A Simple Note on Informational Cascades♣

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Abstract

Seminal models of herd behaviour and informational cascades point out existence of negative information externalities, and propose to 'destroy' information in order to achieve social improvements. Although in the last years many features of herd behaviour and informational cascades have been studied, this particular aspect has never been extensively analysed. In this article we try to fill this gap, investigating both theoretically and experimentally whether and to which extent destroying information can improve welfare. Our empirical results show that this decisional mechanism actually leads to a behaviour pattern more consistent with the theory that in turn produces the predicted efficiency gain.

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JEL Classification: C91; D62

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1 Introduction

Part of social learning is related to an apparently naive behaviour known as *herd behaviour* (Banerjee, 1992)[1] or *informational cascades* (Bikhchandani, Hirshleifer, and Welch, 1992 – BHW, henceforth). A peculiarity of these models, however, is that they view agents' imitative behaviour as perfectly rational, even though characterized by imperfect information.

This behaviour takes place when agents can augment their information set by looking at other agents' behaviour. Although rational, it could cause information externalities that result in an aggregate welfare loss (Becker, 1991). In this situation, the individual rational behaviour may well result in a non-optimal strategy from an aggregate point of view. Looking at the real world, we have abundant empirical evidence for informational cascades. Actually, one of the most attractive features of these kinds of models concerns their direct application to a range of every-day situations. Just to cite an example, we can refer to bubbles in financial markets (Plott, 2002; Hey and Morone (2004); Morone (forthcoming)). The idea underlying these models is simple. Consider the case in which I have to choose between two unknown restaurants and I have no relevant information about them. However, I can infer that the most crowded is the best one and will choose to join the queue. This behaviour is rational, but the possibility that first customers have no pregnant information as well is crucial. BHW point out that the conformity of followers in a cascade contains no informational value (p. 998-999), and this argument has been demonstrated by some empirical evidence (Anderson and Holt, 1997; Allsopp and Hey, 2000). On the other hand, also the impact of signal accuracy on cascade

[1] A generalization of Banerjee's model was proposed in Morone and Samanidou (forthcomming).

efficiency has been demonstrated by some simulations (e.g., Pastine and Pastine, 2005).

The aim of this paper is investigating the possibility to mitigate informational cascades' negative effects forcing the first k subjects in a queue to play only according to their private information. For this purpose, we analyse a sequential model departing from BHW's model in some relevant parts and then we experimentally investigate if "*society may actually be better off by constraining some of the people to use only their own information*" (Banerjee, 1992; p. 798).

The paper is structured as follows. Section 2 is devoted to the new specification of the standard model. The experimental design and results are introduced, respectively, in Sections 3 and 4. Section 5 concludes.

2 Theory

In addition to the seminal papers on herd behaviour and informational cascades (Banerjee, 1992; Bikhchandani et al., 1992), even more recently a paper on word-of-mouth learning (Banerjee and Fudenberg, 2004) notes that the inefficient herding of the standard models does not occur if some agents are forced to use their own private information. Nevertheless, earlier literature did not provide any model capturing this feature. Therefore, in order to fill this gap, we develop a new specification departing from the standard model as advanced in BHW. We retain the main features of the original model. We have a population of $I = \{1, \ldots, N\}$ individuals. Each individual $i \in N$ has to decide whether to adopt a specific behaviour, for example, whether to adopt a new technology or not. All individuals make their choices in a sequential and exogenously determined order. The gain of

adopting, V, is the same for all $i \in N$ and is either zero or one. These two events have the same *ex-ante* probability to occur.

However, each individual i privately observes a conditionally i.i.d. signal about V. This signal s is either 0 or 1: 1 is observed with probability $p > ½$ if the true value is 1, and with probability $1-p$ otherwise.

Under our specification, the first k ($< N$) individuals in the queue are not allowed to observe the decisions already taken, whereas the entire history of decisions is commonly known to the last $N-k$ individuals. We can think of this game as of N-stage game where the first k individuals play simultaneously and the remaining $N-k$ sequentially. As the first k individuals can observe only their own signal, rationality requires them to follow their private information: they should take on the new behaviour if the signal is 1, and reject it otherwise. In contrast, the remaining $N-k$ individuals should base their decision on both their own signal and all past decisions, thereby choosing the most frequently observed action[2]. In case of indifference, we assume that individual i with $i = k+1, \ldots, N$ follows the tie-breaking rule of adopting or rejecting with equal probability.

In our specification it is as if individual i, with $i = k+1, \ldots, N$ has an advantage of additional signals. In this manner, we expect our specification to lead to a more socially efficient final outcome, as the society has a mechanism that allows aggregating the information in a later stage and in a more correct way.

[2] More precisely, Anderson and Holt (1997) show that the optimal strategy in a Bayesian sense whenever the two events are equally probable and signals identically distributed corresponds to the very simple strategy of doing the count of the previous decisions, one's own signal included.

In their model (T1), where all decision makers are allowed to observe their predecessors' action, BHW derive the unconditional *ex ante* probability of a cascade and the *ex ante* probability of no cascade after an even number of individuals n.

$P = 0.75$; $n = 100$	($k = 10$)	($k = 56$)	($k = 98$)
T1	.8077	.8077	.8077
T2	.9690	.9999	.9999
($k = 6$) ; $p = 0.75$	$N = 10$	$n = 100$	$n = 1000$
T1	.8075	.8077	.8077
T2	.9333	.9370	.9370
($k = 56$); $n = 100$	$P = .55$	$p = .85$	$p = .99$
T1	.5664	.9011	.9949
T2	.7778	.9999	1.000

Table 1 Probability of a correct cascade: Comparative static analysis and comparison between models[3]

They also derive the probabilities of ending up in a correct cascade and ending up in a wrong one. We derive the same probabilities after having taken in consideration the fact that the first k players act only based on their own signal s (T2). We show our main results in the Appendix A. At this point, however, it may be more illustrative to compare probabilities of ending up in a correct cascade (to a some extent, it can be considered as an index of efficiency) under the two specifications for some different parameter values (k, number of players observing only their own signal; p, probability of signal correctness; n, number of players having already taken their decision). Figures are shown in Table 1.

[3] In the first row are reported different parameter values at which probabilities are computed and in boldface, parameters held constant for each comparative static exercise. Different values of k are in parentheses since relevant only to our model.

At a first glance, it is evident that probability of ending up in a correct cascade is higher in T2. Entering into details, we can point out that as k increases (top panel), probability of a correct cascade becomes not statistically different from 1, whereas under the standard model the probability is quite high, but never reaches this level. Other results are more obvious, in the sense that probability of a correct cascade is monotonically increasing in the number of subjects that have already made a decision (n, middle panel) and in the signal correctness (p, bottom panel) under both the two specifications, but nevertheless always higher under ours.

Probably, it may be interesting to combine results regarding the effect of changes in signal correctness and number of subjects that act with no clue regarding previous decisions. We perform these comparative static exercises varying simultaneously p and k, while keeping N - the number of individuals in the population – constant. Consequently, we have the opportunity to note that for each value of k there is a probability p^*, under which the difference between the two specifications is maximised. The converse is also true.

3 Experimental design

In order to test empirically whether the new specification of the model allows achieving a social improvement, we ran two computerized treatments at the laboratory of ESSE at the University of Bari. The control treatment (T1) was set in accordance with the original model, whereas in the second treatment (T2) we test

the new specification, with the first k = 4 subjects forced to play basing their decision exclusively on their private information[4].

The experiment was programmed using the Z-tree software (Fischbacher, 2007). Each treatment lasting for about an hour was made up of 22 periods, of which 2 were trial ones. The trial periods were necessary for subjects to become familiar with the experiment, providing them also the opportunity to ask questions about the instructions (Appendix B). The final payment was made only for the 20 real periods and paid at the end of each treatment.

We had N = 10 subjects for each treatment sitting next to a PC terminal connected by a net. The subjects could not see each other or communicate. All of them were students of Economics not familiar with previous similar experiments.

In the experiment, subjects acted as entrepreneurs and their task was to decide whether to invest in a new product or not. The order in which they chose sequentially was randomly determined period by period[5].

However, subjects did not know whether this product would be profitable or not once on the market. Whenever they made the right decision, they gained €0.5, and zero otherwise[6]. For each period the programme established the true value of V

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[4] As we noted above, there is an optimal value for k^* for each parameter combination. We determined the optimal k^* with a Monte Carlo simulation. This simulation provided the winning percentages for each position in the queue, provided that we consider the individual winning percentage as a proxy for individual utility. The simulation consisted of 10 millions iterations for each different value of k, setting N and p at 10 and .75, respectively. We get a measure for social welfare summing up individual winning percentages over the entire population, and we picked the case in which this indicator was at its maximum.

[5] They were informed about their turn via a message on their PC screen.

[6] More precisely, if the product was successful (V = 1), they would gain €0.5 in case of investment, and zero otherwise. If the product was not successful (V = 0), they would gain €0.5 in case of no investment (the right decision in this scenario), and zero otherwise.

but did not reveal it to subjects. Each of them, however, received a free-of-charge signal s about V (a sort of a result of a market survey). These signals took either the value 1 or the value 0 and the signal correctness (p = .75) was common knowledge. The screen displayed these details: one's own turn to play; the position in the queue; where allowed, the decision made by predecessors; and one's own signal. At the end of each period, subjects were informed about the right option and their payoff. When all periods were played, subjects were paid and free to leave the laboratory. Average payoff was €6.75.

4 Results

We start this section showing some data at individual level (raw data provided in the Appendix C). In each position, taking into account predecessors' decisions and the signal realization, we determine which action should be chosen according to the theory. Consequently, we categorize as rational behaviours in accordance with it (in our simple set-up, the optimal strategy in a Bayesian sense corresponds to the count, as explained in footnote 2) and, in case of indifference, whenever subjects adopted the tie-breaking rule, regardless of the fact that it produces a cascade or not. In particular, at the individual level, in the table we report also as cascade behaviour all the cases where "an imbalance of previous inferred signals causes a person's optimal decision to be inconsistent with his or her private signal" (Anderson and Holt, 1997; p. 851), that is, all the cases in which individuals follow the queue, disregarding their signal. As regards behaviours categorized as irrational, namely, inconsistent with the theory, we discriminate cases in which it can be rationalized somehow –

following her own signal – from cases where it cannot be explained whatsoever. Results are reported in Table 2.

	Rational behaviour			Irrational behaviour	
		Occurrence of rational cascades		not rationalized	signal-keeping
		correct	wrong		
T1	146	4	16	33	21
T2	171	12	8	24	5

Table 2 Summary of behaviours observed in the experiment (individual level)

From table 2 it can be clearly noted that the new specification may be effective in driving a more consistent behaviour (146 out of 200 observations - 73% - in T1 vs. 171 out of 200 observations - 85.5% - in T2). However, though lower, among the occurrence of irrational behaviours in T2 the percentage of behaviour that cannot be explained in any case is higher than in T1 (33 out of 54 cases in T1 vs. 24 out of 29 cases in T2). Moreover, it is interesting that while observing the same occurrence of cascade behaviour under the two treatments (20 in each), the percentage of a correct cascade is up to three times higher in T2. Finally, we observe that cascade behaviour is rather fragile (individuals do not choose to conform to the mass, still when their all predecessors made the same choice)[7], and that often they

[7] As an extreme case of this kind of behaviour, we can cite as example the behaviour of subject in position 10 (period 10) under T1 that decided to break the cascade even if all the players before her made the same decision.

also choose to play against their own signal, especially when they are the first in the queue[8].

At this point, in order to test our hypothesis, namely that under the new specification of the model the outcome is socially more efficient, we compare the average earnings under the two treatments.

Position in the queue	Theoretical earnings		Experimental earnings	
	T1	T2	T1	T2
1	0.225	0.4	0.225	0.356
2	0.325	-	0.275	-
3	0.3125	-	0.325	-
4	0.33125	-	0.275	-
5	0.34375	0.4	0.275	0.425
6	0.34375	0.4125	0.25	0.375
7	0.34375	0.4125	0.3	0.375
8	0.34375	0.40625	0.3	0.4
9	0.34375	0.425	0.375	0.375
10	0.34375	0.425	0.375	0.4
TOTAL	**3.25625**	**4.08125**	**2.975**	**3.775**

Table 3 Average earnings for each position and for each treatment[9]

Particularly interesting is the comparison between the theoretical earnings, as it would have been if all individuals behaved according to the theory, given the actual signal realization during the experiment, and the experimental earnings, the

[8] In particular, we observe that first individuals played against their own signal in 8 cases out of 20 periods in T1. Considering that in T2 the first four players made decisions without observing predecessors'signal, we observe this kind of behaviour in 17 instances.

[9] Since the first four positions in T2 can be considered as theoretically indistinct, we decided to provide in the table an average of the relative earnings.

actual payoffs obtained by participants during the experiment. Results are reported in Table 3[10].

First, we note a statistically significant difference between the two treatments (Wilcoxon rank-sum test for theoretical earnings: -3.835, *p-value* = .0001; for experimental earnings: -3.755, *p-value* = .0059). Interestingly, each experimental treatment is not statistically different from its theoretical counterpart (Wilcoxon rank-sum test for BHW and T1: 1.614, *p-value* = .1066; for our specification and T2: 1.190, *p-value* = .2340). Second, for each position in the queue, we observe always higher average earnings under T2[11].

In order to give an additional insight into the social efficiency gain, we can consider the percentage of winning as a useful proxy for individual utility, and then compare this index under the two treatments. In figure 1 we report the index for each different position held in the decisional queue.

[10] It is important to note that the probability to obtain a correct signal was identical across the treatment (fixed at 0.75 over all the treatments), but that the actual realization of signal was different. Consequently, since chance rather than behaviour could explain the difference between the two treatments, we standardise our results dividing the average earnings in Table 3 by the frequency of correct signals, position by position. Results in Appendix D.

[11] The lower average payoff for the fourth position cannot be considered as an exception. Indeed, under our specification the first k (except for the first) individuals observe their situation worsened passing from the first to the second treatment, even if the deterioration of their situation is more than offset by the improving of the remaining N-k individuals, for an appropriate choice of the value k.

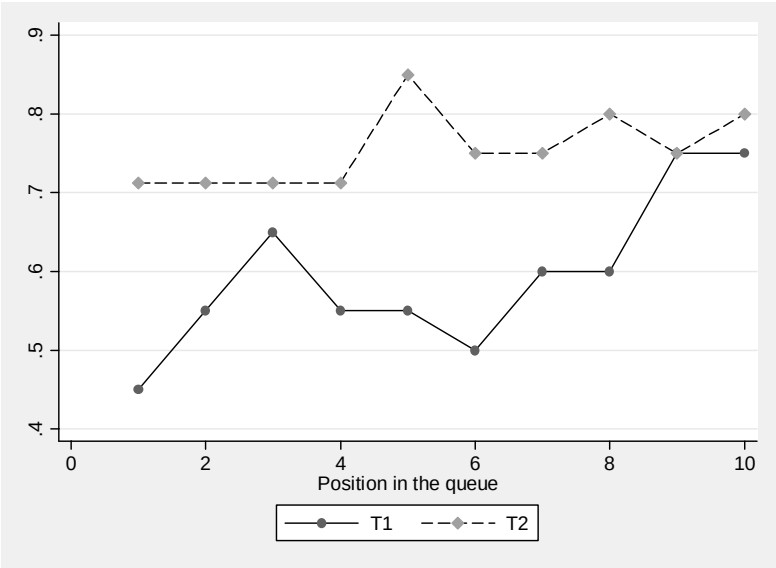

Figure 1 Percentages of winning[12]

Except for the fourth subject in the second treatment (see footnote 10), under the second treatment the percentage is always higher than, or at the most the same as, under the first treatment. Looking at the graph, we may state that the new decisional mechanism is preferable from a social and even individual point of view, i.e. in T2 the winning percentage is alwayes larger then in T1, even if at late stages (position 9 and 10) the winning percentages in the two treatments become very similar.

4.1 Econometric analysis

Finally, we estimate a very simple learning model. Specifically, we constructed a model that links decisions in the experiment to a set of determinants, as follows.

[12] As explained in footnote 10, we averaged percentages the first four positions in T2 given that they can be considered theoretically indistinct.

First, the presence of learning is investigated by the use of the variable *Time* (the period number) and the variable *Time²*, to test for concavity of learning. Moreover, in order to gain further insight, we test for the presence of directional learning (or Cournot behaviour; Selten and Buchta, 1998) by using the *Correctwon* variable (a dummy variable equal to 1 if in the most recent period the subject made the theoretically correct decision and won) and *Correctlost* variable (a dummy variable equal to 1 if in the most recent period the subject made the theoretically correct decision and lost). Our dependent variable, *Correct*, is a dummy variable equal to 1 whenever subjects make the decision consistent with the theory, 0 otherwise. Consequently, we run a probit estimation procedure. Results are reported in Table 4.

Dep. Variable: Correct	Marginal Effect	Std. Error	p-value
Time	.02199	.01628	0.177
Time²	-.00084	.00075	0.264
Correctwon	-.13474	.05139	0.011
Correctlost	-.22575	.08449	0.004
T2	.15977	.04393	0.000
Log likelihood	-217.271		
Pseudo R^2	0.0478		
NOBs	400		

Table 4 Maximum Likelihood probit estimation[13]

We note no trend in observing a more consistent decisions over time (in fact, *Time* is not statistically significant); consequently, concavity for learning is not

[13] Note that the reported significance levels assume independent observations, though this is unlikely to be the case.

statistically significant, either. Even directional learning is not a firm determinant of learning. Indeed, even if both *Correctwon* and *Correctlost* are significant, *Correctwon* does not present the correct direction (positive sign) in our analysis. In fact, we expect the probability of making the correct decision to increase if in the previous period subject made correct decision and won.

Interestingly, on the other hand, the dummy variable for the treatment T2: the decisional mechanism implemented in this treatment is actually effective in increasing probability of making the correct decision by 16%[14].

5 Conclusions

Negative informational externality produced by phenomenon of informational cascade has drawn quite a lot of attention in economic literature. Hence, finding mechanisms useful in eliminating or at least minimising this externality are of quite an interest. The paradox whereby burning a piece of information in a first stage of the sequential decisional process could turn to be a social improvement in a later stage was indeed worth investigating. This was our task. Our empirical results show that this decisional mechanism actually leads to behaviour more consistent with the theory that in turn produces a social improvement. If supported by further analyses – aimed, for example, to design an implementable self-enforcing mechanism - our result may open new challenging scenarios once applied to reality.

[14] However, it could be of interest to test if the dummy variable T2 has a different impact, depending on the different positions taken into account. In order to test for the presence of a possible *structural break* at stage 4, we perform a Chow test. Results are given in Appendix E.

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Appendices

A - In order to get probabilities in the same fashion as BHW obtained, we derived them varying each time the value of k – number of individuals acting with no clue regarding previous actions. Then, having been noted some regularities, we generalized the model, whatever the value for k.

The probability of NO-cascade after $n = 2m$ individuals is simply the probability of observing the same occurrence for the two signals, i.e., $s = 0$ and $s = 1$. Consequently, in our specification, the probability of NO-cascade is shown in 1.a, whenever k is an even number:

$$\frac{k!}{\left(\frac{k}{2}\right)!\left(\frac{k}{2}\right)!} p^m (1-p)^m \qquad 1.a$$

whereas, whenever k is an odd number:

$$\frac{1}{2}\left[\frac{(k+1)!}{\left(\frac{k+1}{2}\right)!\left(\frac{k+1}{2}\right)!}\right] p^m (1-p)^m . \qquad 1.b$$

However, it is of greater importance to consider the probability of ending up in a CORRECT-cascade. The probability to observe a correct cascade after 2 individuals is simply the probability that both of them receive a correct signal, i.e., p^2. The probability to observe a correct cascade after 4 individuals is simply the sum of the probabilities of the two independents events: the occurrence of a correct cascade after 2 individuals OR the occurrence of no cascade after 2 individuals AND the occurrence to observe a correct cascade for the third and fourth players.

In general formulas, after $n = 2m$ individuals, whenever k is an even number:

$$\sum_{j=\frac{k}{2}+1}^{k}\binom{k}{j}p^{j}(1-p)^{(k-j)}+\frac{k!}{\left(\frac{k}{2}\right)!\left(\frac{k}{2}\right)!}p^{\frac{k}{2}}(1-p)^{\frac{k}{2}}\frac{p(p+1)}{2}\left[\frac{1-\left(p-p^{2}\right)^{m-\frac{k}{2}}}{1-\left(p-p^{2}\right)}\right] \qquad 2.a$$

and whenever k is an odd number:

$$\left(\frac{1}{4}\binom{k+1}{\frac{k+1}{2}+1}p^{j}(1-p)^{(k+1-j)}+\sum_{j=\frac{k+1}{2}+2}^{k+1}\binom{k+1}{j}p^{j}(1-p)^{(k+1-j)}\right)+\frac{1}{2}\left[\frac{(k+1)!}{\left(\frac{k+1}{2}\right)!\left(\frac{k+1}{2}\right)!}\right]p^{\frac{k+1}{2}}(1-p)^{\frac{k+1}{2}}\frac{p(p+1)}{2}\left[\frac{1-\left(p-p^{2}\right)^{m-\frac{k+1}{2}}}{1-\left(p-p^{2}\right)}\right] \qquad 2.b$$

Finally, a similar reasoning can be applied to calculate the probability of ending up in a WRONG-cascade after $n = 2m$ individuals. In formulas, whenever k is an even number:

$$\sum_{j=\frac{k}{2}+1}^{k}\binom{k}{j}(1-p)^{j}p^{(k-j)}+\frac{k!}{\left(\frac{k}{2}\right)!\left(\frac{k}{2}\right)!}p^{\frac{k}{2}}(1-p)^{\frac{k}{2}}\frac{(p-2)(p-1)}{2}\left[\frac{1-\left(p-p^{2}\right)^{m-\frac{k}{2}}}{1-\left(p-p^{2}\right)}\right] \qquad 3.a$$

and whenever k is an odd number:

$$\left(\frac{1}{4}\binom{k+1}{\frac{k+1}{2}+1}(1-p)^{j}p^{(k+1-j)}+\sum_{j=\frac{k+1}{2}+1}^{k+1}\binom{k+1}{j}(1-p)^{j}p\right)+\frac{1}{2}\left[\frac{(k+1)!}{\left(\frac{k+1}{2}\right)!\left(\frac{k+1}{2}\right)!}\right]p^{\frac{k+1}{2}}(1-p)^{\frac{k+1}{2}}\frac{(p-2)(p-1)}{2}\left[\frac{1-\left(p-p^{2}\right)^{m-\frac{k+1}{2}}}{1-\left(p-p^{2}\right)}\right] \qquad 3.b$$

To make an example, we provide the calculations for determination of probability of NO-cascade, of ending up in a CORRECT-cascade or in a WRONG-cascade for the specific value of parameters chosen for the experiment, i.e., $k = 4$, $m = 5$, and $p = 0.75$.

Applying formula 1.a:

$$\frac{4!}{2!2!}(0.75)^{5}(0.25)^{5}=0.001390$$

Applying formula 2.a:

$$\sum_{j=3}^{4}\binom{4}{j}0.75^{j}(0.25)^{(4-j)}+\frac{4!}{2!2!}(0.75)^{2}(0.25)^{2}\frac{0.75(1.75)}{2}\left[\frac{1-(0.75-0.5625)^{3}}{1-(0.75-0.56.25)}\right]=0.907531$$

Finally, considering formula 3.a, we get probability of ending up in a WRONG-cascade:

$$\sum_{j=3}^{4}\binom{4}{j}(0.75)^{j}(0.25)^{(4-j)}+\frac{4!}{2!2!}p^{2}(0.25)^{2}\frac{(-1.25)(-0.25)}{2}\left[\frac{1-(0.75-0.5625)^{3}}{1-(0.75-0.56.25)}\right]=0.091079$$

B – *Instructions* (original provided in Italian)

Welcome! This experiment is designed to study how people make decisions. The experiment is very simple, and you will have the possibility of earning money, which will be paid to you in cash at the end of the experiment.

This amount will depend, on the one hand, on your decisions and, on the other hand, on luck.

You will play as an entrepreneur and your task will be to decide to develop a new product or not.

Two scenarios will have the same probability to occur: or all goods will be sold or not a single one.

You will repeat your task 20 times. In each period, the computer will choose the scenario. The scenario will be the same for all the participant, but different in each period.

Whenever you take the right decision, you will earn 0.5€, nothing otherwise, as shown in the table:

	Decision: to invest	Decision: not to invest
All goods sold	0.5€	0
No good sold	0	0.5€

It is important to know that you make your decision in sequence and the order is randomized in each period.

However, you will be provided with two different kinds of information before making your decision. First, you will receive results from a market survey reliable at 75%. In particular, during the experiment you will be provided with a signal according to the result of the survey. As shown in the table, to each signal is connected a different likelihood of the two scenarios:

	Signal = 1	Signal = 0
All goods sold	75%	25%
No good sold	25%	75%

Second, you will be informed about decisions already made by all entrepreneurs before you.
You will not be required to pay for these pieces of information. These will appear automatically on your PC screen when it is your turn to play.
It is important to note that the first four players will not receive this second kind of information. On the contrary, from the fifth player onwards, players will receive all relevant information regarding previous decisions.

Whenever you make your decision, you have to press the OK button to confirm your choice.
As soon an all players have made their decision, on your PC screen you will be informed about the right choice to take in that period and your relative payoff.

You will play for 20 periods, in addition to two trial periods at the beginning of the experiment.
At the end, you will be paid (except for the payoff earned during the trial periods) and you will be free to leave the laboratory.
The rules are very simple. However, please do not communicate with other participants during the experiment. You are free to put questions to experimenters at any time during trial periods raising your hands.
Good luck!

C – *Raw data*

T1 treatment

Period	Subject	Signal	Action	Period	Subject	Signal	Action	Period	Subject	Signal	Action	Period	Subject	Signal	Action
1	6	1	1	6	6	1	1	11	6	1	1	16	1	1	1
1	2	1	1	6	7	1	1	11	4	0	0	16	5	0	0
1	4	1	0	6	3	1	1	11	7	1	1	16	6	0	0
1	7	1	1	6	9	1	1	11	10	0	0	16	10	0	0
1	3	1	1	6	1	0	0	11	9	0	1	16	8	0	0
1	1	1	1	6	8	1	1	11	8	0	0	16	9	0	0
1	9	1	1	6	10	1	1	11	5	0	0	16	7	1	0
1	8	1	1	6	4	1	1	11	3	0	1	16	2	0	0
1	5	1	0	6	2	1	0	11	1	0	0	16	3	1	1
1	10	1	1	6	5	1	1	11	2	0	1	16	4	0	0
2	3	0	1	7	7	0	0	12	2	0	0	17	3	1	1
2	9	0	0	7	1	1	0	12	1	0	0	17	2	0	1
2	5	1	1	7	2	0	0	12	9	1	1	17	8	1	1
2	4	0	1	7	6	0	1	12	4	0	1	17	5	0	0
2	10	1	1	7	4	0	0	12	10	1	1	17	1	0	0
2	6	1	1	7	3	0	0	12	8	1	1	17	6	0	0
2	2	0	0	7	9	0	0	12	7	1	0	17	7	1	1
2	8	0	1	7	10	0	0	12	5	1	1	17	10	0	0
2	7	0	1	7	8	1	1	12	3	1	1	17	4	0	0
2	1	0	0	7	5	1	1	12	6	1	1	17	9	0	0
3	9	0	0	8	2	0	0	13	9	0	1	18	9	1	1
3	1	0	0	8	5	0	1	13	7	0	0	18	5	1	0
3	3	0	1	8	1	1	0	13	4	0	0	18	6	1	0
3	2	0	0	8	10	0	1	13	10	1	0	18	10	0	1
3	5	1	0	8	3	0	0	13	6	0	0	18	4	0	0
3	10	1	1	8	8	0	1	13	5	1	1	18	7	1	0
3	6	0	0	8	4	0	0	13	3	0	0	18	1	1	0
3	8	0	0	8	6	0	0	13	8	0	0	18	8	0	0
3	4	0	1	8	9	0	0	13	1	1	1	18	3	0	0
3	7	0	0	8	7	1	0	13	2	1	1	18	2	1	1
4	4	1	1	9	6	1	1	14	3	0	0	19	2	1	1
4	3	0	0	9	3	0	0	14	9	1	1	19	3	0	0
4	9	0	0	9	4	0	1	14	4	0	1	19	5	0	0
4	2	0	1	9	5	0	0	14	2	0	0	19	4	0	0
4	7	0	1	9	9	0	0	14	1	0	0	19	8	0	0
4	6	0	1	9	1	0	1	14	7	1	1	19	1	1	0
4	8	0	0	9	8	0	0	14	5	0	0	19	9	1	1
4	1	0	1	9	2	0	0	14	10	0	0	19	6	0	0
4	5	1	1	9	10	1	0	14	8	1	1	19	7	0	0
4	10	0	0	9	7	0	0	14	6	0	1	19	10	0	0
5	8	0	1	10	2	0	0	15	5	0	1	20	8	0	0
5	6	0	1	10	5	0	0	15	6	0	1	20	2	1	0
5	10	1	1	10	6	0	0	15	9	0	0	20	3	1	1
5	3	0	1	10	9	0	0	15	2	0	1	20	7	1	1
5	4	0	0	10	7	0	0	15	8	0	0	20	10	1	1
5	5	1	1	10	4	0	0	15	1	1	1	20	4	0	0
5	2	0	0	10	1	0	1	15	7	1	0	20	1	0	0
5	7	0	1	10	3	0	0	15	4	1	1	20	6	1	0
5	9	0	0	10	8	0	0	15	10	0	0	20	9	0	0
5	1	0	1	10	10	1	1	15	3	0	0	20	5	1	1

T2 treatment

Period	Subject	Signal	Action	Period	Subject	Signal	Action	Period	Subject	Signal	Action	Period	Subject	Signal	Action
1	3	0	0	6	2	0	0	11	10	0	0	16	1	1	1
1	10	0	1	6	7	0	0	11	2	1	1	16	10	1	1
1	6	0	0	6	3	0	0	11	3	0	0	16	2	1	1
1	4	0	0	6	1	0	0	11	8	0	1	16	6	1	1
1	2	0	1	6	4	0	1	11	4	0	1	16	8	1	1
1	5	0	0	6	6	0	0	11	6	1	1	16	4	1	0
1	7	0	0	6	9	0	0	11	7	1	1	16	7	1	1
1	9	0	0	6	8	0	0	11	1	0	0	16	3	1	1
1	1	0	0	6	5	0	0	11	5	0	0	16	5	1	1
1	8	0	0	6	10	0	0	11	9	1	1	16	9	0	1
2	2	1	1	7	4	1	1	12	6	0	0	17	5	0	0
2	6	0	0	7	5	1	1	12	2	0	0	17	9	1	0
2	5	1	0	7	7	1	1	12	1	0	0	17	10	0	0
2	1	0	0	7	1	1	1	12	5	0	0	17	2	1	0
2	10	0	1	7	8	0	1	12	8	0	0	17	6	1	1
2	9	0	1	7	2	1	1	12	9	0	0	17	1	1	1
2	8	1	1	7	6	1	1	12	10	0	0	17	3	1	0
2	7	0	1	7	10	1	1	12	7	0	0	17	4	1	1
2	3	0	1	7	9	1	1	12	3	0	0	17	8	0	0
2	4	1	1	7	3	1	1	12	4	0	1	17	7	0	0
3	7	0	1	8	2	1	0	13	8	0	0	18	3	0	0
3	10	0	0	8	1	0	0	13	2	0	0	18	5	0	0
3	2	0	0	8	3	1	1	13	3	0	0	18	10	1	1
3	3	0	0	8	6	1	0	13	1	0	0	18	7	0	0
3	1	0	0	8	8	1	1	13	4	0	1	18	4	0	0
3	9	0	1	8	7	1	1	13	6	0	0	18	9	0	0
3	8	0	0	8	9	1	1	13	7	0	0	18	6	0	0
3	5	0	0	8	4	1	1	13	10	0	0	18	1	0	0
3	6	0	1	8	10	1	1	13	9	0	0	18	8	1	0
3	4	0	0	8	5	0	0	13	5	0	0	18	2	0	1
4	6	0	0	9	7	0	0	14	9	1	1	19	3	1	1
4	9	0	0	9	6	0	0	14	2	0	0	19	6	1	1
4	3	0	0	9	4	0	0	14	4	0	1	19	10	0	1
4	7	0	1	9	10	0	0	14	6	1	1	19	4	1	1
4	4	0	1	9	9	0	0	14	3	1	1	19	8	0	0
4	2	0	0	9	3	0	1	14	5	1	1	19	5	1	1
4	10	0	0	9	8	0	0	14	8	1	1	19	2	1	1
4	1	0	0	9	2	1	0	14	7	0	1	19	9	0	0
4	5	0	0	9	5	0	1	14	10	1	1	19	7	1	1
4	8	1	0	9	1	0	0	14	1	0	1	19	1	1	1
5	8	1	1	10	10	0	1	15	7	1	1	20	6	0	0
5	1	1	1	10	8	1	1	15	10	1	1	20	2	0	0
5	9	0	1	10	4	1	1	15	6	1	1	20	9	0	0
5	3	1	1	10	3	0	1	15	4	1	1	20	3	0	0
5	10	1	1	10	2	1	1	15	8	1	1	20	8	1	0
5	6	0	1	10	6	0	1	15	5	0	1	20	1	0	0
5	7	1	1	10	7	0	1	15	9	1	1	20	4	1	1
5	5	1	1	10	9	0	1	15	2	1	1	20	7	0	0
5	4	1	1	10	1	1	1	15	3	1	1	20	5	0	0
5	2	1	1	10	5	0	1	15	1	1	1	20	10	1	0

D – In Table A1 we show results regarding average earnings, considering that under the two treatments there was a different realization of correct signal, namely, only the probability to get a correct signal was the same ($p = 0.75$) but it may be that differences in theoretical and experimental earnings were due to chance rather than systematic difference in behaviour. In order to compare average earnings taking into account this caveat, we standardise our results dividing figures in Table 3 by the frequency of correct signals, position by position:

	Theoretical earnings		Experimental earnings	
Position in the queue	T1	T2	T1	T2
1	0.01875	0.02105	0.01875	0.02237
2	0.01912	0.02333	0.01618	0.02167
3	0.02083	0.03393	0.02167	0.03036
4	0.01949	0.01974	0.01618	0.01316
5	0.02148	0.02667	0.01719	0.02833
6	0.02865	0.02578	0.02083	0.02344
7	0.02292	0.02578	0.02	0.02344
8	0.01910	0.02539	0.01667	0.025
9	0.02865	0.02361	0.03125	0.02083
10	0.02148	0.03269	0.02344	0.03077
TOTAL	**0.22046**	**0.257975**	**0.20214**	**0.239361**

Table A1 Standardized average earnings for each position and for each treatment

We can observe substantially the same results. At the individual level, for almost all the position, we can still observe higher average earnings under T2 (except for position 6 and position 9 for theoretical earnings and for position 4 and position 9 for experimental earnings).

At the aggregate level, we can confirm all previous results: the two treatments are statistically different (Wilcoxon rank-sum test for theoretical earnings: -2.043, *p-value* = .0410; for experimental earnings: -1.895, *p-value* = .0581). Still, each experimental treatment could be considered not statistically different from its theoretical counterpart (Wilcoxon rank-sum test for BHW and T1: 1.212, *p-value* = .2256; for our specification and T2: 0.832, *p-value* = .4053).

E – The aim of this section is to study whether our sample may be divided into two sub-samples: the first four positions and the later ones. We intend to do so performing a Chow test. Before performing this test, we need to verify if a linear probability model (LPM) provides a good approximation for

probit estimates (for further details, Wooldridge, 2005). We compare estimates separately from the two models and from each sub-sample in Table A2.

Dep. Variable: Correct	Probit (entire)		Chow Test: LPM (entire)		Chow Test: LPM (pos. 1-4)		Chow Test: LPM (pos. 5-10)	
	Coeff.	*p*-value	Coeff.	*p*-value	Coeff.	*p*-value	Coeff.	*p*-value
Time	.06971	0.177	.02152	0.180	.04592	0.072	.00526	0.799
Time²	-.00266	0.264	-.00083	0.263	-.00160	0.168	-.00030	0.751
Correctwon	-.43707	0.011	-.12912	0.012	-.19156	0.020	-.10992	0.094
Correctlost	-.63673	0.004	-.19950	0.005	-.17815	0.117	-.24002	0.008
T2	.50940	0.000	.15721	0.000	.27921	0.000	.08399	0.135
Constant	.41623	0.086	.65743	0.000	.47044	0.000	.79334	0.000
Pseudo R^2	0.0478							
Adjusted R^2			0.0411		0.0945		0.0196	
NOBs	400		400		160		240	

Table A2 Binary choice models: probit and LPM comparison

It is straightforward to note that LPM provides consistent results with probit estimates (signs of the coefficient are the same across models and the same variables are statistically significant), even if different in magnitude (due to different assumptions on the error terms).

At this point, we can consider the underlying model as linear and, hence, proceed to perform the Chow test. Under the null hypothesis, there should be no significant difference between the two sub-samples. We use the F statistic to test this hypothesis.

We report in table A3 the residual sums of squares for the separate regressions (RSS_1 and RSS_2) and the residual sum of squares of the pooled sample regression. We compare the value of the total residual sum of squares (obtained by summing RSS_1 and RSS_2) with the residual sum of squares from the pooled sample regression.

	Residual Sum of Squares		
Regression	Positions 1-4 (N. 160)	Positions 5-10 (N. 320)	Total (N. 400)
	RSS_1	RSS_2	$RSS_1 + RSS_2$
Separate	27.57763	43.19624	70.77387
Pooled			72.41280
F-test	1.482		

Table A3 The Chow test

The F-statistic is 1.482 and the critical value of F(6, 384) at 5% of significance is 2.10. Hence, we conclude that the pooled regression model is an adequate specification. Consequently, there is no statistical difference across the two sub-samples.

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