Why Robots Failed
Demonstrating the Superiority of Multiple-order Trading Agents in Experimental Human-agent Financial Markets

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Abstract: In the past decade there has been a rapid growth of the use of adaptive automated trading systems, commonly referred to in the finance industry as "robot traders": AI applications replacing highly-paid human traders in the global financial markets. The academic roots of this industry-changing deployment of AI technologies can be traced back to research published by a team of researchers at IBM at IJCAI 2001, which was subsequently replicated and extended by De Luca and Cliff at IJCAI 2011 and ICAART 2011. Here, we focus on the order management policy enforced by Open Exchange (OpEx), the open source algorithmic trading system designed by De Luca, for both human and robot traders: while humans are allowed to manage multiple orders simultaneously, robots only deal with one order at the time. We hypothesise that such unbalance may have strongly influenced the victory of human traders over robot traders, reported in past studies by De Luca et al., and by Cartlidge and Cliff. We employed OpEx to implement a multiple-order policy for robots as well as humans, and ran several human vs. robot trading experiments. Using aggregated market metrics and time analysis, we reached two important conclusions. First, we demonstrated that, in mixed human-robot markets, robots dealing multiple simultaneous orders consistently outperform robots dealing one order at a time. And second, we showed that while human traders outperform single-order robot traders under specific circumstances, multiple-order robot traders are never outperformed by human traders. We thus conclude that the performance of robot traders in a human-robot mixed market is strongly influenced by the order management policy they employ.

1 INTRODUCTION

At IJCAI 2001 (Das et al., 2001), a team of researchers from IBM demonstrated that software automated trading agents could consistently outperform human traders in a real-time asynchronous continuous double auction (CDA) market.

The CDA is the market mechanism widely adopted by the majority of modern financial electronic exchanges; in a CDA, traders can asynchronously post bids and offers that are usually publicly visible by every other trader, and a trade is made whenever the outstanding bid price is greater than or equal to the outstanding ask price.

Relevant to real world financial markets, IBM's stunning results triggered worldwide media coverage and in the 13 years since then, such automated trading systems have become commonplace in the world's major financial markets, where they are colloquially known as "trading robots" or "robot traders".

At ICAART 2011 (De Luca and Cliff, 2011a) and IJCAI 2011 (De Luca and Cliff, 2011b) De Luca and Cliff replicated and extended IBM's results for the first time since they were presented in 2001: they found that Vytelingum's "Adaptive Aggressive" (AA) trading robot (Vytelingum et al., 2008; Vytelingum, 2006) dominated both Cliff's "Zero Intelligence Plus" (ZIP) (Cliff, 1996), and IBM's GDX, designed by Tesauro and Bredin (Tesauro and Bredin, 2002); and they confirmed that the three robot traders, AA, GDX and ZIP, perform consistently better than human agents.

The human vs. robot experiments conducted by De Luca and Cliff adopt a methodology that resembles closely the one described in the IBM study. An asynchronous real-time electronic trading system

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featuring a centralised exchange and remote graphical trading terminals was employed. Each experiment had a fixed duration, and pitted 6 robot traders against 6 human traders; both robots and humans were equally split into 3 buyers and 3 sellers. Experiments simulated *sales trading*: real world sales traders aim at maximising their own profit, which is the sum of the commission they charge their clients for every sale or purchase they execute, on their behalf, of a specific amount of a certain commodity at a given price. In the simulated sales trading sessions, a definite part of the automated experimental economics system simulated the agents' clients, and communicated to the agents (both human and robot) the clients' will of buying or selling the virtual commodity, and the numeric values of quantity and price; De Luca and Cliff refer to such instructions as *assignments*, and to the predetermined sequence of assignments distributed to each agent in the course of an experiment as the *schedule* of that agent. At the start of the experiment, the system released the first assignment in the agent's schedule; the agent will start trading such assignment; when (and if) the assignment is traded, the system distributes the second assignment to the agent, and so on, until there are no more assignments left for that agent, or the experiment time is up.

De Luca et al. (De Luca et al., 2011) subsequently ran further human vs. robot (ZIP, AA) experiments on a more realistic market previously explored by Cliff and Preist (Cliff and Preist, 2001): in it, the schedule of each agent still consisted of a fixed number of clients' instructions, but the instructions were periodically released at predetermined times, until the market simulator was stopped. To use Cliff and Preist's nomenclature, we will call such timed instructions *permits*, and we will refer to markets operating on a permit schedule as *continuous-replenishment*, or simply *continuous* markets. Crucially, unlike assignments, permits are released regardless of whether or not the agent has finished trading the previous permit: they are triggered solely by time. Yet, the results De Luca et al. found were strongly contrasting with those obtained previously: indeed, humans outperformed robots in the continuous market simulated in (De Luca et al., 2011), although the victory was not as manifest as that shown in (Das et al., 2001).

The finding of such a trading performance unbalance in favour of humans was as controversial as unexpected: first, because the preceding two human vs. robot trading agents studies showed the undisputed victory of robots; second, because with the realism added by the novel continuous-replenishment mechanism, one would anticipate a scenario closer to the real-world, where the use of automated traders is wide spread because of their excellent performance; and third, because, based on common sense, one would generally expect machines to be better than humans at numerical tasks such as trading.

The matter was later studied by Cartlidge and Cliff (Cartlidge and Cliff, 2012; Cartlidge and Cliff, 2013), who confirmed that, in a market continuously replenished of currency and stock, human traders perform better than robot traders (AA).

Also, Cartlidge and Cliff revealed an undesired behaviour in OpEx's AA implementation, for which AA robot buyers (sellers) would systematically trade with the seller (buyer) offering (bidding) the best price, whenever the difference between the two outstanding bid and ask prices, divided by the mean of the two outstanding prices, dropped below a fixed threshold. In this context, it is useful to recall that in a CDA the outstanding bid price and ask price are often referred to as *best bid* and *best ask*; the difference between the best ask and the best bid is what is commonly called *spread*; and we refer to the spread divided by the mean of the best prices as *relative spread*. AA's behaviour is then usually referred to as *crossing the spread* or *jumping the spread*.

Thus, in further experiments, Cartlidge and Cliff pitted human traders against robot traders implementing a revised version of the AA strategy: one that was free of the unwanted spread-jumping behaviour[1]. They found that, under those conditions, robots performed better than humans, thus concluded that the spread-jumping bug caused the robot traders to perform worse, both in their experiments, and in De Luca et al.'s previous work (De Luca et al., 2011). Indeed, the reassuring victory of robot traders over human traders that they obtained in their experiments was the most recent finding on mixed human/robot agents experimental financial markets at the time we wrote this paper.

We have seen how, in continuous markets, all players receive permits to buy or sell continuously throughout the simulation. We will call *orders* the instructions sent by the trading agents (human and robot) to the market; agents send orders to the market to trade the permits they receive from their clients: a new order is first sent to the market, and can then be amended (i.e. its quantity and price can be modified), or canceled (i.e., removed from the market). Here, we

[1] In reality, the AA strategy would still jump the spread methodically, but the minimum value of the relative spread that triggered the aggressive behaviour had been reduced considerably, with respect to the value previously used. For more details on the spread-crossing behaviour of the AA robot, refer to section 3.

focus on the order management policy robots and humans adopt within the continuous replenishment market paradigm: while permits are distributed to them in the same way, humans and robots manage their orders differently. Humans employ the cash and stock received through permits to send orders to the market without further restrictions: noticeably, humans can send a new order even though orders that they previously sent are still *open*, that is, not completely traded; in particular, human traders can keep multiple (open) orders on the market at the same time. Robots, instead, process one order at a time: they only send a new order to the market upon either successful completion or intentional cancellation of the previous order they sent. Therefore robots manage (at most) one order at a time, whereas humans can (and do) keep several orders simultaneously. We will refer to the former order management policy as "single-order", and to the latter as "multiple-order". Also, for shortness, we will thereafter refer to robots employing a single-order policy as "single-order robots", and to robots employing a multiple-order policy as "multiple-order robots".

Our intuition is that the enforcement of the single-order policy deteriorates the performance of robot traders; we believe that this holds independently from the particular trading strategy robots implement. Inspired by that, we used OpEx to explore how changing the robots' order management policy from single-order to multiple-order affected their performance. We studied the effect of changing from single-order to multiple order in two experiment conditions: one where the AA spread-jumping behaviour was undesirably strong (i.e., within 15%, as in (De Luca et al., 2011)) and the other where the spread-jumping behaviour was much weaker at 1%, as originally intended (as in (Cartlidge and Cliff, 2013)).

The next section explains the basic features of CDA-based markets and defines the metrics we will use to compute the performance of the traders, in order to determine the outcome of our experiments. Section 3 describes the changes we made to the experimental conditions used in previous studies, to implement the multiple-order policy for the robots. We report the results we obtained in our simulations in section 4 and we discuss them in section 5; finally, we present our conclusions in section 6.

2 BACKGROUND

Vernon Smith's Nobel-prize-winning experimental work (Smith, 1962) demonstrated that a CDA-based market made up of human traders can reach close-to-optimal efficiency. Furthermore, Smith proved that the competitive market dynamics will cause the convergence of the transaction prices towards the *theoretical market equilibrium price* p^*. In his seminal study, Smith distributed one unit to sell (buy) at no less (more) than a specific price to each trader; such price is known as cost price c of a seller, or limit price l of a buyer. If p is the price of a specific transaction, the profit made by the buyer is thus $l-p$, while the profit made by the seller is $p-c$.

Formally, let I be the set of buyers and J the set of sellers in the market. Let $L_i = \{l_{i,1}, l_{i,2}, \ldots, l_{i,N_i}\}$ be the set of limit prices of the N_i units owned by buyer i, and $C_i = \{c_{j,1}, c_{j,2}, \ldots, c_{j,M_j}\}$ the set of cost prices of the C_j units owned by seller j. The market equilibrium price is given by:

$$p^* = \arg\max_p \left\{ \sum_{i\in I}\sum_{n=1}^{N_i} \max(0, l_{i,n}-p) + \sum_{j\in J}\sum_{m=1}^{M_j} \max(0, p-c_{j,m}) \right\} \quad (1)$$

To compare the performance of the traders, Smith measured their *allocative efficiency*, which is the total profit earned by the trader divided by the *maximum theoretical profit* of that trader, expressed as a percentage.

Defined as the profit that a trader could have made if all the market participants would have traded their (tradable) units at price p^*, the maximum theoretical profit π_i^* of buyer b_i is given by:

$$\pi_i^* = \sum_{n=1}^{N_i} \max(0, l_{i,n}-p^*) \quad (2)$$

Denoting with $p_{i,n}$ the price at which buyer i actually trades the unit with limit price $l_{i,n}$, the actual profit π_i earned by buyer i is:

$$\pi_i = \sum_{n=1}^{N_i} \max(0, l_{i,n}-p_{i,n}) \quad (3)$$

Therefore, the allocative efficiency E_i of buyer i is:

$$E_i = \frac{\pi_i}{\pi_i^*} \quad (4)$$

The allocative efficiency of a group of N traders is thus:

$$E = \frac{1}{N}\sum_{j=1}^{N} \frac{\pi_j}{\pi_j^*} \quad (5)$$

Equations 5 and 4 also hold for sellers; however, the formula of the profit of a seller must be used instead of that of the profit of a buyer in Equation 2 and Equation 3, in order to evaluate the allocative efficiency of sellers.

In this work, we will also use the following metrics to establish the performance of a market in terms of how far from the theoretical equilibrium it trades.

Used to calculate the difference between the total profit made by two groups, X and Y, divided by the mean of those two profits, delta profit is defined as:

$$\Delta P(X-Y) = \frac{2(\pi_X - \pi_Y)}{\pi_X + \pi_Y} \tag{6}$$

The delta profit of an ideal market, where the two groups match identically, should be zero.

Smith's alpha (Vernon Smith, (Smith, 1962)) captures the standard deviation of trade prices about the theoretical equilibrium price; lower values of α are desirable, indicating trading around p^*. In the following definition, we also normalise α by p^*, and express its value as a percentage by multiplying by 100:

$$\alpha = \frac{100}{p^*}\sqrt{\frac{1}{N}\sum_{i=1}^{N}(p_i - p^*)^2} \tag{7}$$

A measure of how the profit generated by each trader in a group differs from the value it would be expected of them if all transactions took place at the equilibrium price p^*, is the profit dispersion. For a group of N traders, profit dispersion is calculated as the root mean square difference between the profit achieved π_i by each trader, i, and the maximum theoretical profit available to that trader, π^*:

$$\pi_{disp} = \sqrt{\frac{1}{N}\sum_{i=1}^{N}(\pi_i - \pi^*)^2} \tag{8}$$

3 EXPERIMENT METHODOLOGY

3.1 OpEx

We ran our human vs. robot experiments on Open Exchange (OpEx). OpEx is the open source algorithmic trading system developed in 2009-2010 by Marco De Luca at University of Bristol. Since there was no *de facto* standard experimental economics platform to simulate human-agent asynchronous CDA markets when he started his doctoral research in 2009, De Luca created OpEx to offer a free, open source solution. Since its inception, and excluding this study, OpEx has hosted over 70 human-agent trading sessions, which were the empirical basis for six peer-reviewed publications (De Luca and Cliff, 2011a; De Luca and Cliff, 2011b; De Luca et al., 2011; Cartlidge et al., 2012; Cartlidge and Cliff, 2012; Cartlidge and Cliff, 2013). OpEx is available for download from Sourceforge (De Luca, 2012); on 10 November 2014, OpEx had been downloaded 935 times since it was first distributed in February 2012. A complete description of OpEx can be found in (De Luca et al., 2011).

3.2 Experiment Design

We used OpEx to perform a total of 24 human vs. robot trading experiments that took place from Monday 3rd to Thursday 6th February 2014, at Algorithmic Trading Consulting's premises in Boadilla del Monte, Madrid, Spain. All human participants were registered full-time undergraduate economics students at several universities[2] in Spain; none of the subjects had previous (professional) experience in electronic trading.

Each experiment involved 6 human traders and 6 robot traders, both equally divided into 3 buyers and 3 sellers. We split the 24 participants into 4 groups of six; we then used each of those groups in an experimental session consisting of six consecutive but distinct experiments: this way, we were able to run 24 experiments using only 24 participants.

The premises were laid out so that human participants would sit at the two long sides of a large conference room table, three by each side. Each of the seats was set up with a netbook displaying the OpEx GUI; an external mouse and numerical keypad were plugged to the netbook for convenience. Each netbook corresponded to a specific market role: the three netbooks on one side to Buyer1, Buyer2, Buyer3; and the three netbooks on the opposite side to Seller1, Seller2, Seller3 (in this order). The experiment administrator would sit at one of the short ends of the table and use a laptop to control and supervise both the market and the experiment.

At the start of each session humans were randomly allocated to a seat (market role); the subjects were briefed about the rules of the experiment; and they were given some time to familiarise with the OpEx GUI. During the tutorial robot traders were switched off, thus allowing human participants to trade among themselves and get accustomed to the market. Briefing and tutorial took about 10 minutes.

Then the actual experiment was run for six times in a row, each one lasting 10 minutes. At the end of each run, and before the following one, we introduced a 2 minutes break during which the administrator could set the new experimental configuration, and the participants rotated seats anti-clockwise[3] thus

[2] Universidad Complutense de Madrid. Universidad Carlos III de Madrid. Univeridad Rey Juan Carlos.

[3] We purposely mixed human roles between experi-

Table 1: Permit schedule employed in the 24 simulations.

	1	2	3	4	5	6
Buyer1	350 (0)	250 (5)	220 (7)	190 (9)	150 (14)	140 (16)
Buyer2	340 (1)	270 (3)	210 (8)	180 (10)	170 (12)	130 (17)
Buyer3	330 (2)	260 (4)	230 (6)	170 (11)	160 (13)	150 (15)
Seller1	50 (0)	150 (5)	180 (7)	210 (9)	250 (14)	260 (16)
Seller2	60 (1)	130 (3)	190 (8)	220 (10)	230 (12)	270 (17)
Seller3	70 (2)	140 (4)	170 (6)	230 (11)	240 (13)	250 (15)

playing a different market role each time. Overall, an experimental session lasted less than 90 minutes, with 60 minutes of experiment time.

Finally, we motivated all participants by paying each of them €25, plus, to incentivise traders to make profit, the human trader scoring the highest allocative efficiency (as defined in Section 2, Equation 4) on each experimental session was rewarded with an additional €250 prize. Since one prize was given at the end of each experimental session, and the human participants to each session were 6, participants had a 1-in-6 chance of winning the prize. The total cost of the experiments was €25*24 + €250*4 = €1600.

3.3 Supply and Demand

We employed the schedule of permits used in (De Luca et al., 2011; Cartlidge et al., 2012). The limit prices are shown in Table 1, arranged by trader and permit type; the time step at which each permit is sent is shown in brackets. Figure 1 displays the supply and demand curves that originate from such schedule.

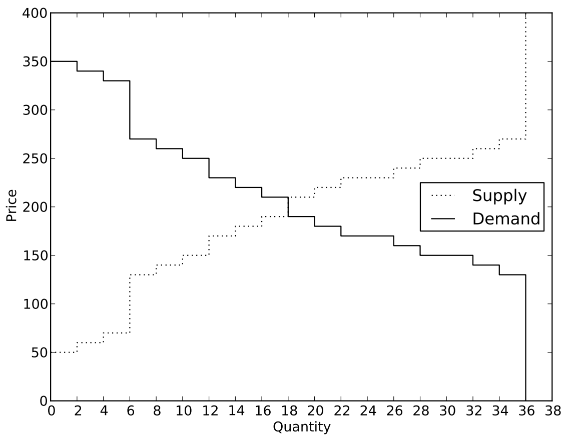

Figure 1: Supply and demand.

ments, to reduce the opportunity for collusion and counteract any bias in market role.

3.4 Robot Configuration

In (Cartlidge and Cliff, 2012), Cartlidge and Cliff noted that the implementation of AA found in version 1.0 of OpEx displayed an undesired behaviour, not documented in Vytelingum's original description of the trading strategy (Vytelingum, 2006; Vytelingum et al., 2008), for which the robot would cross the spread every time its relative value fell below a predetermined percentage threshold. While they reported that the idea of systematically crossing the spread can be reasonable and beneficial to convergence, Cartlidge and Cliff pointed out that the fixed value of 0.15 (i.e., 15%) used for the threshold was much too high[4]. Indeed, they demonstrated that when the threshold was set to the 1% value originally specified by Vytelingum (Vytelingum, 2006), robots outperformed human traders, but when the threshold was set to 15% the humans outperformed the robots. Inspired by their results, in this work we will experiment with both settings of the threshold: 1%, and 15%.

3.5 Order Management Policies

We have seen in Section 1 how permits are the instructions sent by (simulated) clients to agents, while orders are the instructions that agents send to the market. The relationship between permits and orders is not necessarily one to one. In fact, importantly, one permit for one unit can give origin to many orders. This is possible because orders can be cancelled: once an order for one unit has been cancelled, that unit is no longer advertised on the market, therefore a new order can (and should) be sent to the market, to newly implement the client's instruction. Once a unit has been received by a trader as a permit, we will refer to the process of sending, amending, and cancelling or-

[4]Contextually, De Luca was informed of the bug, and released a new version of OpEx in which the value of the threshold that triggered spread-crossing was configurable. The new version, OpEx 1.1, was released on 25 March 2012.

ders that the trader executes in order to trade that unit, as *working* that permit.

The order management policy to apply for humans and robots was a design choice that was faced when permits were distributed on a periodic regular schedule (continuous market, as studied in (Cliff and Preist, 2001; De Luca et al., 2011; Cartlidge et al., 2012; Cartlidge and Cliff, 2012; Cartlidge and Cliff, 2013)), rather than sequentially and only upon completion of the preceding one (cyclical market, as studied in (Das et al., 2001; De Luca and Cliff, 2011a; De Luca and Cliff, 2011b)). By order management policy, we mean the set of rules that apply to a group of traders for what concerns the creation and handling of orders.

We say that a group of traders follows a multiple-order policy, if an order for one unit with a certain limit price l can be sent to the market, provided that a permit for one unit with limit price l has been received, and no further orders are on the market for that particular unit. In particular, this allows a trader to submit a new order for a new unit even though an order of hers for a previously received unit was still on the market.

Instead, the following applies to traders following a single-order policy:

1. when a new permit is received:
   (a) if the trader is not working any permit, then the trader will immediately start working the new permit;
   (b) if the trader is working a more profitable permit than the newly arrived, then the trader will ignore the newly arrived permit and push it to the "deferred permits queue";
   (c) if the trader is working a less profitable permit than the newly arrived, then the trader will cancel the current order, push the associated permit to the "deferred permits queue", and start working the newly arrived permit;
2. when a trader completes the order related to the current permit:
   (a) if the "deferred permits queue" is not empty, the trader retrieves the most profitable permit from it, and starts working it;
   (b) if the "deferred permits queue" is empty, the trader does nothing.

In order to change the order management policy implemented by OpEx robots from single-order to multiple-order, we leveraged the existing infrastructure and implemented the following multiple-order policy (similar rules apply to sellers):

1. when a new permit to buy at limit price l is received by a trader:
   (a) if the trader is not working any permits at limit price l, then the trader will send a new order with limit price l;
   (b) if the trader is already working permits at limit price l, then the trader will amend the existing order at limit price l to include the quantity just requested in the last permit;
2. when a trader completes the order related to the current permit, the trader becomes idle and waits to receive the next permit.

4 EXPERIMENTAL RESULTS

In this paper, we tested both the single-order and the multiple-order policy across 24 simulations: 12 simulations for each policy. And for each of the two policies, we explored the effects of two values of the minimum relative spread threshold crossed by the AA trading strategy: 15%, and 1%. We will refer to this threshold as MaxSpread, after the name of the OpEx variable that represents it.

We tested the four resulting combinations in four sets of six 10 minutes' simulations.

We will first focus on the group efficiency of robots and humans, as defined by Equation 5: Table 2 summarises the results we obtained under the four different experimental conditions employed. Each cell in the table represents an experiment and contains: the symbolic name we gave to the experiment; an indication of the allocative efficiency of the robots, compared to that of the humans; and the level of significance of the result, according to Fligner and Pollicello's robust rank order (RRO) test[5].

Table 2: Group allocative efficiency of robots and humans compared.

	MaxSpread=15%	MaxSpread=1%
	MP15	MP01
M	$R = H$	$R > H$
	$p > 10.4\%$	$1.1\% < p < 2.2\%$
	SP15	SP01
S	$H > R$	$R > H$
	$0.11\% < p < 0.54\%$	$5.1\% < p < 10.4\%$

[5]Fligner and Pollicello first reported on the robust rank order (RRO) test in 1981 (Fligner and Pollicello, 1981). The RRO test is a non-parametric test of difference in medians, which Feltovich showed in (Feltovich, 2003) to apply well to the domain of small sample statistics (in which we definitely are), and to perform better than the more commonly-used Wilcoxon-Mann-Whitney 'U' test.

The bottom section of the table shows that, if robots enforce a single-order policy: when MaxSpread=1%, robots outperform humans (experiment SP01, $R > H$); and when MaxSpread=15%, humans outperform robots (experiment SP15, $H > R$). The results of the RRO test to the raw group allocative efficiency data of robots and humans in the two experiments show that: in experiment SP01 the difference between the two group efficiencies is significant at the 10.4% level (that is, $5.1\% < p < 10.4\%$); and in experiment SP15 the difference is significant at the 0.54% level ($0.11\% < p < 0.54\%$).

Our novel findings resulting from using a multiple-order policy for the robots are shown in the top row of Table 2. In experiment MP01, with MaxSpread=1%, robots outperform humans with a level of significance of 2.2% ($1.1\% < p < 2.2\%$); and in experiment MP15, with MaxSpread set to 15%, the difference between the efficiency of robots and humans is not significant at 10.4% or less (i.e., $p > 0.104$).

Moreover, we compared the performance of robots in all experiments employing a multiple-order policy (MP01 and MP15) to that of their single-order policy counterpart (SP01 and SP15). The RRO test showed that the first group performed better than the second group with a level of significance of 1% ($0.5\% < p < 1\%$).

To extract further comparative information related to the performance of robots conditional to the four experimental conditions, we isolated the raw data of the group allocative efficiency of robots in our four experiments, and thus applied the RRO test to the two series of 6 points (one per each 10-minutes simulation in the 60 minutes experiment) identified by each pair of adjacent cells in Table 2. Table 3 contains the results.

The two groups of robots under comparison are labelled R1 and R2. For each of the four comparisons, the table shows: the experimental conditions under which each of the two groups of robots traded and the significance level according to the RRO test. The experimental conditions include the name of the experiment, the order-management policy, and the value of MaxSpread. The robots in the group labelled R1 are those with the resulting higher group efficiency, as defined in Equation 5.

Looking at market efficiency, that is the group allocative efficiency of all traders, the ranking of the most efficient markets is MP01, MP15, SP01 and SP15 in this order, displaying efficiencies of 0.818, 0.785, 0.780 and 0.773 respectively. Further, for each experiment we calculated the delta profit between the two groups, robots and humans (as per Equation 6); the profit dispersion, shown in Equation 8; and finally Smith's alpha (Equation 7). Table 4 summarises our findings with respect to such statistics.

Table 3: Group allocative efficiency of robots compared across different experimental conditions.

R1	R2	RRO
MP01 Multiple 1%	SP01 Single 1%	$0.54\% < p < 1.1\%$
MP01 Multiple 1%	MP15 Multiple 15%	$0.11\% < p < 0.54\%$
SP01 Single 1%	SP15 Single 15%	$0.11\% < p < 0.54\%$
MP15 Multiple 15%	SP15 Single 15%	$0.11\% < p < 0.54\%$

Table 4: Aggregated market statistics. For each experiment, the table shows: market efficiency; relative profit difference between robots and humans; profit dispersion; and Smith's α.

Experiment	E	ΔP	π_{disp}	α
MP01	0.818	3.6%	901	9.3%
MP15	0.785	-1.8%	1079	12.7%
SP01	0.780	3.4%	1155	17.3%
SP15	0.773	-18.4%	1206	6.4%

While the metrics employed so far explain measurements that apply to the experiment as a whole, we introduce here a novel analysis that reveals components of the human-agent interaction that are related to time. In the course of the simulations, permits are regularly distributed to the traders, following the schedule in Table 1. Once a permit has been distributed to a trader (either human or robot), the trader decides how to employ that permit, compatibly with the order-management policy that is being enforced. Thus, considering both the present market conditions and the past evolution, the trader decides whether to send an order immediately, or delay execution of the order until he estimates there are better market conditions to send the order. The order, in turn, may immediately fill if it's an aggressive order, or it may sit in the order book, perhaps its price may be modified a few times, and then it may eventually fill.

By scanning the time series of order instructions stored OpEx's database with sub-second time resolution, we analysed the Time-To-Complete of the permits, defined as the time elapsed from the moment a permit is received by a trader, to the moment the order that trader sent to work that permit fills. After measuring this time for all trades, we grouped the results by trader group, human or robot, and subsequently by profitability of the permit, intra-marginal or extra-marginal. We thus obtained, for each of our experiments, four populations of Time-To-Complete. Table 5 shows the median of each of the four populations in the different experiments; we labelled each population so that, for example, R-IM stands for "robots, intra-marginal permits", and H-EM stands for "humans, extra-marginal permits".

Table 5: Time-To-Complete for each experiment. The table shows the median (in seconds) of the Time-To-Complete of intra-marginal and extra-marginal permits, for both human and robot traders.

Experiment	R-IM	H-IM	R-EM	H-EM
MP01	0.8	20	119	325
MP15	0.1	10	106	125
SP01	1.7	12	112	140
SP15	0.3	20	110	135

5 DISCUSSION

The results in the bottom line of Table 2 agree with the findings presented in (De Luca et al., 2011; Cartlidge and Cliff, 2012; Cartlidge and Cliff, 2013), that is: if the order-management policy enforced by robots is single-order, humans perform better than robots when MaxSpread is 15%, while robots outperform humans when MaxSpread is 1%. The innovative multiple-order policy we employed for robots revealed the results displayed in the top row of the table. When MaxSpread is 1%, switching from single-order to multiple-order still reveals a victory of robots: we found that the RRO test gives a stricter result for MP01 than it does for SP01, and we interpret this finding as the fact that the switch to a multiple-order policy benefited the robots, as a group. We found though that there is no statistically significant difference between the efficiency of robots and humans in experiment MP15; by visually inspecting Table 2, we notice that, when MaxSpread is 15%, switching from single-order to multiple-order turns the distinct victory of humans in experiment SP15, into what the RRO test signalled as an indecisive case. However, the comparison of all the results obtained for multiple-order robots with those obtained for single-order robots indicate a sharp result (RRO $0.5\% < p < 1\%$): multiple-order robots perform better than single-order robots, regardless of whether MaxSpread is 15% or 1%.

We then studied the relationship that holds between the efficiency of robots across our experiments (see Table 3), and found that indeed the effects of switching the robots policy to multiple-order are explained. All the RRO test results have a level of significance of p=0.54%, except for MP01 vs. SP01 which has a level of significance of p=1.1%: overall, very sharp results. In detail:

- the performance of robots was significantly better in MP01 than it was in SP01: this confirms our previous interpretation of the different level of significance in the two cases;
- robots employing a multiple-order policy perform better when MaxSpread=1% (MP01) than when MaxSpread=15% (MP15);
- even though MaxSpread is 15% (that is, AA is crossing the spread excessively), switching to a multiple-order policy benefits the performance of robots, declaring their efficiency significantly better at the 0.54% level;
- finally, we confirmed that robots that employ a single-order policy perform better when MaxSpread is 1% (SP01 vs. SP15).

Furthermore, we interpreted the metrics summarised in Table 4 as follows. Sorting the experiments by descending value of market efficiency, is indeed equivalent to sorting them by ascending value of profit dispersion[6], and result into the sequence MP01, MP15, SP01, SP15. In both cases, the indication is that markets where the multiple-order policy is in place are better.

The ranking deriving from Smith's α still places MP01 and MP15 before SP01, but it puts SP15 in the first place; to explain this, we speculate that the combination of less simultaneous orders by robots, and AA aggressively accepting more offers, made the trade price time series more stable around p^* in SP15.

The values of ΔP are mixed; however, qualitatively, we can see how the magnitude of ΔP is much higher in SP15, than it is in every other experiment. In particular, we point out how the absolute value of ΔP in MP15 is about one tenth of the value in SP15: we interpret this as a further confirmation that switching to a multiple-order policy boosts the quality of the market.

[6]Higher values of market efficiency, and lower values of profit dispersion are better.

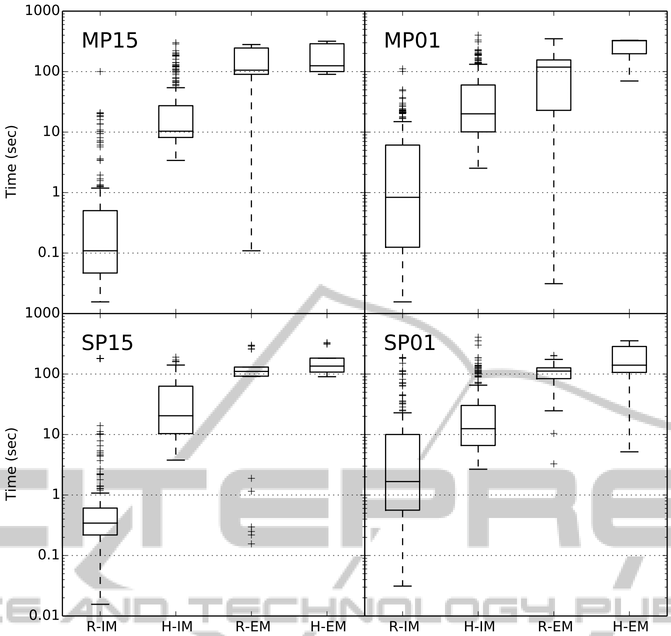

Figure 2: Box plot of Time-To-Complete. The charts of the four sets of experiments MP15, MP01, SP15, SP01 are displayed in the sub-figures, respectively from left to right, and from top to bottom. The time axis is logarithmic.

Figure 2 offers a graphical representation of our time analysis, in the form of a box plot. The bottom, middle, and top of the box are respectively the first, second (median) and third quartiles of each set of data. The whiskers of the plot extend to the lowest datum still within 1.5 IQR of the lower quartile, and the highest datum still within 1.5 IQR of the upper quartile; by IQR we mean the interquartile range, that is the difference between the third and the first quartile.

We will mainly focus on the statistics of Time-To-Complete for the group R-IM: that is, the values we collected for the intra-marginal permits traded by robots in each experiment. With MaxSpread=1%, switching the policy to multiple-order consistently reduces the values of first, second and third quartiles (SP01 vs. MP01); the same effect applies to SP15 vs. MP15. In practise, this means that the robots of MP01 and MP15 send to the market orders that take less time to complete than their counterparts of SP01 and SP15, respectively. Together with the results we found for the group allocative efficiency, this evidences the superior pricing employed by the robots that manage multiple orders simultaneously. Once again, this illustrates how adopting a multiple-order policy is beneficial in both cases: MaxSpread=1%, and MaxSpread=15%.

6 CONCLUSIONS

We resumed the investigation on human vs. robot experimental trading simulations from the last major finding published by Cartlidge and Cliff (Cartlidge and Cliff, 2012; Cartlidge and Cliff, 2013): lower values of the minimum relative spread to cross (MaxSpread) result in better group allocative efficiency for AA robots.

We introduced a new dimension in the analysis, time to complete, that is orthogonal to the one defined by MaxSpread; and we generated data in the newly defined 2-dimensional space of experimental conditions. We evaluated the resulting markets using both aggregated metrics (i.e. allocative efficiency, group efficiency, Smith's α, delta profit, profit dispersion), and time analysis.

First, we proved that we could successfully reproduce Cartlidge and Cliff's results.

Second, we demonstrated that robots that manage simultaneous multiple orders display a superior performance to that of robots that manage one order at a time, separately in the two cases: MaxSpread=1%, and MaxSpread=15%.

Third, we extended our comparison across the MaxSpread axis and proved that the superiority of multiple-order policy on single-order policy holds regardless of the value of MaxSpread. This is a strong result: prior to this study, to the best of our knowledge, the only explanation given to the supremacy of humans over robots reported in (De Luca et al., 2011) was that found by Cartlidge and Cliff (Cartlidge and Cliff, 2012), claiming that this phenomenon was due to a malfunctioning of the AA robots. Indeed, here we proved that multiple-order robots outperform single-order robots. Therefore, the poor performance of robots in (De Luca et al., 2011) may as well be explained by the disadvantage that robots employing a single-order policy have, when they challenge humans allowed to deal multiple simultaneous orders.

Finally, presuming that no commercial algorithm would manage only one order at a time, we argue that we moved a step towards a more realistic model of sales-trading behaviour in real-world financial markets.

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