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Transaction Costs and Informational Cascades in Financial Markets

Marco Cipriani and Antonio Guarino*

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Abstract

We study the effect of transaction costs (e.g., a trading fee or a transaction tax, like the Tobin tax) on the aggregation of private information in financial markets. We implement a financial market with sequential trading and transaction costs in the laboratory. According to theory, eventually all traders neglect their private information and abstain from trading (i.e., a no-trade informational cascade occurs). We find that, in the experiment, informational no-trade cascades occur when theory predicts they should (i.e., when the trade imbalance is sufficiently high). At the same time, the proportion of subjects irrationally trading against their private information is smaller than in a financial market without transaction costs. As a result, the overall efficiency of the market is not significantly affected by the presence of transaction costs. (JEL C92, D8, G14)

Keywords: Informational Cascades, Herd Behavior, Transaction Costs, Tobin Tax

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1 Introduction

There is a long and widespread debate on the role of transaction costs in the functioning of financial markets. Such costs can be imputed to different reasons, such as brokerage commission fees, time involved in record keeping and securities transaction taxes. It has been argued that transaction costs can have negative effects on the process of price discovery. The presence of even very small costs makes rebalancing expensive. Therefore, valuable information can be held back from being incorporated into prices.

In recent years, such a debate has gained new strength, following the proposals by many policy makers around the world to introduce security transaction taxes (e.g., a Tobin tax),¹ especially in emerging markets. Opponents have stressed how these taxes can reduce the efficiency with which markets aggregate information dispersed among their participants. In contrast, proponents have argued that such taxes only reduce excessive trading and volatility, and can prevent the occurrence of financial crises.²

In this paper we will contribute to this debate by studying the effect of transaction costs in a laboratory financial market. We will focus, in particular, on the role of transaction costs in the process of information aggregation.

We will first present a theoretical model similar to that of Glosten and Milgrom (1985), in which traders trade an asset with a market maker. The market maker sets the prices at which traders can buy or sell. The prices are updated according to the order flow, that is, to the sequence of trades. Traders can buy or sell one unit of the asset or abstain from trading. If they decide to trade, they have to pay a transaction cost.

We will show that the presence of transaction costs has a significant effect on the ability of the price to aggregate private information dispersed among market participants. Transaction costs cause “informational cascades:” situations in which all informed traders neglect private information and abstain from trading.³ Such blockages of information can occur when the price is far away from the fundamental value of the asset. Therefore, transaction costs can cause long lasting misalignments between the price and the funda-

¹In 1978, Tobin proposed that all major countries should introduce a tax on foreign-exchange transactions.

²For a review of this debate, see Habermeier and Kirilenko (2003) and the comment by Forbes (2003).

³For a review of the literature on informational cascades, see Gale (1996), Hirshleifer and Teoh (2003), Chamley (2004), and Vives (2008).

mental value of a security. Not only are informational cascades possible, but they also occur with probability one. Eventually, the trade cost overwhelms the importance of the informational advantage that the traders have on the market maker, and therefore informed agents prefer not to participate in the market, regardless of their private information.

These results contrast with those of Avery and Zemsky (1998), who show that informational cascades cannot occur in financial markets where trade is frictionless. In their work, agents always find it optimal to trade on the difference between their own information (the history of trades and the private signal) and the commonly available information (the history of trades only). For this reason, the price aggregates the information contained in the history of past trades correctly. Eventually, the price converges to the realized asset value. With transaction costs, in contrast, the convergence of the price to the fundamental value does not occur since trading stops after a long enough history of trades. With positive probability, no-trade cascades occur when the price is far from the fundamental value of the asset, even for a very small transaction cost.

To test the theory, we have run an experiment. A laboratory experiment is particularly fit to test the theory since in the laboratory we observe the private information that subjects receive and can study how they use it while trading. In our laboratory market, subjects receive private information on the value of a security and observe the history of past trades. Given these two pieces of information, they choose, sequentially, whether they want to sell, to buy or not to trade one unit of the asset at the price set by the market maker. By observing the way in which they use their private information, we directly detect the occurrence of cascades. The experimental results are in line with the theoretical model. Indeed, cascades form in the laboratory as the theory predicts (i.e., when the trade cost overwhelms the gain to trading).

Nevertheless, even if with trade costs cascades do arise, in the laboratory the ability of the price to aggregate private information is not significantly impaired. This happens because when transaction costs are present, there is a lower incidence of irrational behavior and, in particular, of trading against one's own private information. The higher level of rationality makes the price reflect private information more accurately. This explains why the overall impact of transaction costs on the market efficiency is very small.

Our theoretical analysis lends credibility to the arguments against the introduction of a security transaction tax (like a Tobin tax): it is true that financial frictions impair the ability of prices to aggregate private informa-

tion by making informational cascades possible. The occurrence of cascades is confirmed by the laboratory experiment. As proponents of the tax have suggested, however, financial frictions also reduce the occurrence of irrational behavior, which improves the ability of the price to reflect subjects' private information. These two effects offset each other in the laboratory so that the transaction cost does not significantly alter the financial market informational efficiency.

In the theoretical social learning literature, the impact of transaction costs in financial markets has been first discussed by Lee (1998), who studies a sequential trade mechanism in which traders are risk averse and receive signals of different precision. Traders can trade more than once with the market maker and can buy or sell different quantities (shares) of the asset. Transaction costs trigger cascades followed by informational avalanches in which previously hidden private information is suddenly revealed. Eventually, however, a complete stop of information occurs, which results in long-run misalignments of the price with respect to the fundamental.⁴ In contrast to Lee, we study the effect of transaction costs in a standard market microstructure model à la Glosten and Milgrom. Our theoretical setup is particularly useful for our experimental analysis since it is easy to implement in the laboratory. The theoretical result on the occurrence of cascades in a Glosten and Milgrom model with transaction costs was also shown in independent work by Romano (2007).

There are only few experimental papers that have studied informational cascades in financial markets.⁵ Cipriani and Guarino (2005a and 2005b) and Drehmann et al. (2005) have tested Avery and Zemsky's model in the laboratory and have found that, as theory predicts, informational cascades rarely occur in a laboratory financial market in which traders trade for informational reasons only and there are no transaction costs. More recently, Cipriani and Guarino (2008b) have tested the same model using financial markets pro-

⁴In Lee the complete stop of information arises because agents stop trading. Cipriani and Guarino (2008a) show that a complete blockage of information can also arise when agents herd buy or herd sell, because of traders' heterogeneity (stemming from non-informational reasons to trade, such as liquidity or hedging reasons).

⁵While there are only two experiments on herding and cascades in financial markets, there is a much larger experimental literature testing the original herding models where the price is fixed. Among the others, see the seminal paper by Anderson and Holt (1997) and the papers by Çelen and Kariv (2004), Drehmann et al. (2007), Goeree et al. (2007), Huck and Oechssler (2000), Hung and Plott (2001), Kübler and Weizsäcker (2004) and Weizsäcker (2006).

professionals. Their results confirm the absence of informational cascades in a frictionless financial market. Cipriani and Guarino (2008b) also test a model where, because of informational uncertainty, traders may find it optimal to ignore their private information and herd. They find that such a behavior occurs in the laboratory, but less than theoretically predicted.

The structure of the paper is as follows. Section 2 describes the theoretical model tested in the laboratory. Section 3 presents the experimental design. Section 4 illustrates the experimental results. Section 5 concludes.

2 The Theoretical Analysis

2.1 The model structure

In our economy there is one asset traded by a sequence of traders who interact with a market maker. Time is represented by a countably infinite set of trading dates indexed by $t = 1, 2, 3, \dots$

The market

The fundamental value of the asset, V , is a random variable distributed on $\{0, 100\}$, with $Pr(V = 100) = p$. At each time t , a trader can exchange the asset with a specialist (market maker). The trader can buy, sell or decide not to trade. Each trade consists of the exchange of one unit of the asset for cash. The trader's action space is, therefore, $\mathcal{A} = \{buy, sell, no\ trade\}$. We denote the action of the trader at time t by X_t . Moreover, we denote the history of trades and prices until time $t - 1$ by h_t .

The market maker

At any time t , the market maker sets the price at which a trader can buy or sell equal to the expected value of the asset conditional on the information available at time t :⁶

$$p_t = E(V|h_t).$$

The traders

There are a countably infinite number of traders. Traders act in an exogenously determined sequential order. Each trader, indexed by t , is chosen

⁶We do not allow the market maker to set a bid-ask spread. Since, as will be clear in the next paragraph, the market maker trades with informed traders, he loses in expected value each time he trades. This is not a problem since, in the experiment, an experimenter played the role of the market maker.

to take an action only once, at time t . When a trader is chosen to trade, he observes a private signal on the realization of V . The signal is a random variable S_t distributed on $\{0, 100\}$. We denote the conditional probability function of S_t given a realization v of V by $\sigma(s_t|v)$. We assume that, conditional on the asset value v , the random variables S_t are independent and identically distributed across time. In particular, we assume that

$$\sigma(0|0) = \sigma(100|100) = q > 0.5.$$

In addition to his signal, a trader at time t observes the history of trades and prices and the current price. Therefore, his expected value of the asset is $E(V|h_t, s_t)$.

Trading is costly: if a trader decides to buy or sell the asset, he must pay a transaction cost $c > 0$. Every trader is endowed with an amount $k > 0$ of cash.⁷ His payoff function $U : \{0, 100\} \times \mathcal{A} \times [0, 100]^2 \rightarrow R^+$ is defined as

$$U(v, X_t, p_t) = \begin{cases} v - p_t + k - c & \text{if } X_t = \text{buy}, \\ k & \text{if } X_t = \text{no trade}, \\ p_t - v + k - c & \text{if } X_t = \text{sell}. \end{cases}$$

The trader chooses X_t to maximize $E(U(V, X_t, p_t)|h_t, s_t)$. Therefore, he finds it optimal to buy whenever $E(V|h_t, s_t) > p_t + c$, and sell whenever $E(V|h_t, s_t) < p_t - c$. He chooses not to trade when $p_t - c < E(V|h_t, s_t) < p_t + c$. Finally, he is indifferent between buying and no trading when $E(V|h_t, s_t) = p_t + c$ and between selling and no trading when $E(V|h_t, s_t) = p_t - c$.

It is worth emphasizing the simplifications introduced in this model compared to the original Glosten and Milgrom model. First, we have only informed traders, instead of both informed and noise traders. Second, we allow the market maker to set one price only, instead of two prices, a bid price and an ask price.⁸ Both choices are aimed to make the experiment simple and do not affect the theoretical predictions we test in the laboratory.⁹ In par-

⁷We introduce k in the payoff function only for consistency with the experiment, where subjects receive a fixed payment each time they are called to trade. The amount k should not be interpreted as a cash constraint.

⁸Note that in the absence of noise traders, if we had allowed the market maker to set a bid and an ask price, the equilibrium spread would have been so wide that informed traders would have had no incentive to trade.

⁹In a working paper version of this article (Cipriani and Guarino 2006), we present a similar model, in which, as in Glosten and Milgrom, there are noise traders and the market maker sets two prices, a bid and an ask price. We show that such a model has the same theoretical predictions as those presented here.

ticular, adding noise traders to this setup would have made the experiment more difficult to run without offering additional insights on the efficiency of the market.

Finally, note that in actual financial markets, transaction costs may arise from different reasons such as the material cost of executing the trade, brokerage commission fees or a security transaction taxes. Different types of transaction costs may lead to slightly different modelling choices. For instance, securities transaction taxes are usually levied as a percentage tax on the amount transacted, which would imply modelling the transaction cost not as a lump-sum, but as a percentage. We chose a lump-sum cost in our model because it is the easiest to implement in the laboratory. It is easy to show, however, that the theoretical results that we present in this section and then test in the experiment are independent of this choice.

2.2 Predictions

In order to discuss the model's predictions, let us introduce the concept of informational cascade. An informational cascade is a situation in which it is optimal for a rational agent to make a decision independently of his private information (i.e., to ignore his own private signal).

An informational cascade arises at time t when

$$\Pr[X_t = x | h_t, S_t = s] = \Pr[X_t = x | h_t]$$

for all $x \in \mathcal{A}$ and for all $s \in \{0, 100\}$.

In an informational cascade, the market maker is unable to infer the trader's private information from his actions and to update his beliefs on the asset value. In other words, in an informational cascade trades do not convey any information on the asset's fundamental value.

From a behavioral point of view, an informational cascade can potentially correspond to three different trading behaviors, which are of interest in the analysis of financial markets:

1) Traders may disregard their private information and *conform* to the established pattern of trade (e.g., after many buy orders, they all buy irrespective of their signal). In this case, we say that the traders are *herding*.

2) Alternatively, traders may disregard their private information and trade *against* the established pattern of trade (e.g., after many buy orders,

they all sell irrespective of their signal). In this case, we say that the traders are engaging in *contrarian behavior*.

3) Finally traders may simply *abstain from trading regardless of their private information*. In this case we say that there is a *no-trade cascade*.

We now prove that, in equilibrium, the first two situations never arise: traders will never trade against their signal (i.e., engage in herding or contrarian behavior). In contrast, an informational cascade of no trades arises almost surely as t goes to infinity.

In equilibrium, a) a trader either trades according to his private signal or abstains from trading; and b) an information cascade in which a trader does not trade regardless of his signal occurs almost surely as t goes to infinity.

Let us first discuss point a. To decide whether he wants to buy, to sell or not to trade the asset, an agent computes his expected value and compares it to the price. If at time t he receives a signal of 100, his expected value will be

$$E(V|h_t, S_t = 100) = 100 \Pr(V = 100|h_t, S_t = 100) = \\ 100 \frac{q \Pr(V = 100|h_t)}{q \Pr(V = 100|h_t) + (1 - q)(1 - \Pr(V = 100|h_t))} > \\ 100 \Pr(V = 100|h_t) = E(V|h_t) = p_t.$$

Similarly, if he receives a signal of 0, his expected value will be

$$E(V|h_t, S_t = 0) < E(V|h_t) = p_t.$$

Therefore, an agent will never find it optimal to trade against his private information (i.e., to buy despite a signal 0 or to sell despite a signal 100).

Let us now consider point b: as t goes to infinity, agents decide not to trade with probability 1. We refer the reader to the Appendix for a formal proof. Here let us note that, over time, as the price aggregates private information, the informational content of the signal becomes relatively less important than that of the history of trades. Therefore, after a sufficiently large number of trades, the valuation of any trader and of the market maker will be so close that the expected profit from trading will be lower than the transaction cost. Every trader, independently of his signal realization, will decide not to trade.

Note that when an informational cascade arises it never ends. This happens because, during a cascade, a trade does not convey any information on the asset value. After a cascade has started, all the following traders have the same public information as their predecessor (i.e., nothing is learned during the cascade). Therefore, the subsequent traders will also find it optimal to neglect their own private information and decide not to trade, thereby continuing the cascade.

This result on traders' behavior has an immediate implication for the price during a cascade. The market maker will be unable to update his belief on the asset value and, as a consequence, the price will not respond to the traders' actions. The price will remain stuck forever at the level it reached before the cascade started. Note that such a level may well be far away from the fundamental value of the asset. Therefore, in a market with transaction costs, long-run misalignments between the price and the fundamental value arise with positive probability. This suggests that, as opponents of security transaction taxes have claimed, introducing such taxes can significantly impair the process of price discovery.¹⁰

2.3 Parametrization

To run our experiment, we set particular values for the relevant parameters of the model. In particular, we assumed the probability of the asset value being 100, p , equal to $\frac{1}{2}$, and the precision of the signal, q , equal to 0.7. Finally, we set the trade cost, c , equal to 9, which corresponds to 18% of the unconditional expected value of the asset. This level of the transaction cost is obviously greater than actual transaction costs or than reasonable taxes on financial transactions.¹¹ We chose such a high level so that the probability of

¹⁰Securities transaction taxes are usually levied as a percentage tax on the amount transacted and not as a lump-sum tax. It is easy to show, however, that the theoretical results that we present in this section would also hold if the transaction cost were modeled as a percentage. We chose a lump-sum transaction cost only because it is easier to implement in the laboratory.

¹¹According to Domowitz and Madhavan (2001), in the U.S. stock market, trading costs in the period 1990–1998 were equal to 2.2% of the mean returns. Habermeier and Kirilenko report that in Sweden, between 1984 and 1991, taxes on equity trading were levied at 0.5% and taxes on option trading were levied at 1%. These figures are lower than our cost. One needs, however, to consider that following an informational event the number of trades on a stock is very high. As a result, even low levels of transaction costs or taxes can trigger a cascade.

the transaction cost becoming binding was high enough to offer a sufficient number of observations in our analysis. In theory, the transaction costs become binding with probability 1 irrespective of their size, as the number of trading dates goes to infinity. In the experiment, the number of trading dates is, of course, finite, and this dictated the choice of c .¹²

3 The Experiment and the Experimental Design

3.1 The experiment

We ran the experiment in 2001 in the Department of Economics at New York University. We recruited 104 subjects from the university undergraduate courses in all disciplines.¹³ They had no previous experience with this experiment. We ran two treatments, with four sessions for each treatment. In each session we used 13 participants, one acting as subject administrator and 12 acting as traders. Let us describe here the procedures for the main treatment:

1. At the beginning of the sessions, we gave written instructions (reported in the Appendix) to all subjects. We read the instructions aloud in an attempt to make the structure of the game common knowledge to all subjects. Then, we asked for clarifying questions, which we answered privately. Each session consisted of ten rounds of trading. In each round we asked all subjects to trade one after the other.
2. The sequence of traders for each round was chosen randomly. At the beginning of the session each subject picked a card from a deck of 13 numbered cards. The number that a subject picked was assigned to him for the entire session. The card number 0 indicated the subject administrator. In each round, the subject administrator called the

¹²In our experiment, each sequence of trades consists of 12 decisions, a relatively large number compared to similar studies in the literature.

¹³Subjects were recruited by sending an invitation to a large pool of potential participants. For each session of the experiment, we received a large number of requests to participate. We chose the students randomly so that the subjects in the experiment were unlikely to know each other.

subjects in sequence by randomly drawing cards (without replacement) from this same deck.

3. Before each round, an experimenter, outside the room, tossed a coin: if the coin landed tails, the value of the asset for that round was 100, otherwise it was 0. Traders were not told the outcome of the coin flip. During the round, the same experimenter stayed outside the room with two bags, one containing 30 blue and 70 white chips and the other 30 white and 70 blue chips. The two bags were identical. Each subject, after his number was called, had to go outside the room and draw a chip from one bag. If the coin landed tail the experimenter used the first bag, otherwise he used the second. Therefore, the chip color was a signal for the value of the asset. After looking at the color, the subject put the chip back into the bag. Note that the subject could not reveal the chip color to anyone.
4. In the room, another experimenter acted as market maker, setting the price at which subjects could trade. After observing the chip color, the subject entered the room. He read the trading price on the blackboard and then declared aloud whether he wanted to buy, to sell or not to trade. The subject administrator recorded all subjects' decisions and all trading prices on the blackboard. Hence, each subject knew not only his own signal, but also the history of trades and prices.¹⁴
5. At the end of each round (i.e., after all 12 participants had traded once) the realization of the asset value was revealed and subjects were asked to compute their payoffs. All values were in a fictitious currency called lira. In each round students were given an endowment of 100 lire. Each time a student decided to buy or sell the asset, he would have to pay a transaction cost of 9 lire. Therefore, students' payoffs were computed as follows. In the event of a buy, the subject obtained $100 + (v - p_t) - 9$ lire; in the event of a sell, he obtained $100 + (p_t - v) - 9$ lire; finally, if he decided not to trade he earned 100 lire. After the tenth round, we summed up the per round payoffs and converted them into dollars at the rate of $\frac{1}{65}$. In addition, we gave \$7 to subjects just for participating

¹⁴Subjects were seated far away from each other, all facing the blackboard. No communication was allowed in the room. The entrance was in the back of the classroom. When making his decision, the subject was facing the blackboard, but not the other participants.

in the experiment. Subjects were paid in private immediately after the experiment and, on average, earned \$22.50 for a 1.5 hour experiment.

3.2 The Price Updating Rule

The price was updated after each trade decision in a Bayesian fashion. When a subject decided to buy, the price was updated up, assuming that he had seen a positive signal. Similarly, when a subject decided to sell, the price was updated down, assuming that the subject had observed a negative signal. In the case of no trade, the price was kept constant. The rationale for this rule is very simple. In equilibrium, when the trade cost was smaller than the expected profit from buying or selling the asset, subjects should have always followed their signal (i.e., they should have bought after seeing a positive signal and sold after seeing a negative one). On the other hand, not trading was an equilibrium decision when the expected profit from buying or selling the asset was not higher than the trade cost. Therefore, in equilibrium, a buy would reveal a positive signal, a sell a negative signal, and a no trade would be uninformative.¹⁵

Of course in the experiment we could observe decisions off the equilibrium path. This could happen in two sets of circumstances: 1) a subject decided not to trade when in equilibrium the trading cost was not binding; or 2) a subject decided to buy or sell even though the trade cost overwhelmed the gain to trading. In the first case, we left the price constant, whereas in the second we updated the price after a buy (sell) as if the subject had received a positive (negative) signal. As a result, independently of whether the observed decision was on the equilibrium path, after a buy (sell) the price was always updated as if it revealed a positive (negative) signal; after a no trade it was always kept constant.

In the instructions, we explained to the subjects that the price would move up after a buy, down after a sell, and would remain constant after a no trade. We added that the amount by which we would change the price would

¹⁵The price updating rule is the same as that used by Cipriani and Guarino (2005a) and Drehmann et al. (2005) in their benchmark treatments for an economy with no transaction costs. Both Cipriani and Guarino and Drehmann et al. show that the results are robust to changes to the price setting mechanism. Cipriani and Guarino show that the results are robust to the case in which the price is set by subjects acting as market makers. Drehman et al. show that they are robust to the case in which prices are set by an automaton taking into account that subjects' behavior deviates from the equilibrium predictions.

depend on the “working of competitive markets: the price at each turn is the best guess of the good value given the decisions taken by the subjects in the previous turns.”¹⁶ It is worth mentioning that, with our price setting rule, the price moved through a grid, that is, there were only few values at which it was set during the entire experiment. The price movement on the grid depended only on the difference between the number of buys and sells. After a series of buys the price moved from 50 to 70, 84, 93, 97,...; after a series of sells it moved from 50 to 30, 16, 7, 3,...; and after, for instance, two buys and a sell, it would be set equal to 70. This made the updating rule very easy for the subjects to understand. During the first three rounds (which we do not consider in our data analysis), they had the opportunity to learn how the price moved in response to the order flow.

It is important to remark that we chose a trade cost of 9 to avoid the possibility that the cost could be binding only upon receiving a negative signal and not upon receiving a positive one (or vice versa). To understand this point, consider a subject facing a price of 84. He would be (theoretically) indifferent between buying and not trading upon receiving a positive signal and would strictly prefer to sell upon receiving a negative one.^{17 18} Had we chosen another level of the trade cost (for instance 10), theoretically a no trade would have clearly revealed that the subject received a positive signal (since he could never decide not to trade with a negative signal). To be consistent with the theoretical framework, we should have updated the price, but this would have made the updating rule quite complicated and difficult to explain to the subjects.¹⁹

¹⁶Drehmann et al. show that, in an economy with no trade costs, the results are robust to different presentations of the price updating rule (for instance, presenting a table with all the possible prices instead of explaining the rule to subjects in the instructions).

¹⁷When the price is 84, a trader with a negative signal has an expected value of 70 and therefore would make an expected profit of $84 - 70 - 9 = 5$ lire by selling the asset. In contrast, a trader with a positive signal has an expected value of 93 and therefore would make an expected profit of $93 - 84 - 9 = 0$ lire by buying the asset.

¹⁸Note that in the computation of prices, expectations and payoffs, we rounded off values to the one. We chose this rounding since 1 lira was approximately equivalent to 1.5 cents of a US dollar.

¹⁹Keeping the price constant after a no trade is optimal for the market maker only if we impose some assumptions on the subjects’ behavior in the case of indifference. Not updating the price after a no trade at the price of 84 is equivalent to assuming that an indifferent subject in this case buys the asset, so that a no trade always is an off-the-equilibrium path decision and does not convey any information on the signal that the subject received.

3.3 The Experimental Design

To study the effect of transaction costs we compared the results of the treatment described above with a control treatment, identical in everything but for the fact that trading was costless. From now on, we will refer to the main treatment as the Transaction Cost (TC) treatment and to the other treatment as the Control (CO) treatment. The data for the CO treatment are taken from Cipriani and Guarino (2005a), who use them to study herd behavior in financial markets.²⁰

The results described in the next section refer to the last seven rounds of each session only.²¹ Although the experiment was very easy and subjects did not have problems in understanding the instructions, we believe that some rounds were needed to acquaint subjects with the procedures. By considering only the last 7 sessions, we concentrate on the decisions taken after the learning phase.²²

Similarly, at a price of 93, a subject with a negative signal would value the asset 84 and, therefore, would be indifferent between selling and no trading. A subject with a positive signal would value the asset 97 and, because of the trade cost, would strictly prefer to abstain from trading. Not updating the price after a no trade at a price of 93 is equivalent to assuming that an indifferent subject in such a case does not trade, so that a no trade does not convey any information on the private signal.

Considerations similar to those above can be made for no trade decisions at prices of 16 or 7.

The assumptions we made in the cases of indifference, theoretically legitimate, turn out to be fairly consistent with actual behavior in the laboratory. Indeed, over the whole experiment, the frequency of a no trade conditional on receiving a bad signal was very close to the probability of a no trade conditional on receiving a good signal (52% and 51% respectively), which implies that a rational market maker would have not updated the price after a no trade. Given subjects' behavior, for a price of 84, 93, 16 and 7, after a no trade, a market maker should have updated the price to 86, 93, 20 and 7, very close to our updating rule.

²⁰Cipriani and Guarino ran several treatments with a flexible price and no transaction costs. The data used here are those described in that paper under the label of Flexible Price treatment. The data from that treatment are an obvious control for the TC treatment since the experiment setup differs only in the absence of transaction costs (whereas the other treatments in Cipriani and Guarino also differ in other dimensions). Note that both the TC and the CO treatments were run in the same period with similar procedures and with a similar pool of students.

²¹In each round, the 12 subjects were asked to trade in sequence. Therefore, the results refer to 336 decisions per treatment.

²²We replicated the analysis considering all ten rounds and obtained results (available in the online appendix to the article) similar to those described in the remainder of the

4 Results

4.1 Trading Behavior

Let us start the presentation of our results by discussing the average level of rationality in the experiment. Given the sequential structure of the game, to classify a decision as rational or irrational, we need to make some assumptions on each subject's belief on the choices of his predecessors. Following Cipriani and Guarino (2005a) we define rationality by assuming that each subject believes that all his predecessors are rational, that all his predecessors believe that their predecessors are rational, and so on. Under this assumption, a rational subject should always behave as predicted by the theoretical model.²³

The level of rationality in the TC treatment is high (see Table 1): 82% of the overall decisions in the laboratory were rational (i.e., not in contrast with the theoretical model), while only 18% of actions were irrational. Such a level of rationality is higher than the 65% in the CO treatment.²⁴ This increase in the level of rationality is mainly due to the drop in the proportion of irrational no trades. In the CO treatment subjects should have always traded in order to exploit their informational advantage with respect to the market maker. Nevertheless, they decided not to trade in 22% of the cases, which added to the level of irrationality. In the TC treatment, the proportion of no trades was significantly higher (51%).²⁵ These no trade decisions, however, happened mostly when they should have occurred according to theory: indeed, 79% of

paper.

²³In the experiment, subjects sometimes made decisions off-the-equilibrium path (i.e., decisions that could not be the outcome of a rational choice). An important issue is how to update subjects' beliefs after they observe such decisions. If the decision is a no trade, we assume that the following subjects do not update their beliefs (which is consistent with our price updating rule). If the decision is a buy (sell), we assume (again, consistently with our price updating rule) that the following subjects update their beliefs as though it publicly revealed a positive (negative) signal (i.e., the signal that implies the lower expected loss). This last assumption is, in fact, quite innocuous. For instance, if we assumed that trades that cannot be the outcome of a rational choice do not convey any information, our results would be virtually identical since, in the TC treatment, we observed only four such trades (out of 336 decisions).

²⁴According to the Mann-Whitney test, the proportion of rational trades is significantly different in the two experiments (p-value=0.03). Throughout the paper, the independent observations for the Mann-Whitney tests are the per-session averages.

²⁵The p-value for the null that the proportion of no trades is the same in the two experiments equals 0.03 (Mann-Witney test).

no trades occurred when the difference between the trader's expected value and the price was not higher than the transaction cost and, therefore, trading could not be profitable.

Table 1: Rational and irrational decisions

TC treatment			
<i>Rational Decisions</i>			<i>82%</i>
	Buying or Selling	41%	
	No Trading	41%	
<i>Irrational Decisions</i>			<i>18%</i>
	Buying or Selling	8%	
	No Trading	10%	
<i>Total</i>			<i>100%</i>
CO treatment			
<i>Rational Decisions</i>			<i>65%</i>
<i>Irrational Decisions</i>			<i>35%</i>
	Buying or Selling	13%	
	No Trading	22%	
<i>Total</i>			<i>100%</i>

To understand better the relationship between no trading and rational behavior, we computed the proportion of no trade decisions for different levels of the absolute value of the trade imbalance. The trade imbalance at time t is defined as the difference between the number of buys and the number of sells taken by subjects from time 1 until time $t - 1$. As the trade imbalance increases in absolute value, the expected profit from trading becomes smaller and smaller, thus reducing traders' incentives to trade upon their information.

Table 2:
**Proportion of no-trade decisions for different levels of the
 absolute value of the trade imbalance**

TC treatment	
Trade Imbalance	No Trades
0-1	21%
2-3	67%
>3	78%
CO treatment	
Trade Imbalance	No Trades
0-1	16%
2-3	22%
>3	33%

Both in the TC and in the CO treatment, the frequency of no trades increases monotonically with the absolute value of the trade imbalance. In the CO treatment, however, this increase is modest, the proportion of no trades going up from 16% with a trade imbalance between -1 and 1 to 33% with a trade imbalance greater than 3 or smaller than -3 . In contrast, in the TC treatment the proportion of no trades jumps from 21% when the absolute value of the trade imbalance is at most 1 to 67% when the absolute value is 2 or 3 . The proportion of no trades is even higher when the trade imbalance is higher than 3 or lower than -3 . Since, in the TC treatment, for a trade imbalance of at most 1 , not trading was irrational, while it was rational after a trade imbalance of 2 , these results confirm that no trade decisions occurred mainly when they were rational. In a nutshell, the trade cost had a significant impact on subjects' decisions exactly when theory suggests that it should become binding.²⁶

²⁶In an earlier working paper version of their article, Drehmann et al. report the results of a treatment with transaction costs. As do we, they find that transaction costs increase the proportion of no trades (although to a much lower extent). In contrast to our results, however, in their experiment the proportion of rational no trades is modest. In particular, they find that, out of 26% of no trades, only 12% are rational. It is not easy to compare our results to theirs since they ran several transaction cost treatments with various parameter values (different from ours) and report aggregate statistics for all treatments. Furthermore, in their experiments, the size of the transaction cost is smaller than in ours (they ran two

Rational no trade decisions are not the only reason for the increase in the level of rationality in the TC treatment as compared to the CO treatment. The increase in the level of rationality is also due to a different proportion of trading against private information. In the CO treatment, 13% of all decisions are irrational buys and sells against private information. In contrast, in the TC treatment only 6% of decisions are irrational trades against the private signal.²⁷

Following Cipriani and Guarino (2005a), we can explain part of the irrational buying or selling decisions in the CO treatment in terms of contrarian behavior and (a modest proportion of) herd behavior. Herding refers to the situation in which a trader with a signal in contrast with the past history of trades (i.e., a positive signal after many sells or a negative signal after many buys) decides to disregard his private information and follow the market trend.²⁸ In contrast, contrarianism refers to the situation in which a trader with a signal that reinforces the past history of trades (i.e., a positive signal after many buys or a negative signal after many sells) decides to disregard both his private information and the market trend.²⁹ In Table 3 we report the proportion of herding and contrarian behavior. In the CO treatment, subjects herded 12% of the time in which herding could arise, and they acted as contrarians in 19% of the time in which contrarianism could occur. In contrast, in the TC treatment, herding and contrarianism occurred

sets of treatments, with a 1% and a 5% transaction cost). It should be noted that a transaction cost of 1% or 5% becomes binding only when the absolute trade imbalance is quite high (4 and 6 respectively), but this seldom happens if there are relatively few trades in each round. Drehmann et al. (2002) attribute the high percentage of irrational no trades to a psychological effect that makes subjects abstain from trade even when it would not be rational.

²⁷Using the Mann-Whitney test, the p-value for the null that the proportion of trades against private information is the same in the two experiments equals 0.03. Note that the 6% indicated in the text differs from the percentage (8%) in Table 1 since 2% of trades were irrational not because they did not agree with private information, but because the trade cost was greater than the expected profit from trading.

²⁸In particular, following Cipriani and Guarino, we say that in period t there is a situation of potential herd behavior when the trade imbalance (in absolute value) is at least 2 and the subject receives a signal against it. If the subject trades against the signal we say that he herds.

²⁹In particular, following Cipriani and Guarino, we say that in period t there is a situation of potential contrarian behavior when the trade imbalance (in absolute value) is at least 2 and the subject received a signal agreeing with it. If the subject trades against the signal, we say that he engages in contrarian behavior.

in only 7% of the cases in which they could have happened; indeed, in most of these cases subjects preferred not to trade.³⁰ This explains why both contrarianism and herding are even less pronounced in the TC than in the CO treatment, which in turn explains why the proportion of irrational trades is lower.^{31 32}

Table 3: Herding and Contrarian Behavior

Herd Behavior	TC treatment	CO treatment
Herding	7%	12%
Following Private	29%	46%
No Trade	64%	42%
Relevant cases	70	66
Contrarian Behavior		
Contrarian behavior	7%	19%
Following private	23%	63%
No trade	69%	18%
Relevant cases	147	132

4.2 No-trade Cascades

Table 4 shows the sequences of traders' decisions in the TC treatment. We highlighted in gray those periods in which a subject took a decision facing a

³⁰Note that, according to the Mann-Whitney test, the proportion of contrarianisms is significantly different in the two treatments (p -value = 0.03), but the proportion of herding is not (p -value=0.26). This result confirms the finding by Cipriani and Guarino that in the presence of the price mechanism herd behavior rarely arises.

³¹It is also interesting to note that when the trade imbalance (in absolute value) was at most 1 (and, therefore, the trade cost was not binding), irrational decisions to buy or sell amount to 4%, whereas they amount to 9% in the CO treatment. Although this difference is not significant (p -value=0.14), it seems to suggest that, with trade costs, subjects are more careful in making their decisions (and, therefore, they use their information more efficiently). This may have added to the rationality in the TC treatment.

³²In contrast to our results, in Drehmann et al. (2002) the proportion of irrational buying and selling against one's own signal is not significantly reduced by the presence of transaction costs. The reason may be that in their experiments, since transaction costs are lower, the overall proportion of buying and selling on the total number of decisions decreases with respect to the treatment with no trade costs, but only slightly (from 81% to 77% or 72%, for a transaction cost of 1% or 5% respectively). As a result, the proportion of rational buy and sell orders is also not affected substantially by the transaction cost.

price greater than or equal to 93 or smaller than or equal to 7 and, therefore, could have rationally chosen not to trade regardless of his private information. In bold we indicated those decisions that were indeed no trades.

The table clearly illustrates the pervasiveness of no trade decisions. In many rounds (19 out of the 28) there were indeed long sequences of no trades. In almost all the cases, the no trade decisions started when the price reached the level of 7 or of 93. In four rounds only ($IV - 4$, $IV - 5$, $IV - 7$, $IV - 8$) sequences of no trades started at a price of 16 or 84 and, even more rarely, at prices closer to 50, the unconditional expected value of the asset.³³

[Insert Table 4 about here]

We must stress that our experiment offers a particularly *tough* test of the prediction that, when trade is costly, cascades of no trades occur in the financial market. At a price of 7 or 93, the expected payoff from trading becomes equal to or lower than zero, depending on the signal that the subject receives. At the price of 93, a rational agent receiving a negative signal has an expected value of 84 and is *indifferent* between selling and not trading (since the trade cost is set at 9).³⁴ Analogously, when the price is 7, a rational agent receiving a positive signal has an expected value of 16 and is *indifferent* between buying and not trading.³⁵ Therefore, in our setup, deviations from a cascade of no trades are not necessarily irrational. A no trade cascade, as defined in Section 2, theoretically occurs at prices of 7 and 93 under the assumption that an indifferent agent always decides not to trade. In the laboratory, however, we may expect some subjects to follow a different (and, still rational) strategy, thus breaking the cascade. Furthermore, it must be noticed that at a price of 84 a rational agent receiving a positive signal is indifferent between buying and not trading. Therefore, theoretically, the price of 93 can only be reached if an indifferent agent buys with positive

³³The Roman numeral denotes the session. The Arabic numeral indicates the round within each session. For instance, $II - 5$ refers to the fifth round of the second session. Note that we are reporting only the last 7 rounds of each session of the experiment. This is why, for instance, the first round in session II is labeled $II - 4$.

³⁴In contrast, a rational agent receiving a positive signal has an expected value of 97, and, therefore, his optimal choice is to abstain from trading.

³⁵In contrast, a rational agent receiving a negative signal has an expected value of 7, and, therefore, his optimal choice is to abstain from trading.

probability. Similarly, a no trade cascade can arise at the price of 7 only if an indifferent agent at the price of 16 sells with positive probability. In summary, in our set up, cascades are more difficult to form and easier to break.

What happened in the experiment? Given the signal realizations, if all subjects had been rational and followed the rules indicated in the previous section in the cases of indifference, a no trade cascade would have occurred in 25 of the 28 rounds.³⁶ In fact, in the TC treatment sequences of no trades did occur in 19 out of these 25 rounds. Sequences of no trades often arose in the laboratory and were almost never broken. Therefore, the theoretical prediction of the model finds strong support in the laboratory.

4.3 The Price Path

Let us now discuss how the subjects' behavior affected the price path. We are particularly interested in studying the ability of prices to aggregate private information in the laboratory. To this aim, we computed the distance between the final actual price (i.e., the price after all subjects have traded) and the full information price (i.e., the price that the market maker would have chosen if the signals had been public information). Figures 1 and 2 show this distance in the TC treatment and contrast it to that in the CO treatment. Two interesting differences emerge. First, in the TC treatment we have one instance in which distance between the final price and the full information price was greater than 50. This was the instance in which a misdirected cascade arose (i.e., the full information price, 84, was above the unconditional expected value, whereas the actual price, 16, was below it). This never happened in the CO treatment. Second, in the CO treatment 50% of the time the difference between the final price and the full information price was less than 5 lire, whereas this happens only 21% of the time in the TC treatment. Since with trade costs subjects often stopped trading whenever the price was 7 or 93, it was less common for the difference between the full information price and the final price to become very small.

In the TC treatment the average distance between the final actual price and the full information price is 14.5 lire. Had subjects behaved in a perfectly

³⁶In particular, this is true under the assumption that an indifferent agent buys (sells) with probability 1 when the price is 84 (16) (which is the assumption used to compute the equilibrium price).

rational manner, such a distance would have been 14 lire.³⁷ This reflects the similarity between the behavior observed in the laboratory and that predicted by the theory.

In contrast, in the CO treatment, the distance between final actual prices and full information prices is 12 lire. Theoretically, however, with no trade costs the distance should have been 0. Therefore, in the CO treatment, there was a misalignment of the price with the fundamental value of the asset (which is expressed by the full information price) due to the irrational trades in the laboratory. In contrast, in the TC treatment, the inability of the price to aggregate private information completely cannot be attributed to a significant discrepancy between theoretical and actual behavior: indeed the level of irrationality (and, more specifically, the proportion of trades against private information) is significantly lower than in the CO treatment. The distance between actual and full information prices is due to the presence of transaction costs, which reduced the incentive of subjects to reveal their private information by placing orders on the market.

In conclusion, the overall effect of transaction costs on the informational efficiency of prices is ambiguous: on the one hand, they reduce the incentive for subjects in the laboratory to trade irrationally against their private information; on the other hand, they increase (both theoretically and experimentally) the incentive to abstain from trading altogether. In our experiment, these two forces offset each other, and, as a result, the ability of prices to aggregate private information is not significantly different from the case of a frictionless market such as that implemented in the CO treatment.

These results help to shed some light on the possible effect of a tax on financial transactions such as a Tobin tax. It has been argued by opponents of the tax that such a tax would generate misalignments of asset prices with respect to the fundamentals. Our theoretical analysis supports this view: by introducing a wedge between the expectations of the traders and of the market maker, a tax on financial transaction may prevent the aggregation of the private information dispersed among market participants. The occurrence of cascades is confirmed by the laboratory. On the other hand, as proponents of the tax have suggested, a tax on financial transaction reduces the incidence

³⁷This theoretical distance is obtained using, for the cases of indifference, the same assumptions explained above when we discussed informational cascades. An alternative way of computing the last price would be to assume that when indifferent, subjects always followed their signals. Under this assumption, the average distance would have been 10 lire.

of irrational trading by market participants and, in particular, of herding and contrarianism. These two effects offset each other in the laboratory so that the introduction of a security transaction tax does not significantly alter the ability of the price to aggregate private information.

5 Conclusions

We have analyzed the effects of transaction costs in financial markets through a laboratory experiment. We observed cascades in our laboratory market when the theory predicts that they should indeed arise. Informational cascades impair the process of information aggregation and may create a misalignment between the price and the fundamental value of an asset. In this sense, our results highlight the negative effect of transaction costs on the process of price discovery. Our experimental results, however, suggest that one should be cautious in concluding that transaction costs have a strong effect on the informational efficiency of the financial market. In fact we found that the presence of a transaction cost does not affect the convergence of the price to the fundamental value in a significant way. This is due to the fact that transaction costs reduce the frequency with which agents irrationally trade against their private information.

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7 Appendix

7.1 Proof of Proposition 1

To prove the proposition, we show that there exists a time T such that for any $t > T$, the following two inequalities will be satisfied almost surely:

$$E[V|h_t] - c < E[V|h_t, S_t = 0] \quad (A1)$$

and

$$E[V|h_t, S_t = 100] - c < E[V|h_t]. \quad (A2)$$

When these two inequalities are satisfied, a trader chooses not to trade independently of his signal (i.e., we have an informational cascade where all traders abstain from trading). It is easy to prove that (A1) and (A2) are satisfied if and only if $p_t < a$ or $p_t > b$, where $0 < a < 50$ and $50 < b < 100$.³⁸ Furthermore, since the price is a conditional expected value, it is a bounded martingale with respect to the history of trades.³⁹ As a result, it will converge

³⁸We omit the proof, which is a simple application of Bayes's rule. The reader interested in a more detailed proof of the no-trade cascade result (in a model with noise traders and a bid-ask spread) will find it useful to read the working paper version of this article (Cipriani and Guarino 2006).

³⁹Indeed, $E(P_{t+1}|H_t) = E(E(V|H_{t+1})|H_t) = E(V|H_t) = P_t$.

to a random variable as t goes to infinity. Let us denote this random variable by P_∞ .

We now prove the proposition by contradiction. Let us assume that the price converges to a value in the interval $[a, b]$. There are three possibilities:

a) the price converges to a value such that both a trader with a high and one with a low signal follow their private information. In this case, the price is updated as in a market without transaction costs. Therefore, after a buy or sell the price is updated up or down of an amount strictly greater than $\varepsilon > 0$, a contradiction.⁴⁰

b) the price converges to a value such that traders with a high signal buy and traders with a low signal abstain from trading. In this case, the market maker will update the price after a no trade as if it were a sell. The same argument presented in point a) holds.

c) the price converges to a value such that traders with a low signal sell and traders with a high signal abstain from trading. In this case, the market maker will update the price after a no trade as if it were a buy. The same argument presented in point a) holds.

Therefore, the realizations of P_∞ must belong either to the interval $(0, a)$ or $(b, 100)$, which proves the proposition.

⁴⁰We omit the proof, which is a simple application of Bayes's rule.

Table 4: No Trade Cascades

Session I												
IV	S	B	B	B	S	S	B	B	S	B	S	S
V	NT	S	S	NT	NT	B	S	B	S	B	B	B
VI	B	B	NT	S	S	NT	B	B	NT	NT	B	NT
VII	B	B	B	NT	NT	NT	NT	NT	NT	NT	NT	NT
VIII	B	NT	B	B	B	S	S	NT	B	NT	NT	S
IX	NT	B	S	B	B	S	NT	S	B	B	NT	NT
X	NT	B	B	B	S	B	NT	B	NT	NT	NT	NT
Session II												
IV	B	NT	NT	B	S	NT	S	NT	S	S	NT	NT
V	B	B	B	NT	NT	NT	NT	NT	NT	NT	NT	NT
VI	B	B	B	NT	NT	NT	NT	NT	NT	NT	NT	NT
VII	B	S	NT	NT	S	S	S	NT	NT	NT	NT	NT
VIII	S	S	NT	S	NT	NT	NT	NT	NT	NT	NT	NT
IX	B	B	B	NT	NT	NT	NT	NT	NT	NT	NT	NT
X	B	B	B	NT	NT	NT	NT	NT	NT	NT	NT	NT
Session III												
IV	S	S	S	NT	NT	NT	NT	NT	NT	B	B	B
V	S	B	S	S	S	NT	B	S	NT	NT	NT	NT
VI	S	S	B	S	B	S	S	NT	NT	NT	NT	NT
VII	S	S	S	NT	NT	B	NT	S	NT	NT	NT	NT
VIII	B	B	B	NT	S	B	NT	NT	NT	NT	NT	NT
IX	S	S	S	B	S	NT	NT	NT	S	NT	NT	NT
X	S	S	NT	S	NT	NT	NT	NT	NT	B	NT	S
Session IV												
IV	NT	B	B	B	NT	S	B	S	NT	NT	NT	NT
V	S	S	S	S	B	B	NT	NT	NT	S	NT	NT
VI	NT	NT	NT	NT	S	S	B	S	S	NT	NT	NT
VII	S	B	NT	B	NT	S	S	S	NT	NT	B	NT
VIII	B	B	NT	NT	NT	NT	NT	NT	NT	B	NT	NT
IX	B	NT	S	NT	NT	B	NT	B	B	S	NT	B
X	NT	B	S	B	B	B	S	NT	NT	B	S	B

Figure 1: Distance between the theoretical and the actual last prices in the CO treatment

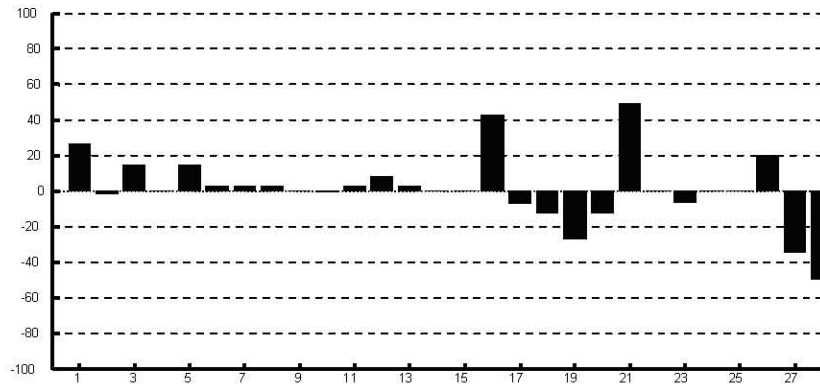


Figure 2: Distance between the theoretical and the actual last price in the TC treatment

