choices across the four treatments. Table 4 reports average effort choices and their standard deviation for the group averages. It also states the equilibrium benchmarks. Fig. 3 displays the cumulative distribution functions of choices by types and treatment.

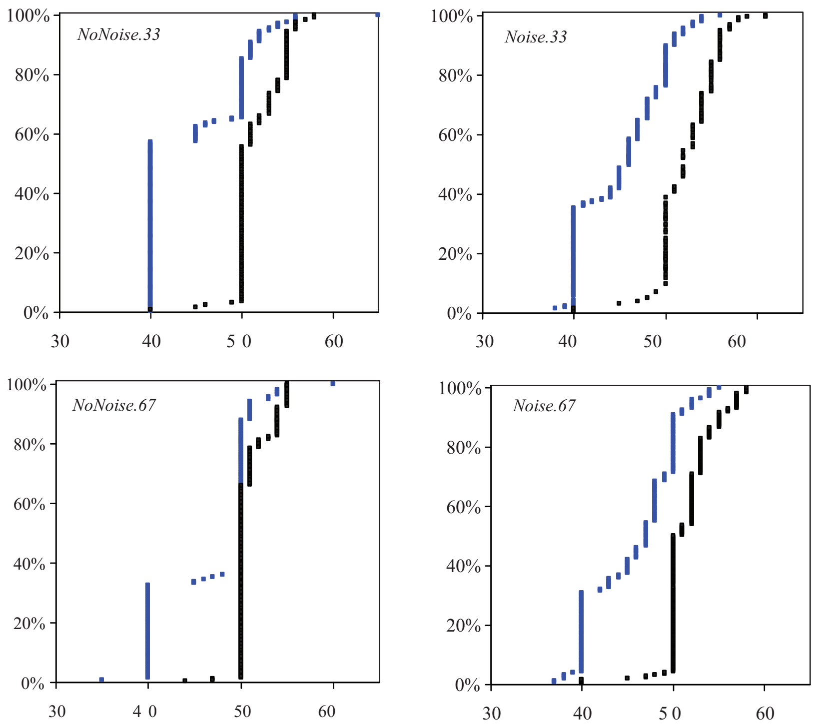


**Fig. 3.** CDFs of effort choices (low type, left ■; high type, right ■).

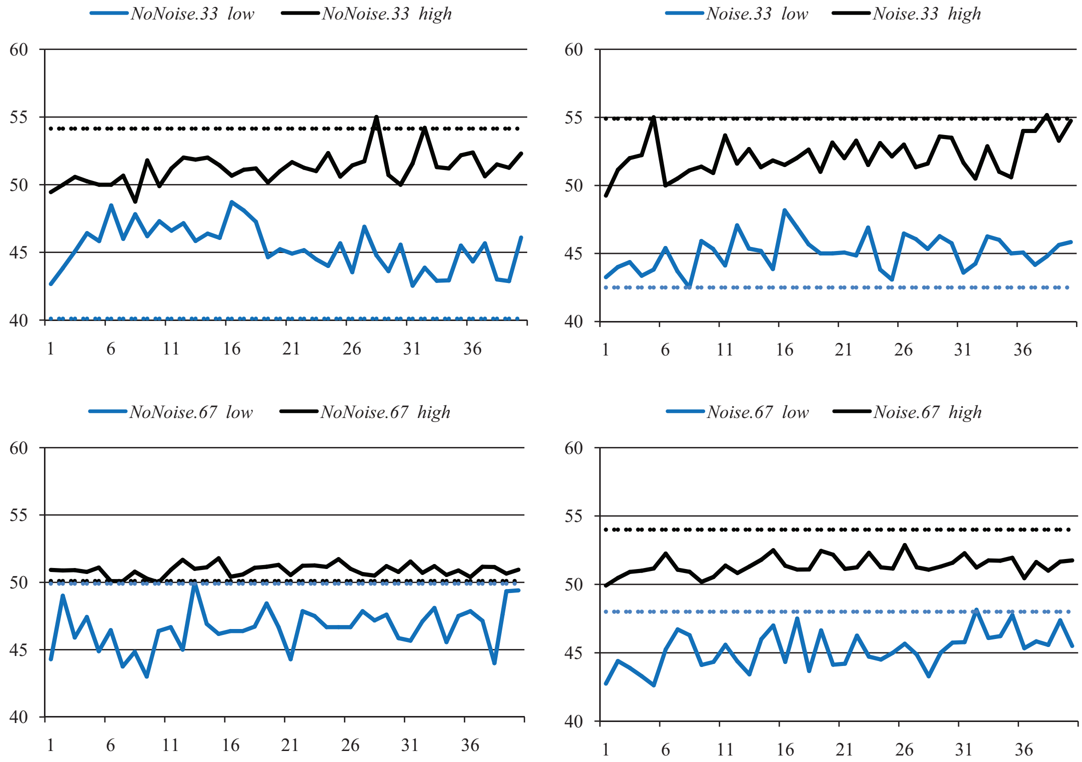


**Fig. 4.** Effort over time (low type, bottom ■ ; high type, top ■); equilibrium, dotted lines).

As in previous signaling games, choices do not perfectly settle on the equilibrium benchmarks even in the second half of the experiment, as can be seen in Table 4. Fig. 3 also indicates that there is no complete separating or pooling behavior in any treatment. Frequently, workers choose their myopic best action. While this is consistent with equilibrium behavior for one type in the *NoNoise* treatments, the frequency is nowhere near 100 percent for any type in any treatment. Another general observation is that the average signaling distortions (that is, the $a - t$ margins) of the $\underline{t}$ types are larger than those of the $\bar{t}$ types in all treatments. Whereas $\bar{t}$ generally provide too little effort compared to the equilibrium benchmark (except in *NoNoise.67*), $\underline{t}$ provide too much effort with the low prior and too little effort with the high prior. Indeed, this is true right from the beginning, period 1.[12],[13]

Fig. 4 shows the time trends of the effort decisions for each treatment and type. The pattern in *NoNoise.33* is typical for a signaling experiment where separating behavior emerges (see, for example, Cooper and Kagel, 2009): first, $\underline{t}$ mimic $\bar{t}$'s effort levels; this appears to trigger the high types to increase their efforts, which eventually leads to a decline of low's efforts, with behavior having settled down for the second half of experiment. This is in stark contrast to *NoNoise.67*, in which pooling is predicted and no major time trends appear. In the treatments with noise, we observe increasing effort levels in the first half of the experiment but no tendencies over time in the second half (except for the effort increase of $\underline{t}$ in *Noise.67*, see footnote 14). While we do recognize separating behavior in both *Noise* treatments, it is also apparent that the development over time is different from the pattern in *NoNoise.33*. We thus conclude some tentative support for our setup from Fig. 4 in that both of our treatment variables seem to have the predicted effect, that is, pooling *vs.* separating in *NoNoise*, and qualitatively different separating behavior with both types signaling in *Noise*.[14]

[12] The excess signaling of $\underline{t}$-types may correspond to the phenomenon of over-investments in contests (for example, Fonseca, 2009; Sheremeta, 2010). It appears that subjects receive an extra utility from winning a contest. In our setup, getting employed is pretty much a forgone conclusion for high types, whereas low types may be willing to invest an excess effort for the sole purpose of getting employed.

[13] In period 1 (where all effort choices are still completely independent), 30 of 44 high types choose $a = \bar{t} = 50$ but only 14 of 36 low types $a = \underline{t} = 40$. A chi-square test indicates that the difference in proportions is significant ($d.f. = 2$, $p = 0.009$). Similarly, the 95 percent confidence interval of period-1 actions for the high type, [49.01, 50.94], includes the myopic best action, 50, whereas the confidence interval for $\underline{t}$, [41.54, 44.85], does not include 40. See also Fig. 4.

[14] We examine the correlations between experimental time periods and effort levels. In periods 1–20 three of eight possible correlations (two types × four treatments) were significant, whereas in periods 21–40 only one correlation was significant. In the first half of the experiment, $\bar{t}$-types increase their efforts over time in *NoNoise.33* (Spearman's $\rho = 0.526$, $p = 0.017$), *NoNoise.67* ($\rho = 0.390$, $p = 0.090$) and *Noise.67* ($\rho = 0.606$, $p = 0.005$). In the second half of the experiment, $\underline{t}$-types increase their efforts over time in *Noise.67* ($\rho = 0.496$, $p = 0.026$).

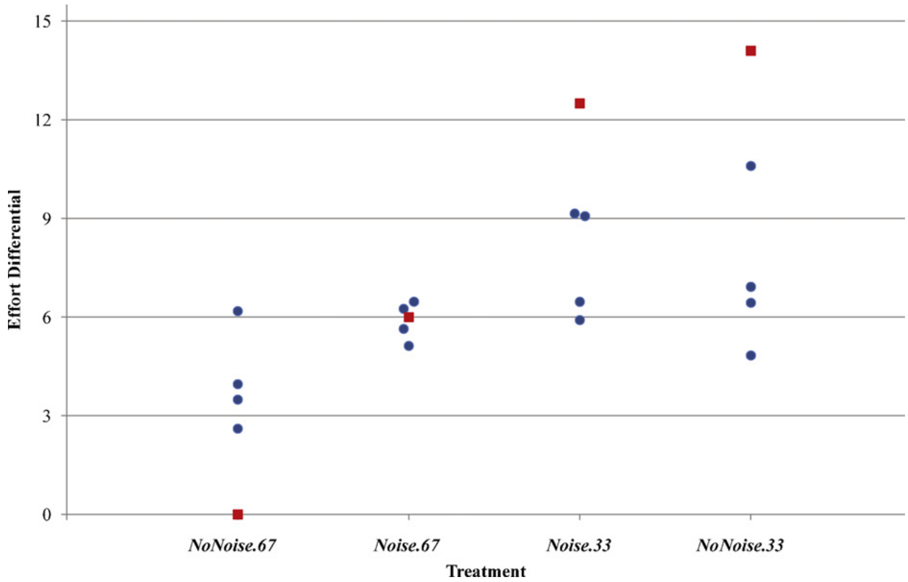


**Fig. 5.** Effort differentials $\Delta a = \bar{a} - \underline{a}$ (equilibrium: squares ■ ; group data: bullets ● ).

We now formally test our hypotheses. It turns out that while effort decisions do not perfectly converge, there are a number of observations that are consistent with the equilibrium benchmarks.

Table 4 shows that the ranking of effort averages across the four treatments is consistent with the theory. For $\underline{t}$, recall from Hypothesis 1(a) that the lowest effort benchmark (namely 40.0) should occur in *NoNoise.33*, next comes *Noise.33* with a level of 42.5, followed by *Noise.67* (48.0) and the highest effort levels for $\underline{t}$ (50.0) should occur in *NoNoise.67*. The actual averages are ranked precisely in this way. A Jonckheere–Terpstra test rejects the null hypothesis (that effort averages of the $\underline{t}$ type workers are drawn from the same distribution) at $p = 0.016$ in favor of the alternative Hypothesis 1(a). Conducting the Jonckheere–Terpstra test for the ranking of effort choices by the $\bar{t}$ types yields a similar result ($p = 0.031$), rejecting the null in favor of Hypothesis 1(b).

Now consider the effort differential, $\Delta a = \bar{a} - \underline{a}$. The effort differential is the amount of separation between the types and therefore tells us something about the important pooling *vs.* separating issue. The impact of noise on this variable is ambiguous in theory. There is less separation with noise for the low prior but more separation with noise for the high prior. From Hypothesis 1(c), the theoretical benchmark for $\Delta a^*$ (ranked in ascending order) is $\Delta a^* = 0.0$ in *NoNoise.67* (the pooling case), 6.0 in *Noise.67*, 12.5 in *Noise.33* and finally 14.1 in *NoNoise.33*. Fig. 5 shows the average amount of separation between types for each group and the theoretical benchmark. The picture shows that the theory works well in organizing the data. The ranking of the group averages by treatment is the one predicted except that *Noise.33* has a marginally higher average than *NoNoise.33*. Applying the Jonckheere–Terpstra test on the average effort differentials yields a highly significant rejection of the null hypothesis ($p = 0.006$), therefore providing support for Hypothesis 1(c).

Part of Hypothesis 1(c) is the proposition that with $\rho_0 = 2/3$ pooling ($\Delta a^* = 0.0$) should occur without noise but separating ($\Delta a^* = 6.0$) with noise. This is an intriguing hypothesis which can be tested directly in a pairwise comparison of these treatments. A ranksum test confirms that the effort differential is smaller in *NoNoise.67* than in *Noise.67* ($p$=0.057, exact test). The CDFs in Fig. 3 also provide evidence in this direction. For *NoNoise.67*, Fig. 3 reveals that the pooling equilibrium effort level of 50 is the mode (60.25 percent). This is true for both types as $\underline{t}$ workers choose $a = 50$ in 51.5 percent and $\bar{t}$ types in 64.8 percent of the cases. The frequency of $a = 50$ effort choices is significantly smaller in *Noise.67* and indeed in all of the other three treatments where $a^* = 50$ should not occur in equilibrium (pairwise comparisons with exact rank-sum tests, all $p = 0.029$ or smaller). This is support for the pooling *vs.* separating hypothesis with $\rho_0 = 2/3$. By contrast, if $\rho_0 = 1/3$ there should be separation both with and without noise and the prediction regarding the effort differential is 12.5 and 14.1 for *Noise.33* and *NoNoise.33*, respectively. As there is separation either way and since the predicted effort differentials do not differ much, unsurprisingly, *Noise.33* and *NoNoise.33* do not differ significantly.[15]

Hypothesis 2 suggests that workers should always signal (that is, choose $a > t$) in treatments with noise. Specifically, $\underline{t}$ should choose $a > 40$, and $\bar{t}$ should choose $a > 50$ with noise. Whereas the CDFs show that in many cases workers do actually

[15] For the sake of completeness, the third pairwise comparison of Hypothesis 1(c), *NoNoise.67 vs. NoNoise.33*, confirms the theory in that we have a significant effect ($p = 0.057$). Comparing Hypotheses 1(a) and (b) pairwise does not yield significant test results. Thus, while the Jonckheere–Terpstra *omnibus* test rejects the null hypothesis in these cases (Hypotheses 1(a) and (b)), we cannot identify a pair of treatments which (mainly) accounts for this rejection.

**Table 5**
Employment rates (equilibrium values in italics, standard deviation in parenthesis).

| Prior | *NoNoise* | | *Noise* | |
|---|---|---|---|---|
| | $\underline{t}$ | $\bar{t}$ | $\underline{t}$ | $\bar{t}$ |
| 1/3 | *0.000*<br>0.232<br>(0.030) | *1.000*<br>0.609<br>(0.066) | *0.060*<br>0.272<br>(0.036) | *0.833*<br>0.609<br>(0.041) |
| 2/3 | *1.000*<br>0.470<br>(0.057) | *1.000*<br>0.864<br>(0.064) | *0.490*<br>0.565<br>(0.078) | *0.880*<br>0.834<br>(0.056) |

choose their myopic best actions, they also show that $\underline{a} = 40$ and $\bar{a} = 50$, respectively, are selected less frequently in the stochastic-signal treatments. Statistical support for this can be obtained by collecting the share of effort choices strictly larger than the (type-specific) myopic best action for each group. In the eight groups of the noisy treatments, 62.8 percent of the workers' actions have $a > t$ but this is only the case in 47.8 percent of the deterministic treatments. A ranksum test reveals that this difference is significant ($p = 0.023$). Note that we obtain this significant result even though one type is predicted to choose $a > t$ also in the *NoNoise* treatments.

We can also check Hypothesis 2 for $\underline{t}$ only. In the *Noise* treatments, $\underline{t}$'s equilibrium actions lie strictly between 40 (=$\underline{t}$) and 50 (=$\bar{t}$). By contrast, in the *NoNoise* treatments, the equilibrium actions for $\underline{t}$ are 40 and 50, and moreover the worker is (at least in theory) revealed as being $\underline{t}$ when choosing $a \in (40, 50)$. With noise, effort choices between 40 and 50 generate signals that only imperfectly reveal the worker as $\underline{t}$. These considerations are confirmed in the data. The CDFs show that 39.04 percent of the $\underline{t}$ observations are in the $a \in (40, 50)$ interval with noise but, without noise, only 5.58 percent are. This difference is significant (rank-sum test, $p < 0.001$). The result suggests that subjects clearly understood the noisy signal-generating mechanism.

Finally, for the noise treatments, Hypothesis 3 states that signaling distortions $(a - t)$ are larger for the less frequent type. At face value, this hypothesis is clearly rejected. As mentioned above (and as in other signaling experiments), the $\underline{t}$ types distort more in all treatments compared to the $\bar{t}$ types, and we find $\underline{a} - \underline{t} > \bar{a} - \bar{t}$ in all groups of all treatments. However, in *relative* terms, the prediction is supported. From Table 4, note that the $\underline{t}$ types distort more with the high prior whereas the $\bar{t}$ types distort more for the low prior. Testing this formally, the ratio $(\underline{a} - \underline{t})/(\bar{a} - \bar{t})$ is significantly smaller in *Noise.33* than in *Noise.67* (ranksum test, $p = 0.042$). Interpreting the predictions in relative rather than absolute terms (which seems warranted as the $\bar{t}$ types signal too little anyway, right from the first period on), we find support for Hypothesis 3.

### *5.2. Manager (receiver) behavior*

Table 5 shows how frequently managers employ the workers. Compared to the equilibrium benchmark, $\bar{t}$ is employed too rarely, and $\underline{t}$ is employed too often. This finding is not particularly surprising given the above result that low types usually signal too much and high types sometimes too little. The hypothesis regarding $\underline{t}$, Hypothesis 4(a), turns out to be supported by the data in that we reject the null hypothesis (Jonckheere–Terpstra, $p = 0.003$). That is, even though quantitatively the predictions fail, the theory still yields a useful qualitative prediction regarding $\underline{e}$. Regarding the ranking of $\bar{e}$ (the employment rates of $\bar{t}$), we cannot reject the null hypothesis that all treatments are drawn from the same distribution (Jonckheere–Terpstra, $p = 0.245$), that is, we find no support for Hypothesis 4(b).[16]

Fig. 6 shows that average employment rates across the two types, $e$, meet the equilibrium benchmarks rather accurately in three of the four treatments. That is, regarding average employment rates per treatment, the theory works well even in a quantitative sense. (The exception is *Noise.67*.) A Jonckheere–Terpstra test on the average employments rates confirms that the ranking we observe rejects the null hypothesis ($p = 0.004$). This significant test result is in favor of Hypothesis 4(c). From Fig. 6, it is evident that the differences are mainly driven by the prior rather than by noise.[17]

Employment decisions obviously depend on workers' effort levels. Fig. 7 shows the likelihood of getting employed as a function of the effort signals. The probabilities are obtained from simple probit regressions where the response decision $r \in \{0, 1\}$ is a function of $s$, the signal received. For each treatment there is a separate regression. The probits are based on data from periods 21 to 40 and are clustered at the matching-group level (Wooldridge, 2003). As expected, the probability of an $r = 1$ choice increases with the received signal $s$. Indeed, in all treatments, most of the increase occurs between $s = 40$

[16] Correlations over time for the employment decisions are as follows. $\bar{t}$-types are employed less frequently over time in *NoNoise.33* (Spearman's $\rho = -0.488$, $p = 0.029$), and more frequently in *Noise.67* ($\rho = 0.419$, $p = 0.069$). In periods 21–40, $\bar{t}$-types' employment rates decrease over time in *NoNoise.67* ($\rho = -0.472$, $p = 0.036$). Time trends are perhaps less conclusive for the employment rates than for the effort decisions as employment rates can be quite volatile. The reason is that, depending on the chance move, the less frequent type ($\bar{t}$ [$\underline{t}$] in the *.33* [*.67*] treatments), may occur only rarely in some period for some treatment (the minimum we find is two instances). Thus employment rates may vary substantially, indeed, in the *Noise* treatments this is predicted to occur in equilibrium.

[17] Indeed, if we compare our treatments pairwise, we find significant test results when we compare *.33* and *.67* treatments (ranksum tests, all $p = 0.0105$) but not when we compare *NoNoise* to *Noise* treatments. The results of these pairwise tests are the same for Hypothesis 4(a)–(c).

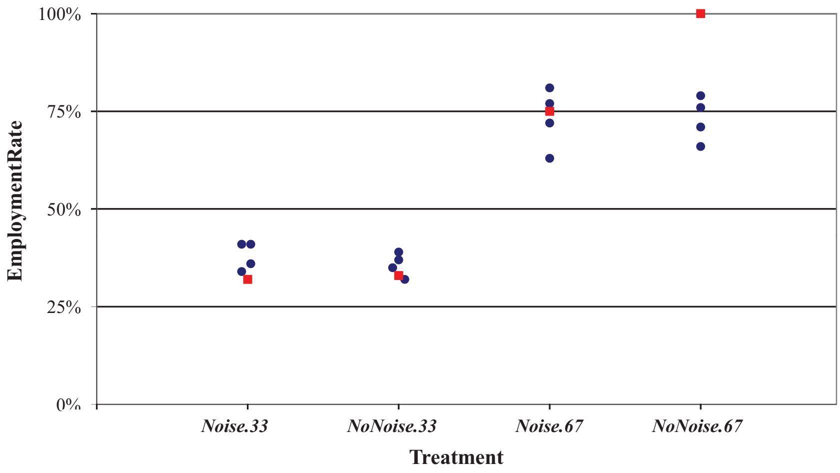


**Fig. 6.** Employment rates (equilibrium: squares ■ ; group data: bullets ● ).

and $s = 55$. In all four cases, both the constant and the marginal effects of the effort signal are significant at $p < 0.001$ . All regressions are highly significant with the pseudo $R^2$ varying between 0.190 (*Noise.67*) and 0.413 (*NoNoise.33*).

There are two further intuitive observations from Fig. 7. First, the employment likelihood is higher with $\rho_0 = 2/3$ . Both with and without noise, $r = 1$ responses are more likely with the high prior. As can be seen in Fig. 7, the $\rho_0 = 2/3$ treatments first-order stochastically dominate those with $\rho_0 = 1/3$ . Second, the figure shows that the curves in the noise treatments are flatter than their *NoNoise* counterparts for the middle range of effort choices. For the $\rho_0 = 1/3$ prior, $\Pr(r = 1)$ is larger in the noise treatment for $s \leq 49$ and smaller otherwise. Similarly, for the $\rho_0 = 2/3$ prior, $\Pr(r = 1)$ is larger with noise as long as $s \leq 55$ and smaller otherwise. This is intuitive. A signal of, say, 45 is rather unlikely to have been sent by $\bar{t}$ without noise. With noise, there is some chance $\bar{t}$'s choice was distorted negatively to the level of 45. The reverse is true for choices larger than, say, 50 and 55, respectively. Such high choices are almost surely sent by $\bar{t}$ when there is no noise. With noise, there is still the chance that noise caused the high signal and thus managers are less likely to choose $r = 1$ compared to *NoNoise*, given the same effort choice.

Probit regressions (not reported here to economize on space) reveal that these two effects are significant. A dummy for the high prior has a significant impact on the likelihood of getting employed whereas a *Noise* regressor *per se* is positive but insignificant. Only when we include interaction terms does *Noise* become significant. The *Noise*×*Effort* interaction is negative and significant.

We also use probit regressions to test Hypothesis 5 which is on the switching strategies. Specifically, we run the probits mentioned in the previous paragraph above separately for each group and calculate the median accepted effort choice for each group, that is, the effort choice under which there is a 50 percent probability of being employed. We compare these median threshold effort levels (one for each group, four for each treatment) to the ranking of switching points given in Hypothesis 5. (If 100 percent of our subjects behaved consistently with the theory, they would reject every signal below the switching point and employ for every signal above that point, and thus the median signal that results in employment would be equal to the predicted switching point.) A Jonckheere–Terpstra rejects the null hypothesis at $p = 0.001$, supporting the

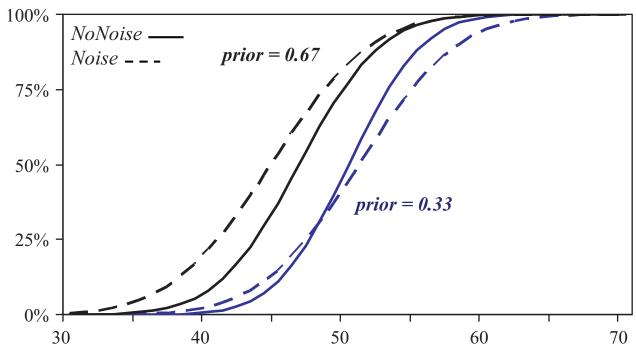


**Fig. 7.** Probability of employment as a function of the signal.

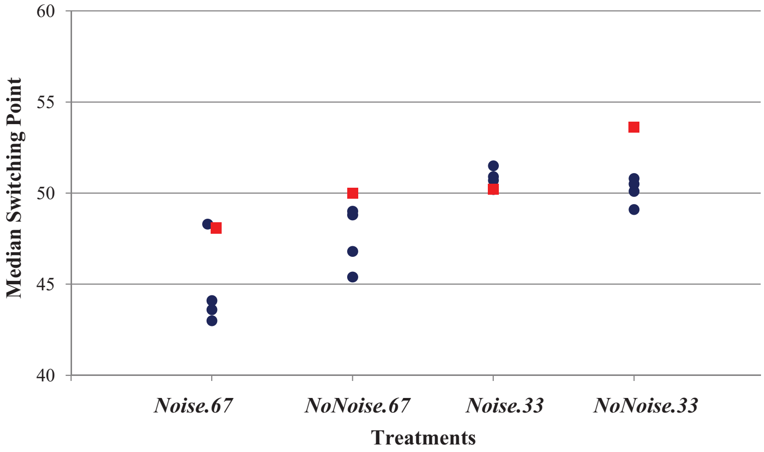


**Fig. 8.** Switching points (equilibrium: squares ■; group medians: bullets ●).

**Table 6**
Optimal effort choices given employment decisions (and difference between actual average effort and optimal choices).

| Prior | NoNoise | | Noise | |
|---|---|---|---|---|
| | $\underline{t}$ | $\bar{t}$ | $\underline{t}$ | $\bar{t}$ |
| 1/3 | 40.8<br>+3.5 | 54.9<br>−3.3 | 42.0<br>+3.2 | 55.2<br>−2.4 |
| 2/3 | 48.0<br>−1.1 | 53.2<br>−2.2 | 46.6<br>−0.9 | 52.6<br>−1.1 |

ordinal ranking in Hypothesis 5.[18] Fig. 8 shows the empirical median switching points per group in conjunction with the equilibrium switching points.

### 5.3. How do players respond to others' empirical behavior?

Above, we saw that behavior does not seem to converge fully to equilibrium benchmarks. On the other hand, the support for some implications of the theory indicates that play is far from erratic. This raises the question of how subjects respond to the actual empirical behavior of the other subjects.

We first analyze how workers' actions correspond to the employment decisions. To do this, we determine the optimal effort level given managers' actual employment decisions in periods 21–40, separately for each treatment and type. Specifically, we use the acceptance probabilities from the probit regressions underlying Fig. 7 and calculate workers' expected payoffs from this. For some effort level $a$,

$$100 - \frac{1}{2}(a - t)^2 + Pr(r = 1|a) \times 100$$

is $t$'s expected payoff.

The main insight from this exercise is that average effort choices sometimes do not differ much from the value that maximizes expected payoffs and, whenever they do differ, this can be explained by "flat" expected payoff maxima. Table 6 shows the results.

There are eight cases, one for each treatment and each type. For example, the first entry (top left) indicates that, given the empirical receiver behavior in the second half of the experiment, $\underline{t}$'s optimal choice in *NoNoise.33* was 40.8, yet the actual average choice was 3.5 higher (44.3) than the optimal choice of that type in that treatment (where the actual averages are obtained from Table 4). In three of eight cases, the (absolute) differences are 1.1 or less, suggesting a certain coincidence of optimal and actual average effort choices. Moreover, note that larger differences (say, three or more effort units) are subject to the disclaimer that differences in expected payoffs are truly minor. In *Noise.33*, the payoff loss from not playing optimally is merely 1.35 percent and 1.75 percent in *NoNoise.33*, for low and high types, respectively. The biggest loss in expected payoff (5.6 percent) occurs for $\bar{t}$ in *Noise.33*. The fact that deviating from optimal behavior causes only minor losses of expected payoffs suggests that the discrepancy of optimal and actual average effort choices, if they occur at all, should be

[18] Testing pairwise, only the comparison of treatments *NoNoise.67* and *Noise.33* is significant ($p = 0.029$, exact ranksum test).

**Table 7**
Optimal switching points given effort decisions (and difference between actual median and optimal switching points).

| Prior | *NoNoise* | *Noise* |
|---|---|---|
| 1/3 | 50.7 | 47.7 |
| | −0.6 | −1.2 |
| 2/3 | 53.8 | 45.0 |
| | −3.0 | −0.5 |

interpreted with caution (Harrison, 1989). The minor losses in expected payoffs also explain why play does not converge to equilibrium more quickly.

Table 6 reveals another result. The optimal effort choices given empirical behavior are sometimes surprisingly close to the equilibrium benchmarks (see Table 4). In *NoNoise.33* and *Noise.33*, they almost exactly coincide for both types. In *Noise.67*, the gap between optimal effort choices and the equilibrium benchmark is 1.4 for both types which does not seem to be too far off the mark. Only the pooling prediction in *NoNoise.67* differs from the optimal effort choices.

Next, we analyze how managers' decisions correspond to actual worker behavior. To this end, we calculate at which signal the probability that it was sent by $\bar{t}$ is 1/2—this is the empirically optimal switching point for risk neutral receivers. This is done with probit regressions ($t = \bar{t}$ as a function of the observed $s$), separately for each treatment and based on the signals in periods 21–40. The actual switching point for each treatment is the lowest signal $s$ that, based on the probits, leads to employment with probability of at least 1/2. These actual switching points can be taken from Fig. 7. Table 7 shows the results of this analysis. It reveals that, although the empirically optimal switching points are larger than the actual switching points throughout, they do correspond closely to one another. In *NoNoise.67* and *Noise.33*, they differ by only 0.5 and 0.6 units of effort, respectively, and in *NoNoise.33* they differ by 1.2 only. In *NoNoise.67* they differ by three units of effort. Again, the small differences between optimal and actual behavior are remarkable.

### 5.4. Does the theory perform better in stochastic settings?

A final issue that we discuss here is that, quantitatively, the theory appears to be more closely in line with behavior in the treatment with noise compared to the deterministic setting. To make this statement precise, consider the absolute difference between equilibrium values given in Table 3 and the treatment averages. As for the effort levels chosen (Table 4), the *Noise* treatment averages are closer to the equilibrium in three of four cases. In Table 5 (employment rates), the comparison reveals that the averages with noise are closer to the equilibrium in all four cases. Similarly, when we look at optimal decisions given empirical behavior of the other players, the noisy variants have a smaller gap between optimal choice and average choices in all four cases of Table 6 and in one of two cases in Table 7.[19]

What could be driving this result? One possibility is that because there are multiple equilibrium configurations in the deterministic case but a unique equilibrium in the stochastic version of our game, coordination on equilibrium might be easier in the noisy case. However, there is only limited evidence that subjects play any of the non-refined equilibria, as seen above. We believe that what may be driving the results is the fact that the stochastic variant captures aspects of decision making that the deterministic variant fails to address. Consider, for example, employment rates. In the deterministic game, in equilibrium, there are no errors in hiring, that is, in a separating equilibrium 100 percent of high types are employed and 0 percent of low types; whereas in a pooling equilibrium 100 percent of workers (that is, both types) are employed. However, in the data both Type-I errors and Type-II errors occur. That is, high types are sometimes erroneously not hired and low types are erroneously employed. In contrast, Type-I and Type-II errors are an equilibrium phenomenon in the noisy variant. Consequently employment rates are never extreme. Empirically, Type-I and Type-II errors are rather frequent—an aspect of the data that is well accounted for by the stochastic version of the model. Of course, the signaling model with noise does not take errors in decision making into account, but in the data Type-I and Type-II errors occur both because of noise and because of decision errors. Our point is, hence, that the stochastic model correctly predicts Type-I and Type-II errors even if, partly, they occur for the wrong reason. As a result, the theory performs better when noise is explicitly modeled.

## 6. Conclusion

We consider a sender–receiver signaling game in an environment in which the signal-generating mechanism is subject to homoskedastic noise. This noisy setup differs markedly from the standard deterministic case. With deterministic signals, a unique perfect Bayesian equilibrium can only be obtained after the application of equilibrium refinements and, depending on the prior belief, there is either a separating equilibrium or a pooling equilibrium. With noise, the unique equilibrium is separating and the equilibrium actions vary smoothly with the level of the noise and the prior belief.

We further contrast the differences between deterministic and noisy environments by reporting on subject behavior in experiments that we ran. Specifically, we study a frame where workers can choose effort levels as a signal and personnel

[19] Since the noisy setup performs better in 12 of 14 cases, one could argue that a binomial test rejects the null hypothesis that theories perform equally well at $p < 0.01$.

managers decide whether to employ the workers. We compare games without noise to those with noise, and games with a "high" and "low" prior. Many predictions are confirmed in the data in qualitative terms and some are even relatively close in quantitative terms. In particular, the theory has predictive power regarding the main variables of interest, *viz.*, effort levels, employment rates, and employment cut-offs (that is, switching points). As predicted, given a high prior there is more pooling behavior without noise compared to noisy environments. Also consistent with theory is that subjects choose their myopic best action less frequently in the noisy treatments. Absent ample learning opportunities, signaling experiments usually do not converge fully and often myopic choices and naive mimicking rather than sophisticated play is observed. However, even though we also do not see complete convergence in our data, we do find remarkable support for the theory, and where we find deviations these regularly result in near-negligible differences in payoffs compared to optimal play. In particular, subject behavior is distinct across treatments and in line with equilibrium differences of the two model specifications.

Furthermore, while the stochastic model may analytically be more challenging than the deterministic model, subject behavior seems more in line with the equilibrium in the stochastic treatment and model in contrast to a comparison of the empirical data of the deterministic treatment and the deterministic equilibrium. We conjecture that this is due to the fact that stochastic (noisy) settings may be similar to stochastic (non-uniform) play by subjects, leading to greater congruence between the equilibrium of the stochastic game and the empirical data.

We see two avenues for future research. One interesting extension is to more closely examine the rate of convergence in subject behavior across the noisy and the deterministic treatments in order to ascertain how subject learning differs between the two settings. Also, cross-game learning (Cooper and Kagel, 2005, 2009) for two different priors or for noise *vs.* no noise may lead to interesting insights. Secondly, carefully eliciting beliefs should be intriguing for our game as the zero-one decision of the second mover is only a coarse measure of beliefs. Analyzing the first movers' second-order beliefs seems a further useful addition to our design as it may identify motives perhaps not captured by the theory. For example, first-movers may deliberately trade off a lower chance of being employed against lower effort cost beyond the extent suggested by theory. Of course, the learning and belief-elicitation issues nicely complement one another as the beliefs will be a useful indicator of learning.

## Appendix. Supplementary data

Supplementary data associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2011.12.002.

## References

Ashenfelter, O., Currie, J., Farber, H.S., Siegel, M., 1992. An experimental comparison of dispute rates in alternative arbitrage systems. Econometrica 60, 1407–1433.
Backus, D., Driffill, J., 1985. Inflation and reputation. Am. Econ. Rev. 75 (3), 530–538.
Bagwell, K., Riordan, M.H., 1991. High and declining prices signal product quality. Am. Econ. Rev. 81 (1), 224–239.
Brandts, J., Holt, C.H., 1992. An experimental test of equilibrium dominance in signaling games. Am. Econ. Rev 82, 1350–1365.
Brandts, J., Holt, C.H., 1993. Adjustment patterns and equilibrium selection in experimental signaling games. Int. J. Game Theory 22, 279–302.
Carlsson, H., Dasgupta, S., 1997. Noise-proof equilibria in two-action signaling games. J. Econ. Theory 77, 432–460.
Cho, I.-K., Kreps, D.M., 1987. Signaling games and stable equilibria. Quart. J. Econ. 102, 179–221.
Cooper, D.J., Garvin, S., Kagel, J.H., 1997a. Adaptive learning vs. equilibrium refinements in an entry limit pricing game. Econ. J. 107, 553–575.
Cooper, D.J., Garvin, S., Kagel, J.H., 1997b. Signaling and adaptive learning in an entry limit pricing game. RAND J. Econ. 28, 662–683.
Cooper, D.J., Kagel, J.H., 2005. Are two heads better than one? Team versus individual play in signaling games. Am. Econ. Rev. 95, 477–509.
Cooper, D.J., Kagel, J.H., 2009. The role of context and team play in cross-game learning. J. Eur. Econ. Assoc. 7, 1101–1139.
de Haan, T., Offerman, T., Sloof, R., 2011. Noisy signaling: theory and experiment. Games Econ. Behav. 73 (2), 402–428.
Fischbacher, U., 2007. Zurich toolbox for readymade economic experiments. Exp. Econ. 10, 171–178.
Fonseca, M., 2009. An experimental investigation of asymmetric contests. Int. J. Ind. Org. 27, 582–591.
Grossman, S., Perry, M., 1986. Perfect sequential equilibrium. J. Econ. Theory 39 (1), 120–154.
Harrison, G., 1989. Theory and misbehavior of first-price auctions. Am. Econ. Rev. 79, 749–762.
Hertzendorf, M.N., 1993. I'm not a high-quality firm, but I play one on TV. RAND J. Econ. 24, 236–247.
Hollander, M., Wolfe, D.A., 1999. Nonparametric Statistical Methods. John Wiley and Sons, Hoboken, NJ.
Kübler, D., Müller, W., Normann, H.T., 2008. Job market signaling and screening: an experimental comparison. Games Econ. Behav. 64, 219–236.
Mailath, G.J., Okuno-Fujiwara, M., Postlewaite, A., 1993. Belief-based refinements in signaling games. J. Econ. Theory 60, 241–276.
Matthews, S., Mirman, L.J., 1983. Equilibrium limit pricing: the effects of private information and stochastic demand. Econometrica 51, 981–996.
Milgrom, P., Roberts, J., 1982. Limit pricing and entry under incomplete information. Econometrica 50, 443–459.
Milgrom, P., Roberts, J., 1986. Price and advertising signals of product quality. J. Polit. Econ. 94, 796–821.
Miller, R., Plott, C., 1985. Product quality signaling in experimental markets. Econometrica 53, 837–872.
Potters, J., van Winden, F., 1996. Comparative statics of a signaling game: an experimental study. Int. J. Game Theory 25, 329–353.
Reinganum, J., Wilde, L., 1986. Settlement, litigation, and the allocation of legal costs. RAND J. Econ. 17, 557–566.
Sheremeta, R.M., 2010. Experimental comparison of multi-stage and one-stage contests. Games Econ. Behav. 68, 731–747.
Sosis, R.H., Ruffle, B.J., 2003. Religious ritual and cooperation: testing for a relationship on Israeli religious and secular kibbutzim. Curr. Anthropol. 44 (5), 713–722.
Spence, M., 1973. Job market signaling. Quart. J. Econ. 88, 355–374.
Spence, M., 1974. Market Signaling – Informational Transfer in Hiring and Related Processes. Harvard University Press, Cambridge.
Wooldridge, J.M., 2003. Cluster-sample methods in applied econometrics. Am. Econ. Rev. 93 (2), 133–138.
Zahavi, A., 1975. Mate selection – a selection for a handicap. J. Theor. Biol. 53, 205–214.