Cross−impact and Price Bubbles: A Two-Asset Lab-Experiment

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Abstract

We explore cross−impact in a hybrid experimental market with human and artificial agents, varying liquidity across treatments. In treatment *Separated* participants hold distinct portfolios for two stocks, while in *Unique* they hold a unique portfolio, i.e., can freely move capital between assets. Larger bubbles and asymmetric cross-market impact occur with unique portfolios and decreasing value asset. When comparing experimental and synthetic data, cross−impact is attributed to human players, especially when stock values are close to each other. Artificial players also react to human presence, contributing to cross−impact.

Keywords: Market-impact, cross−impact, Hybrid-Experimental Market, Bubbles, Artificial traders.
JEL Codes: C91, D47, G14, G17, G40.

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1 Introduction

The phenomenon of financial bubbles has long attracted researchers due to its connection to human irrationality and behavioral biases. Understanding the underlying mechanisms behind the formation and persistence of these bubbles is crucial for economists and policy makers (Blanchard 1979; Tirole 1985; Froot et al. 1991; Abreu et al. 2003; Martin et al. 2012; Hirano et al. 2016; Miao et al. 2018).

Of particular interest is the role of market liquidity in promoting bubble formation, both in experimental (Kirchler et al. 2012, Palan 2013) and real markets (CFTC-SEC 2010; Kirilenko et al. 2017; Menkveld et al. 2019). Events like the Flash Crash of 2010 show how liquidity can destabilize markets, with portfolio orders triggering rapid instabilities across assets and markets.

In this paper, we conduct an experiment to explore the impact of market liquidity on price dynamics and estimate, in a controlled environment, the so-called "price-impact matrix". In its simplest form, this matrix allows us to derive two sets of estimates: the diagonal *self-impact* and the off-diagonal *cross-impact* parameters. The self-impact parameters capture the (partly mechanical) direct effect of trade imbalance on the price of an asset, while the cross-impact parameters capture the more subtle effect of the trade imbalance of one asset on the price of another asset. Importantly, the cross-impact parameters reveal how trading pressure from one asset can affect the price behavior of another asset and contribute to understanding the interconnectedness within financial markets (e.g., Le Coz et al. 2024). From an empirical perspective, to capture a stock's trade imbalance, we rely on the well-known measure of Order Flow Imbalance (OFI), developed by Cont et al. (2014), defined as the net difference between buy and sell orders (see also Mertens et al. 2019,Rama et al. 2022).

Since Kyle's seminal work (1985), a large body of empirical literature has focused on estimating this matrix with real financial data. However, this exercise poses significant challenges, requiring substantial observations, high trading volumes, and sophisticated techniques to mitigate microstructure noise (e.g., Mastromatteo et al. 2017; Schneider et al. 2019; Mertens et al. 2019; Tomas et al. 2022; Rama et al. 2022). For example, using real data, one must hypothesize the categories of stocks where spillover effects are anticipated (e.g., Min et al. 2018) and control for common components in order flow imbalance (Capponi et al. 2020).

Generally speaking, the existence of cross-impact can be attributed to different causes. First, the correlation between returns of assets driven by the same factors could lead to cross-impact since traders which use statistical arbitrage strategies, such as pair trading based on cointegration of prices, trade both assets simultaneously impacting both prices. The second explanation is due to commonality in liquidity provision, empirically documented since (Chordia et al. 2000). Since price impact is correlated with bid ask spreads, common movements of the latter lead to correlation of impacts and ultimately to cross asset effects. The third explanation is related to the practice of portfolio trading: traders investing in multiple assets trade simultaneously the different positions, and therefore, due to the mechanical effect of market impact, the order flow of one of the assets is correlated with the price changes on another asset exactly because of the portfolio trading.

Empirically it is very complicated to disentangle the three effects, due to lack of suitable data on trading decisions of single investors and to econometric identification issues. In this paper, we design an experimental setup to isolate the third effect. In fact in our setting, the fundamental values of the two assets are not correlated, ruling out the first mechanism. Second, we introduce a type of artificial players, market-makers (MM), that by providing liquidity optimize their posting independently for each asset, ruling out the common liquidity provision. Finally, to investigate the role of the third mechanism, we manipulate liquidity in two treatments, one in which human participants hold distinct portfolios for two assets (*T1-Separated*) and one in which they hold a unique portfolio (*T2-Unique*), that is, they can freely move capital between assets. The differential effect between the two treatments give us a measure to isolate and quantify the role of the third mechanism described above.

To achieve this, we expand the design of Smith et al. (1988) (SSW) by introducing a two-asset market setup—similar to Caginalp et al. (2002) — in a continuous double -auction open limit order book structure (e.g., Fink et al. 2020; Kirchler et al. 2012; Smith et al. 1988), where one asset exhibits speculative characteristics with a declining fundamental value while the other maintains a constant fundamental value. We then implement our market segmentation, by restricting capital flow between assets in $T1 - Separated$ and allowing free capital movement in $T2 - Unique$.

To tackle the challenges of limited human liquidity in experimental markets, additionally enhancing external validity, our set-up features in both treatments two types of artificial players: near zero-intelligent traders (ZI) and market makers (MM) (e.g., Duffy et al. 2006; Haruvy et al. 2006; Baghestanian et al. 2015). ZI traders emulate inexperienced, irrational, or liquidity-seeker human traders, as shown in Duffy et al. (2006), and ensure adequate liquidity levels consistent with real financial markets (see also Bao et al. 2022). MMs follow the Avellaneda-Stokov model (Avellaneda et al. 2008), acting as CARA utility maximizers who solve a complex stochastic programming problem. By continuously and regularly proposing prices at which they are willing to buy and sell a given asset, MMs aim to ensure sufficient liquidity throughout all trading periods of our experiment and contribute significantly to incorporate private information of traders into price (Glosten et al. 1985) . Indeed, their strategy is considered the benchmark for real algorithmic traders in market making, with the primary objective of providing and maintaining liquidity.[1] As detailed below, ZI traders place limit orders, but they do with a directional view, each time either sending a bid or a ask order. This is different from MMs which place simultaneously limit orders on both sides of the book, trying to earn the spread. Thus ZIs cannot be considered as pure liquidity providers as MMs. Finally, both ZIs and MMs place orders independently in the two assets,

[1]Market-makers are said to be part of 70% of the electronic trades in the United States (40% in the European Union (EU) and 35% in Japan). Some of them are "official," i.e., there is an agreement with an exchange for maintaining fair and orderly markets (e.g., the Designated Market Makers) while others are just acting as liquidity providers without any obligation to do it (e.g., high-frequency traders) see Guéant (2016).

thus in the *T2-Unique* treatment only human traders can act strategically in the two assets.

While a series of papers has considered ZI agents (see Bao et al. 2022 for a review), only a limited number of experimental studies have implemented MMs. The primary focus, though, has been on assessing how human participants rely on various algorithms (see Asparouhova et al. 2020, Aldrich et al. 2020). Among the papers related to our framework, one notable example is Angerer et al. (2023), which studies how arbitrage-seeking algorithms in financial markets, whether making or taking liquidity, influence market efficiency, liquidity, and wealth distribution. In contrast, Leal et al. (2020), while not implementing these types of traders, investigates their role in driving expectations by manipulating traders' beliefs.

Thus, our experimental setup, while sheddings light on the origin of the intriguing phenomenon of cross impact in financial markets, contributes to the expanding body of literature on the interaction between artificial players and human traders in hybrid markets (Bao et al. 2022). Specifically, by including both human traders and artificial players, our design uniquely enables us to analyze the distinct effects each group has on market pricing and to quantify the impact matrix for humans and ZI players separately.[2] To this end, alongside the data collected in the lab, we generate synthetic data that simulates market conditions in continuous time without human traders. We then compare this synthetic data to the lab data.

Overall, the literature on the interaction between human and algorithmic traders in hybrid markets presents a nuanced understanding of trading dynamics. On the one hand, some papers highlights that hybrid markets exhibit slower convergence to equilibrium prices compared to all-human or all-algorithm markets, with human traders underperforming algorithms (e.g., Gerig 2007; Miao et al. 2018). On the other hand, some research (e.g. Feldman et al. 2010) reveal that while algorithmic traders generally earn higher profits, human traders may outperform algorithms during market crashes and demonstrate adaptability in volatile environments. Across studies, though, we also note a variance in the 'intelligence' of traders, specifically in terms of their propensity to trade in alignment with fundamental values. Furthermore, Farjam et al. (2018) also suggest that the mere expectations of algorithmic traders may induce participants to behave more rationally, in contrast with Leal et al. (2020).

More generally, our experimental framework contributes to the expanding body of research on experimental markets featuring multiple assets (Duffy et al. 2022). Previous studies have shown that the inclusion of related financial instruments in the same market can reduce mispricing and boost market activity, especially when dividends between these instruments are negatively correlated. Nevertheless, when asset correlations are not perfectly positive, participants often struggle to align asset prices with their fundamental values.

Crucially, our experimental approach did not involve the pre-registration of our study on any established platform. Instead, we undertook a comprehensive approach to ensure rigor and reliability in our experimental design and hypothesis formulation. This involved - through Agent Base Models (ABMs) developed in discrete time - the generation of simulated

[2]As explained in section 2,we configured MMs to place only limit orders, ensuring a sufficient bid-ask spread for noise traders and humans.

data in which we assume a set of different trading strategies for players (e.g. directionalist or market neutral), which are detailed in an accompanying paper (Cordoni et al. 2021).

Consistently with previous experimental research, we anticipate larger bubbles for the asset with a declining fundamental value (Palan 2013, Kirchler et al. 2012). More critically, we hypothesize a notable increase in cross-market impact in scenarios where capital movement across markets is unrestricted (H1), expecting this influence to be uneven (H2) and predominantly driven by increased trading activity in the decreasing-value asset affecting the constant−value asset market. Additionally, we predict that the self−impact will stay unchanged across different experimental conditions (H3), and posit that cross-market impact will intensify during periods of entanglements, specifically when the fundamental values of the two assets converge closely (H4).

The results from our experimental data align closely with the hypotheses. Specifically, we observe the formation of financial bubbles for assets with decreasing fundamental values. As anticipated, a more significant cross−impact is observed in the treatment where participants can freely move capital between assets (*T2−Unique*). In this treatment, the cross−impact primarily arises from the asset with a decreasing fundamental value, rather than from the asset with a constant fundamental value. This phenomenon occurs when distinguishing between types of player activity and can be primarily attributed to human activity, especially during periods of entanglement of fundamental values. Unexpectedly, a cross−impact effect consistently arises from the constant value asset to the decreasing value asset. Importantly, by comparing synthetic data (i.e., involving only artificial players) with experimental data (which includes human players), we attribute this effect to ZI players' responses to the presence of human players.

2 The experimental market

Our experimental framework is built upon a market design where each asset pays a random dividend at the end of each period, aligning with the methodologies used in previous studies such Smith et al. (1988) and Kirchler et al. (2012) . The fundamental value of an asset is calculated as the sum of discounted future dividend payments plus a terminal value. For asset i, $\overline{d}_i$ represents the expected dividend payment while TV_i its terminal (or buy-out) value, i.e., the terminal payoff paid by i at the end of the last trading period T. Thus, at time t the fundamental value for asset i is given by$FV_{t,i} = (T - t + 1) \cdot \overline{d}_i + TV_i$.

In each period, the dividends of the first asset are independently drawn from a set $d_1 = \{0, 0.1, 0.16, 0.22\}$, with a terminal value equal to 1.80, leading to a declining fundamental value over time. Conversely, the second asset's dividends are independently drawn from the support $d_2 = \{-0.2, -0.1, 0, 0.1, 0.2\}$, with a terminal value of 2.80, implying a constant fundamental value due to its expected dividend value of zero. Negative dividends represent holding costs (see Kirchler et al. 2012). Kirchler et al. (2012) have shown that an asset with a constant fundamental value decreases the probability of observing a bubble, while the opposite occurs with a decreasing fundamental value.

Our market also incorporates both human and artificial players, creating a hybrid environment that mirrors the complexities of real-world financial markets. Artificial traders are introduced to replicate the high trading volumes of actual markets and enable a more accurate estimation of price and cross−impact effects. Among these artificial players we distinguish between ”near zero intelligent” traders (ZI), simulating the behavior from inexperienced humans in prior experiments (see Duffy et al. (2006) Baghestanian et al. (2015)), and market-makers (MMs), who provide liquidity by posting quotes according to Avellaneda et al. (2008).

The trading system operates as a double auction market with continuous open-order book dynamics, ensuring anonymity in order submissions. Each trading period lasts 180 seconds, across 15 periods, with participants initially endowed with two units of each asset and a cash balance in Experimental Currency Units (ECU). Orders are restricted to single shares, and participants cannot engage in short selling (e.g., Fink et al. (2020)).

Participants have two primary ways to trade, that is by submitting *limit orders* and *market orders*. With the latter, traders immediately buy (market buy) at the lowest available ask or sell (market sell) at the highest available bid in the order book, enabling quick transactions at current market prices. With the former, traders specify the maximum price they're willing to pay (limit bid) or the minimum price they're willing to accept (limit ask) for an asset. These orders are visible in the order book, helping to form the market's bid-ask spread, and are automatically cleared whenever the specified price conditions are met. Human participants cannot go short.

ZI agents have the same initial inventory as human participants and cannot go short either. They are programmed by imposing the *weak foresight* assumptions of Duffy et al. (2006), i.e., their probability to be a buyer π_t is decreasing among the T trading periods,

$$\pi_t = \max\{0.5 - \varphi t, 0\}, \text{ where } \varphi \in \left[0, \frac{0.5}{T}\right).$$

A positive φ implies a gradual increase of excess supply towards the end of the market and it contributes to the reduction in mean transaction prices. Thus, each ZI trader j places a quote following a convex combination of the previous period mean traded price $\overline{p}_{t-1}$ and a random quantity u_t. This random price is proportional to the current expected fundamental value (FV_t) drawn from a uniform distribution with support $[0, \kappa \cdot FV_t]$, where $\kappa > 0$. This $\kappa > 0$ captures the possibility that agents can make some decision errors. In other words, if j is a buyer and has enough endowment of cash x^j, j can place a bid limit order with price given by

$$b_t^j = \min\{(1-\alpha)u_t + \alpha\overline{p}_{t-1}, x_t^j\}$$

and if j is a seller and has at least one share to sell he can place a ask limit order with price given by

$$a_t^j = (1-\alpha)u_t + \alpha\overline{p}_{t-1},$$

where $\alpha \in (0, 1)$ is the so called *anchoring* parameter. The anchoring parameter plays a crucial role in the price-bubble formation, since prices will necessarily increase at the beginning to decrease as the fundamental value decreases.

In all treatments, we set-up the following parameters: for asset 1 $\varphi = 0.25/T, \alpha = 0.8499, \kappa = 4.1946$, while for asset 2 $\varphi = 0, \alpha = 1 - 0.8499, \kappa = 2$. The choice of these parameters is grounded in both previous experimental evidence (see Duffy et al. 2006, Baghestanian et al. 2015) and on our simulation analysis (see Cordoni et al. 2021). This ensures that we have ZI traders who generate the price path we expected: a price bubble on the asset with decreasing fundamental value and no bubble for the asset with constant fundamental value. In Cordoni et al. (2021), a proportion of 1 human to 3 ZI traders emerged as necessary to guarantee an order book sufficiently populated to obtain precise estimation of price impact. Meanwhile, we maintained a constant number of market makers at 10.[3]

Market-makers in our study adopt the Avellaneda and Stoikov model, referencing previous period mid prices instead of fundamental values to place optimal quotes for maximizing expected CARA utility over a finite time horizon in an order book. This approach, diverging from classical models like Ho et al. (1981), aims to prevent order book imbalances and enhance the learning pace, making the experiment more reflective of real financial markets. Unlike the SSW experiment where initial prices are set below fundamental values (Duffy et al. 2006; Palan 2013), our setup initializes the book with market-maker bids to mitigate this issue.

Following Guéant et al. (2013), which formalizes the Avellaneda-Stoikov model, our market-makers can hold long or short maximum authorized inventories, distinct from human and ZI traders. They post bid/ask quotes in an opening session before the round starts, ensuring liquidity each round without directly trading, as their orders can only be accepted by human or ZI traders. Their optimal quotes, influenced by inventory and risk aversion (Avellaneda et al. 2008), vary with risk parameters selected from an equidistant grid between 0.5 and 1

$$
\begin{aligned}
S^{b*}(t, p, q) &= p - \frac{1}{\kappa} \ln\left(\frac{v_q(t)}{v_{q+1}(t)}\right) - \frac{1}{\gamma} \ln\left(1 + \frac{\gamma}{\kappa}\right) \\
S^{a*}(t, p, q) &= p + \frac{1}{\kappa} \ln\left(\frac{v_q(t)}{v_{q-1}(t)}\right) + \frac{1}{\gamma} \ln\left(1 + \frac{\gamma}{\kappa}\right),
\end{aligned}
$$

where p is the current value of the reference price (the mid-price), γ is the market-maker's risk-aversion, κ characterizes the price sensitivity of market participants, and q are the optimal quotes are derived by solving a linear system specific to the Avellaneda et al. (2008) model. The MMs' inventories, as well as their optimal quote, are updated when a trader hits one of their quotes, generating a new spread.

In conclusion, market-makers play a crucial role in stabilizing price impact and facil-

[3] In Cordoni et al. 2021, we also demonstrate that the trading volumes in our experiments align with those observed in earlier studies. Furthermore, Cordoni (2022) illustrates how incorporating artificial ZI traders into our model replicates a price bubble dynamic similar to those documented in seminal research and experiments.

itating agent learning without dominating the market. Their limited active participation, focusing on limit orders, ensures a balanced trading environment predominantly driven by human and ZI traders. This setup not only prevents liquidity crises but also neutrally influences price formation, underscoring their importance in experimental financial markets.[4]

2.1 Market Treatments

To isolate the effect of liquidity while taking into consideration the cross−impact in bubble contagion, we run two main treatments, which only differ in the constraints imposed on traders' portfolios. The first treatment, named *T1-Separated*, features traders holding two separate portfolios where they cannot transfer funds from one asset to another. For example, the money earned from the sale of one asset share cannot be used to purchase the other asset. The second treatment, named *T2-Unique*, features a single portfolio where traders are free to invest their money in either of the two assets without any restrictions. For example, the money earned from the sale of one asset share can be used to purchase the other asset. Essentially, in *T2-Unique*, traders have the freedom to move their capital from one asset to the other. By comparing these two types of markets, we can identify how changes in liquidity affect *cross-* and *self-impact*. Additionally, in both treatments, asset dividends are placed in their respective portfolios. According to Kirchler et al. (2012), merging the savings account for dividend cash with portfolio cash implies an increasing Cash/Asset ratio (C/A), which in turn generates an increase in the available liquidity for traders, fostering price bubbles.

Furthermore, to validate our experimental results, we generate synthetic data by replicating the experimental markets within a simulated environment that includes only artificial traders. This analysis significantly differs from the ABM analysis in Cordoni et al. (2021) because it is conducted in continuous time rather than discrete time and does not impose any assumptions about traders' strategies (i.e., being market-neutral or directional).

For both treatments, we generate the market counterfactual with no human participants, i.e., *T1-Separated-Synthetic* and *T2-Unique-Synthetic*. By selecting the same random seed numbers, we can exactly replicate the behavior of artificial traders observed in the laboratory markets, such as the number of posted bids by the artificial traders, their prices, and market side (buy or sell orders), in the absence of human traders, and then match the results with the corresponding trading behavior in the market with humans. Table (1) provides a treatment overview.[5] An English translation of the experimental instructions is available at the end of the paper.

[4]Indeed, in our laboratory sessions most of transactions and trading activity can be attributed to the trading of human and ZI traders. Market makers (MMs) are configured to place only limit orders with parameters ensuring a sufficient bid-ask spread for noise traders and humans. They ensure market liquidity, preventing imbalances during panic sells or booms. MMs also help populate the initial order book at the start of each market round. (see Table 2).

[5]For a simulation of the game visit our platform (https://crossedmarket.herokuapp.com/demo/)

Table 1: TREATMENT OVERVIEW

	Separated	**Unique**
Hybrid	T1 − Separated	T2 − Unique
Only Artificial agents − Synthetic	T1− Separated − Synthetic	T2−Unique − Synthetic

3 Statistical price model: market and cross−impact estimation

Since the seminal paper of Kyle (1985), linear models for market-impact are widely used to study the effect of (aggregate) net order flow on price movements. To estimate market impact we rely on the well-known measure of order flow imbalance (OFI) proposed by Cont et al. (2014).

As described above, OFI represents the imbalance between supply and demand at the best bid and ask prices during a fixed time interval. It encompasses trades, limit orders and cancelations, see Cont et al. (2014). In particular, for each asset $i = 1, ..., M$ we compute $\boldsymbol{OFI}_t \in \mathbb{R}^M$ between period $t + k$ to t as

$$OFI_t = L^b_k - C^b_k - M^s_k - L^s_k + C^s_k + M^b_k \tag{1}$$

where M^b_k denotes the total size of buy trades at the current best ask, L^b_k C^b_k the total size of buy orders that arrived to and canceled from current best bid during the time interval. The quantities L^s_k, C^s_k, M^s_k for sell orders are defined analogously.

We then estimate the following model

$$\boldsymbol{r}_t = \mu + \Lambda\ \boldsymbol{OFI}_t + \boldsymbol{\varepsilon}_t \tag{2}$$

where $\boldsymbol{r}_t \in \mathbb{R}^M$ represents the assets returns, i.e. $p_{t+k} - p_t$, $\boldsymbol{\varepsilon}_t \in \mathbb{R}^M$ is the residual term and is uncorrelated from $\boldsymbol{OFI}_t$, i.e., $\text{cov}(\boldsymbol{\varepsilon}_t, \boldsymbol{OFI}_t) = 0$. The matrix of coefficient $\Lambda \in \mathbb{R}^{M\times M}$ is the *market-impact matrix*. In our case, with only two assets ($M = 2$)

$$\Lambda \equiv \begin{bmatrix} \lambda_{1,1} & \lambda_{1,2} \\ \lambda_{2,1} & \lambda_{2,2} \end{bmatrix}$$

The diagonal components of Λ represent the so-called *self-impact* coefficients, while the off-diagonal terms represent the *cross−impact* effect between the selected assets.

In particular, we have

$$\Lambda = Cov[\boldsymbol{OFI}_t]^{-1} Cov[\boldsymbol{OFI}_t, \boldsymbol{r}_t]$$

Let us indicate $Cov[\boldsymbol{OFI}_t]$ with $K \equiv \begin{bmatrix} k_1^2 & \rho k_1 k_2 \\ \rho k_1 k_2 & k_2^2 \end{bmatrix}$ where k_1^2 represents the variance of OFI_1, k_2^2 represents the variance of OFI_2 and ρ the correlation between OFI_1 and OFI_2.

If we denote with C the covariance matrix between OFI and market returns, $\boldsymbol{r}_t$, i.e., $C = Cov[\boldsymbol{OFI}_t, \boldsymbol{r}_t] \equiv \begin{bmatrix} c_{11} & c_{12} \\ c_{21} & c_{22} \end{bmatrix}$, we obtain

$$\Lambda = \frac{1}{(1-\rho^2)} \begin{bmatrix} \frac{1}{k_1^2} & -\frac{\rho}{k_1 k_2} \\ -\frac{\rho}{k_1 k_2} & \frac{1}{k_2^2} \end{bmatrix} \begin{bmatrix} c_{11} & c_{12} \\ c_{21} & c_{22} \end{bmatrix}$$

$$\lambda_{1,2} = \frac{1}{(1-\rho^2)} \left[\frac{c_{12}}{k_1^2} - \frac{c_{22}\rho}{k_1 k_2} \right] \tag{3}$$

$$\lambda_{2,1} = \frac{1}{(1-\rho^2)} \left[\frac{c_{21}}{k_2^2} - \frac{c_{11}\rho}{k_1 k_2} \right] \tag{4}$$

As it appears from the above, the identification of λ_{12} and λ_{21} depends on the value of ρ. Indeed, for ZI agents and MMs, ρ is always equal to zero as, by construction, their orders are uncorrelated across markets, both in *T1-Separated* and *T2-Unique*, and any unobservable common component in the order flow of the two stocks can be ruled out. In other words, it is always possible to identify cross-impact parameters for artificial players. For human players, ρ needs to be distinguished into two components: $\rho_{human} = (\rho_{behavioural}, \rho_{portfolio})$. The first component, $\rho_{behavioural}$, is related to the correlation between the order flow of the two stocks due to human trading strategies across markets. As there is no reason to assume a different set of strategies across treatments, this component will remain equal across treatments. The second component, $\rho_{portfolio}$, is associated with the possibility for human traders to redirect liquidity across the two markets. In this case, as there is no possibility to move capital across markets in *T1-Separated*, $\rho_{portfolio}$ will be different from zero (by construction) only in *T2-Unique*. In other words, by comparing cross-market impact coefficients between treatments, we are able to identify the cross-impact due to liquidity change on market returns.

4 Hypotheses

In the following we formulate our hypotheses.

- Hypothesis 1 (H1): *cross−impact will be larger in T2-Unique than in T1-Separated.* Since there are no restrictions in moving the capital from one asset to the other in *T2-Unique*, we expect $\rho_{portfolio}$ to be greater in *T2-Unique* than in *T1-Separated*. Thus in *T2-Unique*, ρ will be larger for human participants and positive for ZI traders in reaction to human players' activity. Given the same $\rho_{behavioural}$ for humans, than we have $\lambda_{12_{T2-Unique}} > \lambda_{12_{T1-Separated}}$ and $\lambda_{21_{T2-Unique}} > \lambda_{21_{T1-Separated}}$.

- Hypothesis 2 (H2): *cross−impact will be asymmetric between the two assets*. In particular, we expect a larger cross−impact for the decreasing value asset than for the constant value asset, rather than the opposite. In other words, $\lambda_{12_{T2-Unique}} > \lambda_{21_{T2-Unique}}$. Indeed, the price bubble of the decreasing value asset is mainly driven by an endogenous mechanism, e.g., it does not depend on the price realization of the constant value asset and it is often observed in single asset experiments (see Palan 2013 for a review). In particular, should this hypothesis not be verified, it would imply a violation of dynamic arbitrage in the sense of Gatheral, see Gatheral (2010) and Schneider et al. (2019).

- Hypothesis 3 (H3): *Self−impact will not change significantly across treatments*. Intuitively, self-impact is a mechanical effect of order flow imbalance, as such should not be affected by the liquidity in the market. In other words, we expect that the removal of liquidity constraints will only affect cross−impact without significantly affecting the self-impact. That is, $\lambda_{11_{T2-Unique}} \sim \lambda_{11_{T1-Separated}}$ and $\lambda_{22_{T2-Unique}} \sim \lambda_{22_{T1-Separated}}$.

- Hypothesis 4 (H4): *Within treatments, the relationship between asset prices and fundamental values makes the cross−impact effect negligible*. This strong hypothesis can be tested by controlling for the distance of the two assets fundamental values, exploiting the experimental data features. In other words, we expect that *cross−impact effect to be larger during periods in which the fundamental values of the two assets are very close to each others*, that is when the distance between the two fundamental values becomes negligible.

As stated above, to ensure rigor and reliability in our experimental design and hypothesis formulation (Page et al. 2021), we also grounded our hypotheses on ABM simulation in Cordoni et al. (2021). Therein, agents with different trading programs mimick the behavior of simple two-assets trading strategies, like the directional (i.e. the same position in both markets) and the market-neutral ones (i.e. two opposite position in the two markets). The results of this simulation exercise are substantially in line with the hypotheses reported above. We only slightly modify the way in which we test H4. In particular, in equation (2) we now introduce an additional interaction with a dummy variable *Entanglement* equal to 1 if we are in a trading period in which the fundamental values of the two assets are very close to each other (that is, between period 5 and 10) and zero otherwise (see also section).[6]

5 Experimental evidence

Data collection started in June 2022 and ended in October 2022. Participants were randomly selected out of a pool more than 3000 students from 20 departments of the University of Pisa. Invitations were only sent to students enrolled in Economics and STEM subjects. The experiment was conducted in the EMBEDS laboratory of Sant'Anna School of Advanced

[6]Precisely, this hypothesis formulation is equivalent to that of Cordoni et al. (2021) since the logarithm of the ratio between the fundamental values is zero in the middle of the trading session.

Studies in Pisa. Each session was programmed to run one experimental market with a maximum of 10 human participants, 30 ZI agents, and 10 market-makers. Whenever we did not reach the number of 10 human players, the number of ZI traders was rescaled to maintain the proportion of 1:3 with the population of human players. We ran 24 market sessions in total, 12 for T1-Unique and 12 for T2-Separated. Overall, 217 participants took part in the experiment, 109 for T1−*Separated* and 108 T2−*Unique*. Thus, on average, we have about 9 participants per sessions. The average age of participants was 24.50 and 50% were female. The average earning was about 15 Euro. We collected information about participants' level of financial literacy. On average, participants were able to answer correctly slightly more than two out of three financial questions.

To create synthetic data and generate the same behavior of artificial agents observed in the laboratory sessions, each experimental market had a unique seed number that was used to replicate the experimental session without human participants (24 market session in totals). We also add 16 market sessions to increase statistical power. Thus, in total, we have synthetic data for the same 24 market sessions as those conducted in the laboratory, plus additional data, for a total of 40 market sessions .[7]

5.1 Experimental results

5.1.1 Descriptive statistics

We began by calculating OFI_1 and OFI_2. As defined by OFI (refer to Mertens et al. 2019), it is necessary to divide the order book at a specific frequency to aggregate the information for the computation of this measure. Informed by the simulation analysis conducted by Cordoni et al. (2021), we chose to divide the order book to obtain six observations, or subrounds, within each 180-second trading period. Consequently, we established the interval size at 27 seconds for the order book, ensuring that at least one observation relevant to OFI measures and mid-returns would be available in all six subrounds. However, we excluded the initial observations from each trading period in our regression analysis as we focus on mid-returns. This exclusion resulted in five point-observations for each of the 15 trading periods of the game, equating to 75 observations for OFI_1 and OFI_2 for each experimental session. Given that there are 24 market sessions in total, our sample from the laboratory comprises a total of 1800 observations (900 for *T1-Separated* and 900 for *T2-Unique*).

In the Appendix, Table (11) reports summary statistics for our variables of interest across treatments. Specifically, the OFI measures are reported both at an aggregate level and separately for human and ZI agents. The OFI measures for ZI traders are calculated by subtracting the contributions of human players from the overall OFI. Theoretically, these measures also include MMs' activity. However, as shown in Table (2), MMs' contributions to OFI measures are negligible since they only place limit orders simultaneously on both sides of

[7] We also generated additional synthetic data with 40 ZI traders, and replicate the analysis with only 24 market sessions. Results are analogous and available upon request.

the book, with cancellations and submissions almost balancing each other. Therefore, we henceforth refer exclusively to the OFI measures for ZI players as OFI_{ZI}. From this table, it also emerges that the contribution of human players to trading activity is not negligible, despite them being only 1/3 of ZI traders, as they are involved as counterparts in at least 42-45% of the executed trades. Table (12) in the Appendix presents the correlations across OFI measures. It is crucial to note that the correlations between the OFI measures of both human and ZI traders are not statistically different from zero. This is a result of our experimental design, which rules out any unobservable common component for ZI traders and only includes behavioral strategy for human traders. See section 3. The only significant correlations observed are between humans and ZI traders, which, as mentioned earlier, are expected due to the construction of the measures.

Table 2: SUMMARY STATISTICS: EVENT TYPE BY AGENTS

	Humans	***ZI agents***	***Market−Makers***
		T1−Separated	
Cancellation	25%	31%	45%
Execution	45%	55%	–
Submission	16%	37%	47%
		T2−Unique	
Cancellation	19%	28%	53%
Execution	42%	58%	–
Submission	11%	37%	52%

To provide a preliminary indication of price bubbles across treatments, Table (3) computes the Relative Absolute Deviation (RAD) of each stock price from its expected fundamental value, as described by Kirchler et al. (2012). The formula is $RAD = (1/T)\Sigma_r^{1/T}|\bar{P}_r - FV_r|/|FV_r|$, where $\bar{P}_r$ is the average price in round r, and FV_r is the expected fundamental value in that round. A RAD of 0.1, for instance, signifies that prices, on average, deviate by 10 percent from the fundamental value. As anticipated, larger bubbles (i.e., higher RAD levels) were observed for the asset with a decreasing value compared to that with a constant value, with RAD measures being notably larger in *T2−Unique* than in *T1−Separated*. The price dynamics for orders initiated by either human or ZI players were substantially similar, aligning with expectations since artificial traders were designed to mimic human behavior in SSW experimental designs, replicating the same price dynamics and trading volumes typical of human participants (Duffy et al. 2006; Baghestanian et al. 2015; Cordoni 2022). This similarity is further evidenced by Figures (1) and (2) in the Appendix, which distinguish between the average price dynamics for the two assets across treatments for orders initiated by humans and artificial players, respectively.

Table 3: FINANCIAL BUBBLES: RELATIVE ASBOLUTE DEVIATIONS ACROSS TREATMENTS

	RAD_1	RAD_2
T1−Separated	1.248 (0.527)	0.085 (0.297)
T2−Unique	1.763 (0.717)	0.369 (0.297)
T1−Separated−Synthetic	1.500 (0.495)	0.005 (0.394)
T2−Unique−Synthetic	1.912 (0.665)	0.283 (0.407)

5.1.2 Cross-impact estimations

We now employ our OFI measures to estimate model (2). In the first regressions we aggregate all the information of the order book without distinguishing between human participants and artificial traders. Table (4) reports estimation of the price impact matrix in both treatments: the upper part of represents the impact−matrix for *T1−Separated,* while the bottom part represents the impact−matrix for *T2−Unique*. In both cases the diagonal elements are the estimates for self−impact i.e., $\lambda_{1,1}$ and $\lambda_{2,2}$, while the off−diagonal elements represent the main measures of our interest, i.e. $\lambda_{1,2}$ the cross−impact effect of OFI_1 on $return_2$ and $\lambda_{2,1}$ the cross−impact effect of OFI_2 on $return_1$. As expected, self−impact measures are statistically significant and quite stable across treatments: an increase by one in the OFI_1 results in about 20−25 percentage points (p.p. in the following) in the $return_1$, while in OFI_2 results in slightly more than 32-33 p.p. in the $return_2$. Even when looking at the off−diagonal elements the economic significance of the coefficients appears very similar across treatments: an increase by one in OFI_2 implies between 5−7 p.p. increases in the $return_1$, while an increase by one in OFI_1 implies a negligible variation in $return_2$. The cross−impact parameter, however, is statistically significant at 5% level only in *T2−Unique.* In general, we do not observe significant difference across treatments.

To better understand the dynamics of the cross−impact, we now check in Table (5) whether cross−impact becomes larger when individuals have more difficulties in distinguishing the value of the two assets. We therefore compute the impact-matrix for periods of *Entanglement* (right part of the Table 5), in which the value of the two stocks are close to each other, and periods of no Entanglement (left−part of Table 5). Results for self−impact parameters, i.e. $\lambda_{1,1}$ and $\lambda_{2,2}$, are substantially in line with those in Table (4): we only observe an increase in the magnitude for the $\lambda_{2,2}$ in periods of entanglement in *T2−Unique*. Results for the cross−impact parameters substantially confirm the results previously observed in Table (4) for $\lambda_{2,1}$ in periods of no entanglement, while $\lambda_{1,2}$ now emerges negative and statistically significant in periods of entanglement. The economic impact is also quite large

Table 4: Cross−impact: Experimental results

	$return_1$	$return_2$
	T1−Separated	
OFI_1	0.222***	-0.032
	(0.038)	(0.037)
OFI_2	0.052	0.322***
	(0.036)	(0.035)
	T2−Unique	
OFI_1	0.266***	-0.007
	(0.039)	(0.038)
OFI_2	0.073*	0.334***
	(0.036)	(0.035)
N obs	1800	1800

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$
cross-impact$_1$T1 vs T2: ns
cross-impact$_2$ T1 vs T2: ns

being the coefficient equal to −11.2 p.p. increase in $return_2$ for an increase in one in OFI_1 in *T1−Separated*. In this case, the difference between treatments is also statistically significant (p−value=0.044)

Overall, this first set of regressions confirms our preliminary hypotheses of an asymmetric cross−impact which is larger in *T2−Unique*, and a price impact dynamics that is more complex when we distinguish between periods of *Entanglement*. Indeed, the difference between treatments appears statistically significant only in those period in which the fundamental values of the two assets are close to each others. Contrary to our expectations, however, we observe a larger cross−impact that goes from asset 2 to asset 1, and a negative cross−impact that goes from asset 1 to asset 2. To better understand these results, in the next section we distinguish the behavior of humans from the one ZI players instead of aggregating the information altogether.

5.2 Experimental results: human vs artificial players

In Table (6) we present the general results: the left−panel reports the impact−matrix for humans while the right-panel reports the impact−matrix for ZI players. The first important effect to notice is that the cross−impact $\lambda_{2,1}$ observed in Table (4) and (5) can be largely attributed to ZI traders activity. As explained in the subsequent section, this is a second order effect, i.e. it is due to the reaction of ZI traders to human presence, since in synthetic data without humans players the cross impact is absent. In addition, the cross−impact $\lambda_{1,2}$ in *T2-Unique* (which we expected to observe) now emerges in the impact-matrix of the humans. The effect is equal to 6 p.p. and significant at 10% level. Results for self-impact instead highlight that the activity of ZI traders contributes significantly to the increase in prices while humans tend to reduces it.

Table 5: Cross−impact & Entanglement: Experimental results

	$return_1$	$return_2$	$return_1$	$return_2$
	$Entanglement = 0$		$Entanglement = 1$	
	$T1-Separated$		$T1-Separated$	
OFI_1	0.254***	0.022	0.174**	-0.116*
	(0.049)	(0.047)	(0.060)	(0.058)
OFI_2	0.054	0.347***	0.048	0.280***
	(0.046)	(0.044)	(0.059)	(0.057)
	$T2\text{-}Unique$		$T2\text{-}Unique$	
OFI_1	0.242***	-0.046	0.301***	0.052
	(0.050)	(0.048)	(0.062)	(0.060)
OFI_2	0.092*	0.374***	0.044	0.273***
	(0.047)	(0.045)	(0.057)	(0.055)
N obs	1800		1800	

$*p < 0.10, **p < 0.05, ***p < 0.01$

	cross-impact$_1$ T1 vs T2: ns	*cross-impact*$_1$ T1 vs T2: ns
	cross-impact$_2$ T1 vs T2: ns	*cross-impact*$_2$ T1 vs T2: 0.044

Table 6: Cross−impact Human vs ZI agents: Experimental results

	Humans			ZI	
	$return_1$	$return_2$		$return_1$	$return_2$
	$T1 - Separated$			$T1 - Separated$	
OFI_1	-0.153***	0.027	OFI_1	0.383***	-0.019
	(0.034)	(0.033)		(0.040)	(0.038)
OFI_2	-0.028	-0.119***	OFI_2	0.039	0.435***
	(0.034)	(0.033)		(0.035)	(0.034)
	$T2 - Unique$			$T2 - Unique$	
OFI_1	-0.182***	0.060*	OFI_1	0.383***	0.025
	(0.035)	(0.034)		(0.038)	(0.037)
OFI_2	0.039	-0.066**	OFI_2	0.092**	0.435***
	(0.034)	(0.033)		(0.036)	(0.035)
N obs	1800			1800	
	cross-impact$_1$ T1 vs T2: ns			*cross-impact*$_1$ T1 vs T2: ns	
	cross-impact$_2$ T1 vs T2: ns			*cross-impact*$_2$ T1 vs T2: ns	

Table 7: Cross−impact: Human vs ZI agents in Periods of Entanglement

	Humans			Artificials	
	$return_1$	$return_2$		$return_1$	$return_2$
$Entanglement = 0$					
	$T1 - Unique$			$T1 - Unique$	
OFI_1	-0.116**	0.114***	OFI_1	0.398***	0.025
	(0.046)	(0.044)		(0.051)	(0.049)
OFI_2	0.048	-0.004	OFI_2	0.030	0.470***
	(0.043)	(0.042)		(0.044)	(0.043)
	$T2 - Unique$			$T2 - Unique$	
OFI_1	-0.154***	0.032	OFI_1	0.359***	0.003
	(0.045)	(0.043)		(0.048)	(0.046)
OFI_2	-0.040	-0.077*	OFI_2	0.143***	0.492***
	(0.044)	(0.043)		(0.047)	(0.045)
$Entanglement = 1$					
	$T1 - Separeted$			$T1 - Separeted$	
OFI_1	-0.200***	-0.092*	OFI_1	0.367***	-0.079
	(0.053)	(0.051)		(0.064)	(0.062)
OFI_2	0.025	-0.164***	OFI_2	0.053	0.365***
	(0.053)	(0.052)		(0.059)	(0.057)
	$T2 - Unique$			$T2 - Unique$	
OFI_1	-0.234***	0.093*	OFI_1	0.428***	0.047
	(0.057)	(0.055)		(0.063)	(0.061)
OFI_2	-0.020	-0.184***	OFI_2	0.023	0.351***
	(0.055)	(0.053)		(0.056)	(0.054)
N obs	1800			1800	

Table 8: ZI agents - Synthetic vs experimental data

	Experimental data			**Synthetic data**	
	$return_1$	$return_2$		$return_1$	$return_2$
	T1-Separated			T1-Separated	
OFI_1	0.426***	-0.025	OFI_1	0.410***	0.031
	(0.038)	(0.036)		(0.030)	(0.028)
OFI_2	0.041	0.445***	OFI_2	-0.004	0.499***
	(0.035)	(0.033)		(0.027)	(0.026)
	T2-Unique			T2-Unique	
OFI_1	0.425***	0.007	OFI_1	0.418***	0.007
	(0.037)	(0.035)		(0.030)	(0.028)
OFI_2	0.096**	0.463***	OFI_2	0.006	0.501***
	(0.035)	(0.034)		(0.028)	(0.026)
N obs	1800			3000	

In Table (7) we repeat the same analysis but now distinguishing between periods of *Entanglement*. Once again, the cross−impact $\lambda_{2,1}$ emerged in *T2-Unique* and can be attributed to the activity of ZI traders. This exercise now show that this effect emerges strongly in periods with no entanglement. We also observe that the cross−impact of $\lambda_{1,2}$ can be attributed to the activity of humans players in *T2-Unique* in periods of entanglement (slightly larger 9.3 p.p. but still significant at 10% level). In addition, two new results emerge for human players in *T1-Separated*: a strong positive $\lambda_{1,2}$ appears (equal to 11.4 p.p. and statistically significant at 5%) in periods of no entanglement and a negative one (equal to -9.2 p.p. and statistically significant at 10% level) in periods of entanglement.

Overall, these results confirm the asymmetric effects of cross−impact, which emerge significant in both treatments. However, it appears quite stable and positive in *T2-Unique* where expected, while it switch signs for humans players in *T1-Unique* depending of how close the fundamental values of the two assets are close to each other. Moreover, we can now attribute the cross−impact $\lambda_{2,1}$ to the trading activity of ZI traders reacting to the human traders, while for $\lambda_{1,2}$ can be largely attribute to human traders.

In the next section, we compare our experimental data with our synthetic data to confirm that even when the activity of the ZI traders appear as the relevant one, we can attribute it to the reaction of artificial players to the presence of human players in the market.

5.3 Comparing experimental and synthetic data

In this section we finally compare the activity of ZI traders when playing along with human players with their activity when they playing on their own, i.e. our synthetic control data. Results are reported in Table (8). Once again we report the impact-matrix for the two treatments (right-panel for synthetic data and left-panel for experimental data). Indeed, we observe that cross-market impact is never significant when using synthetic data, while it is positive (9.6 p.p.) and statistically significant (at 5% level) in experimental data in *T2-Unique*.

In Table (9) we again distinguish between periods of more or less entanglement. Indeed, we can see that the effect identified above, i.e. the cross−impact $\lambda_{2,1}$ for ZI traders on as-

Table 9: ZI traders: Synthetic vs experimental data

	Synthetic data			**Experimental data**	
	$return_1$	$return_2$		$return_1$	$return_2$
			$Entanglement = 0$		
	$T1 - Separated$			$T1 - Separated$	
OFI_1	0.390***	0.047	OFI_1	0.437***	-0.018
	(0.035)	(0.034)		(0.048)	(0.046)
OFI_2	0.008	0.514***	OFI_2	0.025	0.468***
	(0.035)	(0.034)		(0.044)	(0.042)
	$T2 - Unique$			$T2 - Unique$	
OFI_1	0.420***	0.019	OFI_1	0.397***	-0.010
	(0.036)	(0.034)		(0.046)	(0.044)
OFI_2	0.028	0.504***	OFI_2	0.145**	0.507***
	(0.035)	(0.033)		(0.046)	(0.044)
	$return_1$	$return_2$		$return_1$	$return_2$
			$Entanglement = 1$		
	$T1 - Separated$			$T1 - Separated$	
OFI_1	0.407***	-0.038	OFI_1	0.462***	-0.008
	(0.063)	(0.060)		(0.056)	(0.053)
OFI_2	0.070	0.404***	OFI_2	-0.022	0.476***
	(0.058)	(0.055)		(0.043)	(0.041)
	$T2 - Unique$			$T2 - Unique$	
OFI_1	0.470***	0.034	OFI_1	0.413***	-0.020
	(0.063)	(0.059)		(0.054)	(0.052)
OFI_2	0.031	0.403***	OFI_2	-0.030	0.495***
	(0.054)	(0.052)		(0.045)	(0.043)
N obs	3000			1800	

set 1 is even larger in periods of no entanglement and again statistically different from the estimated cross-impact parameter derived from synthetic data.

Finally, to get more precise insights on the cross−impact results we look at the possible factor trading strategies employed by human participants. In particular, we can categorize traders' strategies by looking at the resulting sign of our OFI measures. We define traders "directionalist" if the average measures of OFI_1 and OFI_2 in each period are either both positive or both negative, while we define traders "market-neutral" if the average measures of OFI_1 and OFI_2 exhibit opposite sign in the period. However, we denote those strategies when one between OFI_1 and OFI_2 is equal to zero as "other".

We did not find any significant differences between treatments in terms of strategies. We report in Figure (3) and (4) the share of each category between directionalist and market neutral over time in T1-*Separated* and T2-*Unique*. On average among the trading periods, we observed that approximately 40% of traders employ a directional strategy, and another 40% adopt a market-neutral approach. This is a strength of our design as it does not impose any behavioral constraints across different treatments.

Table 10: Conclusion overview

Hyp.	**T1- Synthetic**	**T2 - Synthetic**	**T1 - Separeted**	**T2 - Unique**
H1-Existence of CI	×	×	×	✓
H2-Asymmetries of CI	–	–	–	✓
H3-Invariance of (self)MI	✓	✓	✓	✓
H4- Period of confusions	–	–	✓	✓

6 Discussion and Conclusions

Cross−impact refers to the phenomenon where trading one asset affects the price of another asset. While self-impact can be related to mechanical movements generated by the order book structure (Cont et al. 2014), cross−impact is a more complex phenomenon influenced by multiple factors (Capponi et al. 2020,Mastromatteo et al. 2017,Schneider et al. 2019 Cordoni et al. 2020, Mastromatteo et al. 2017). One such factor is the existence of market participants who trade in multiple assets simultaneously using specific strategies. Additionally, the interaction between different trading strategies can also generate cross−impact, as seen in Cordoni et al. (2021). Another mechanism is the transmission of information between assets, where price movement in one asset provides information about the value of another asset. For example, if the price of a stock in a particular sector increases, it could inform the value of another sector, subsequently affecting the prices of stocks in that sector (Mastromatteo et al. 2017). Furthermore, the market structure, including trading rules and regulations for moving capital, as considered in this experimental analysis, can also affect cross−impact. In other words, cross−impact results from the combination of various mechanisms that have a significant impact on real financial markets, especially during bubble crashes, such as the liquidity crash in 2010.

The empirical literature has proposed a number of ways of estimating it through order flow imbalance measures over a sufficiently long time period. However, a number of challenges remains to reach its statistical identification (Min et al. 2018; Capponi et al. 2020). Using an experimental approach greatly simplifies cross−impact identification, for example, by reducing noise, matrix dimensionality and isolating one possible mechanism. This paper follows this route.

Specifically, we experimentally investigate cross-impact in a hybrid market, featuring both human and artificial players: near-zero-intelligence (ZI) traders and market-makers. ZI traders emulate the trading behavior of inexperienced human participants by placing random buy and sell orders based on historical prices and a simple probabilistic model. Market Makers (MMs) are a more sophisticated type of agent, designed to provide continuous liquidity by posting buy and sell quotes according to the Avellaneda-Stoikov model, a strategy that optimizes their inventory and risk aversion over time, thus reflecting the behavior of professional liquidity providers and ensuring a stable trading environment. The inclusion of MMs is particularly novel as they do not engage in active trading unless there is a liquidity shortage, allowing us to isolate and observe the pure effects of human and ZI trader interactions on market dynamics. Moreover MMs act independently on each asset, removing the possibility that cross-impact is driven (also) by commonality in liquidity provision.

To capture cross-impact effects, our experimental design manipulate portfolio liquidity. In the *T1-Separated* treatment, traders are restricted from reallocating liquidity between separate portfolios for each asset, simulating a segmented market scenario. Conversely, the *T2-Unique* treatment allows traders to freely allocate liquidity within a single, unified portfolio, mirroring conditions where capital can move freely between assets. This controlled variation between treatments enables a precise examination of how liquidity constraints and capital mobility influence cross-market impacts and bubble formation, providing valuable insights into the interconnectedness of financial markets.

To validate our model and ensure rigor and reliability in our experimental design and hypothesis formulation, we referenced the experimental hypotheses discussed in Cordoni et al., 2021. In that study, within an agent-based model (ABM) developed in discrete time, we simulated price dynamics in a similar experimental market using only artificial agents and specific trading strategies. These hypotheses align with those formulated in our current paper. Specifically, we anticipated significant (asymmetric) cross-impact in scenarios with portfolio effects for the decreasing value asset, while we expected self-impact to remain stable. This alignment reinforces the robustness of our experimental design and the anticipated outcome

Our results are largely consistent with our hypotheses (see Table 10). As expected, there is a significant and asymmetric cross-impact in the treatment with unrestricted liquidity movement, namely *T2-Unique*. This effect, originating from the asset with a downward-sloping fundamental value to the constant-value asset, becomes more pronounced during periods of entanglement, i.e., when the two assets have closer fundamental values. Interestingly, we also identify a robust cross-impact effect in *T2-Unique*, transitioning from the constant-value asset to the downward-sloping one.

Additionally, to further validate our experimental results, we generate synthetic data in continuous time and without trading strategies. This approach allows us to compare laboratory evidence with data where only artificial players are active in the markets. By comparing synthetic with experimental data, we find that the cross-impact effect emerges as a second-order effect, attributed to ZI traders responding to human activity. Furthermore, we also observe a cross-impact effect (although not consistently) for humans in *T1-Separated*. Importantly, no significant difference emerges when comparing the trading strategies of human players across treatments. As expected, self-impact appears stable across treatments.

Overall, these results confirm that cross-impact is closely linked to the amount of liquidity available in the market and tends to increase with the degree of market entanglement (i.e., when it becomes difficult to differentiate the fundamental values of stocks). While our study did not explore the broader origins and causes of cross-impact (such as information diffusion or spillover effects across different sectors), our findings nonetheless highlight a critical aspect of capital movements across markets. Specifically, our results suggest that significant spillover effects can occur from one market to another, particularly when participants can exploit market dynamics. This is evident in our experiment where human players were able to take advantage of bubble formations induced by inexperienced (both artificial and human) traders. This exploitation amplifies cross-impact, leading to greater market instability. To mitigate these adverse effects, our findings support the implementation of restrictions on capital movements, at least in period of suspected bubble formation. By limiting the ability to freely move liquidity between different assets, it is possible to reduce cross-impact and, consequently, enhance market stability. Such measures could include segmented portfolios or temporary restrictions on certain types of trades. These interventions can help prevent the amplification of speculative bubbles and reduce the likelihood of spillover effects that destabilize multiple markets.

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7 Additional Tables and Figures

Table 11: SUMMARY STATISTICS

	T1-Separated					T2-Unique				
VARIABLE	Mean	Std. Dev.	Min	Max	Obs	Mean	Std. Dev.	Min	Max	Obs
$Return_1$	-.193	.94	-2.556	2.301	900	-.147	.986	-3.622	2.822	900
$Return_2$	.013	.947	-2.559	2.307	900	.062	.951	-2.146	2.606	900
OFI_1	.117	1.013	-2.999	3.85	900	-.021	1.046	-3.579	3.813	900
OFI_2	-.068	1.133	-4.087	3.783	900	-.004	1.083	-3.512	4.429	900
$OFI_{1,human}$	-.008	.903	-2.552	2.385	900	-.069	.866	-2.562	2.308	900
$OFI_{1,ZI}$	.124	.778	-2.13	2.499	900	.049	.804	-3.405	2.454	900
$OFI_{2,human}$	-.054	.896	-2.354	2.382	900	0	.883	-2.606	2.9	900
$OFI_{2,ZI}$	-.014	.859	-2.309	2.757	900	-.004	.847	-2.366	3.55	900

Table 12: CORRELATION ACROSS OFI MEASURES

	T1−separated				
	$OFI_{1,human}$	$OFI_{2,human}$	$OFI_{1,ZI}$	$OFI_{2,ZI}$	***Overall***
$OFI_{1,human}$	1				**0.032**
$OFI_{2,human}$	-0.017	1			
$OFI_{1,ZI}$	-0.217*	0.028	1		
$OFI_{2,ZI}$	0.017	-0.216*	-0.014	1	
	T2−Unique				
$OFI_{1,human}$	1				**-0.002**
$OFI_{2,human}$	0.057	1			
$OFI_{1,ZI}$	-0.281*	-0.049	1		
$OFI_{2,ZI}$	-0.069	-0.166*	0.072	1	

*$p < 0.05$, Pairwise correlations with Bonferroni correction

Figure 1: Experimental results: Price dynamics over treatments human players

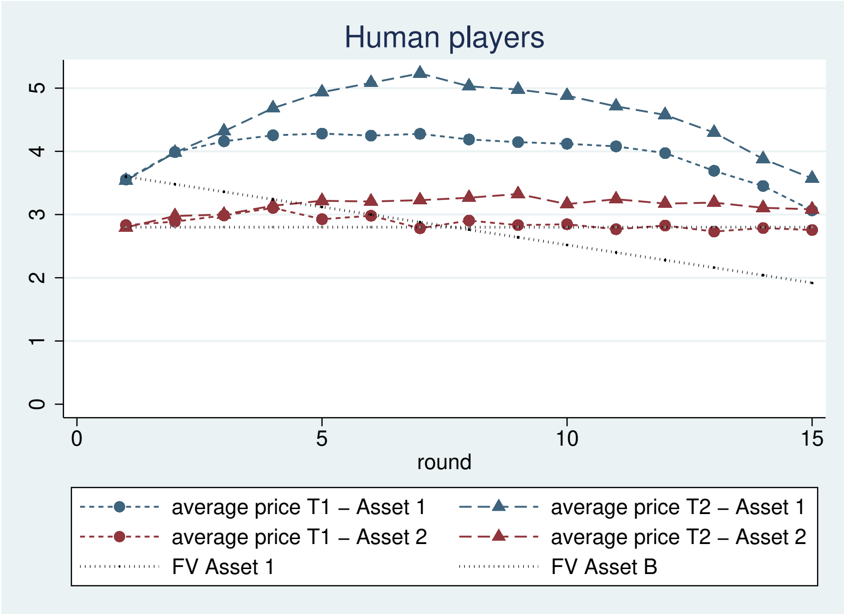


Figure 2: Experimental results: Price dynamics over treatments ZI players

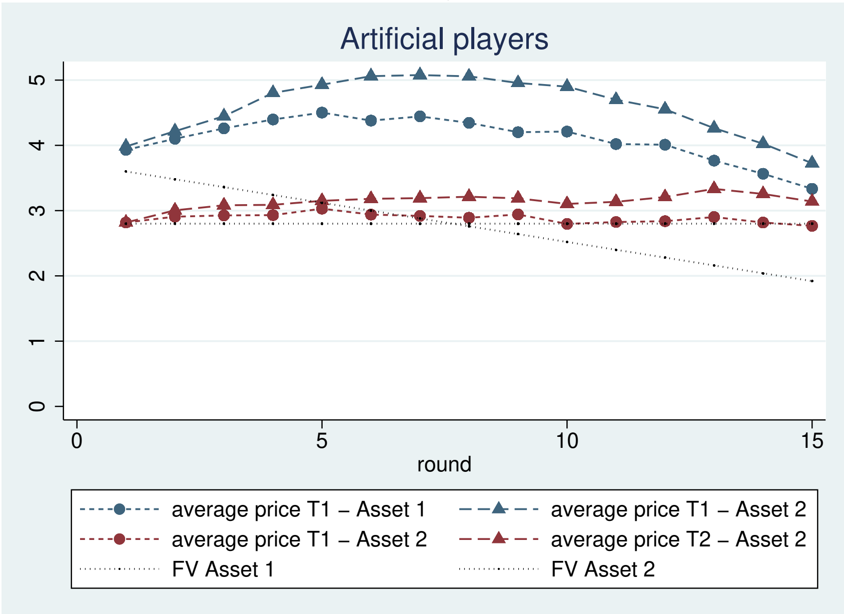


Figure 3: TRADING STRATEGIES ACROSS PERIODS: MARKET NEUTRAL

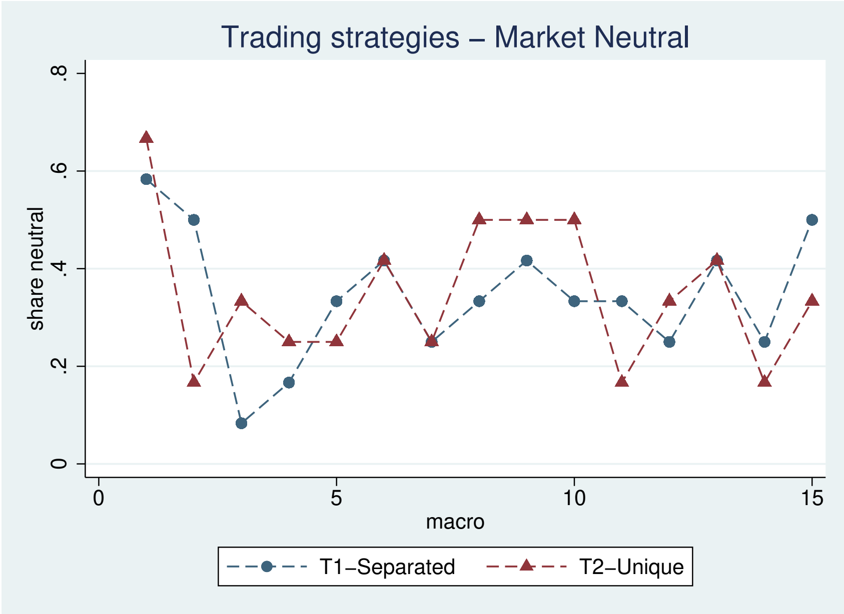
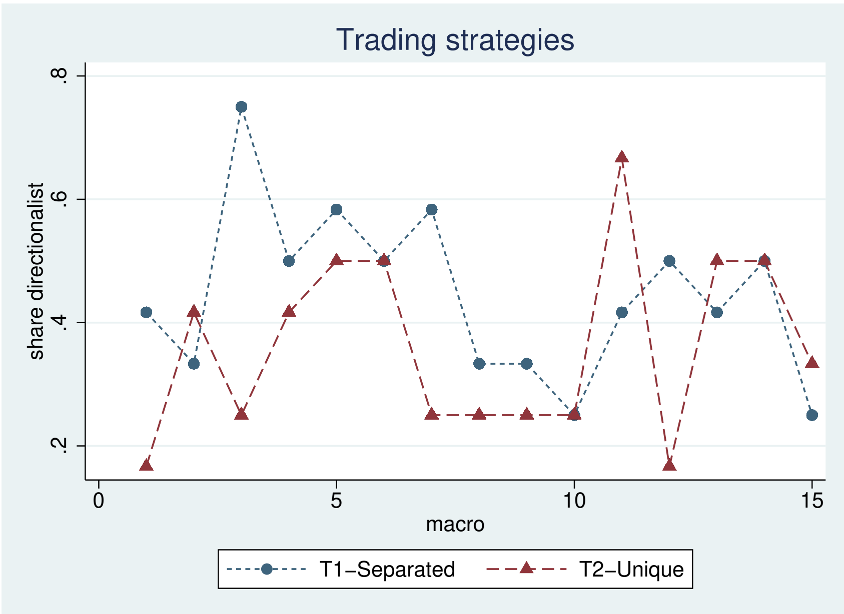


Figure 4: TRADING STRATEGIES ACROSS PERIODS: DIRECTIONALIST



Dear participant, we welcome you to this experimental session and kindly ask you to refrain from talking to other participants for the duration of the experiment. If you experience difficulties, please contact one of the supervisors.

This experiment will last about 1 hour. For your participation you will receive 5 Euros. Based on the choices you make during this experiment, you will be able to earn more Euros. The currency of this experiment will be **the ECU.** You will earn **ECUs** which will translate into Euros at the end of the experiment.

One ECU corresponds to 0.35 Euros (i.e. 1 ECU= 0.35 Euros).

General information

This experiment reproduces a market in which traders can trade the shares of two fictitious companies, simply called **company A** and **company** B, for **15 consecutive periods.**

Market description

The market is made up of 30 subjects: 10 human traders and 20 artificial traders, i.e., algorithms. Each of the 10 human traders (and therefore yourself) receives an initial endowment of **4 shares** in addition to virtual money (ECU): **two shares of asset** A, **two shares of asset** B, plus **20 ECU** in cash. At the beginning of the experiment, a type A share has an estimated fundamental value (hereinafter VF) of **3.60 ECU,** while a type B share has an estimated fundamental value **of 2.80 ECU.**

[T2-Unique] Evaluating the shares at their initial FV we obtain that the portfolio of each subject at the beginning of the experiment amounts to **32.80 ECU** (=3.60*2+2.80*2+20=32.80).

[T1-Separated] Evaluating the shares at their initial FV and equally dividing the available cash, we obtain each subject at the beginning of the experiment has a portfolio for trading asset A equal to **17.20 ECU (=3.60*2+10)** and a portfolio for trading in asset B equal to **15.60 ECU (=2.80*2+10).** Thus in total **32.80**

[T2-Unique] In any period, you can sell and/or buy shares.
[T1-Separated] In any period, you can sell and/or buy shares but the cash available in portfolio A cannot be used to for investing in market B (and viceversa).

The exchanges are made in the form of a double auction, i.e. each trader can appear as a buyer or seller (but you can never buy and sell yourself). Considers that:

1. an order can be **cancelled**

2. an order can be **modified**

3. if an order is satisfied by a counterpart, it will be executed automatically (for example if a trader enters a buy order of 100 and there is a seller in the book with a sell order less than or equal to 100, the order is **automatically** executed to 100).

In particular, to carry out the exchanges you have two options: 1) **select an offer already present** in the market with the relevant button; 2) enter your offer using the ORDER button.

NB: In this last case, your order will be automatically executed (if it turns out to be the best and is executable), otherwise your order remains in the market until it is cancelled. Your portfolio, shares and virtual stocks of ECUs will be automatically transferred to the next trading period. Each trading period ends automatically after **180 seconds.**

Also keep in mind that your ECUs and stock inventory can never drop below zero.

The list (i.e., the ***book)*** of orders is empty at the beginning of each period and is **anonymous,** i.e. **the identity of the trader who sends an order is not displayed,** whether they are human or artificial agents.

Dividends

At the end of each trading period, each stock pays a **dividend *(profit or cost of holding)*** which is added to your position in ECUs.

The dividend of activity A amounts to **0, 0.1, 0.16** or **0.22** ECU, with equal probability of realisation. Thus, the **average dividend** of a type A asset amounts to ECU **0.12** per period. Asset A has a duration of 15 trading periods, i.e., after dividends have been paid at the end of period 15, assets A are repurchased by the investigator at a price of 1.80 **ECU** each.

Asset B's dividend is **0.1, 0.2** or **0** ECU and may involve **holding costs** of **-0.1 or -0.2** ECU, each with equal probability of realisation. Thus, the **average dividend** of a type B asset amounts to **0** ECU at the end of each period. Asset B has a duration of 15 trading periods, ie, after dividends have been paid at the end of period 15, Asset B is repurchased by the investigator at a price of 2.80 **ECU .**

Calculate your earnings

At the end of the market (after 15 periods), your total earnings will equal your **ECU** cash holdings **plus the value of your shares . At the end of the experiment, type-A shares have a value of 1.80 each, while type B has a value 2.80.**

So the **total earnings in ECUs** at the end of the experiment are equal to

*Earnings = ECU Available + (Num shares A) * 1.80 + (Num shares B) * 2.80*

The total number of ECUs must be translated into Euros at the following rate: one ECU corresponds 0.35 Euros

Fundamental Value (FV)

For any stock, at any time, the fundamental value (FV) is given by the following formula

FV = (15- Current period + 1) * average dividend + Terminal Value,

The following tables (1) and (2) may help you make your decisions.

The first column, labeled ***"Current Period"*** indicates the period iwhich the FV is calculated. The second column indicates the number of holding periods, e.g. from the period of the second column until the end of the market (15). The third column, labeled ***"Average Dividend"*** provides the average dividend amount in each period for each unit held in your portfolio/inventory. The fourth column, labeled Terminal value, reports the terminal value of the asset, while the fifth column, labeled ***"Fundamental Value",*** gives the expected total dividend earnings (by asset) for the remainder of the experiment plus the terminal value at the period 15. That is, for each unit you hold in your inventory for the remainder of the market, you receive in expectation the amount listed in column 5, which is defined as the current period's FV: the number in column 5 is calculated by multiplying the numbers in column 2 and 3 + column 4.

For example, in Table (1) you get the FV for a type A stock. Let's say for example that there are 4 periods remaining in a market. Since the dividend on a unit of asset expects a dividend of 0.12 ECU (per period for each asset), if you hold an asset for the remaining 4 periods, the total dividend paid on the unit over 4 periods is expected to be 4* 0.12 = 0.48 to which is added a terminal value of 1.80. The FV at period 12 is therefore 2.28.

Tab1

Periodo	Numero periodi detenzione	x	Divendo medio	+	Valore Terminale=	Valore Fondamentale A
1	15		0.12		1.80	3.60
2	14		0.12		1.80	3.48
3	13		0.12		1.80	3.36
4	12		0.12		1.80	3.24
5	11		0.12		1.80	3.12
6	10		0.12		1.80	3
7	9		0.12		1.80	2.88
8	8		0.12		1.80	2.76
9	7		0.12		1.80	2.64
10	6		0.12		1.80	2.52
11	5		0.12		1.80	2.4
12	4		0.12		1.80	2.28
13	3		0.12		1.80	2.16
14	2		0.12		1.80	2.04
15	1		0.12		1.80	1.92

Tab 2

Periodo	Num periodi di dentenzione	Divdendo medio	+	Valore Terminale =	Valore Fondamentale
1	15	0		2.8	2.8
2	14	0		2.8	2.8
3	13	0		2.8	2.8
4	12	0		2.8	2.8
5	11	0		2.8	2.8
6	10	0		2.8	2.8
7	9	0		2.8	2.8
8	8	0		2.8	2.8
9	7	0		2.8	2.8
10	6	0		2.8	2.8
11	5	0		2.8	2.8
12	4	0		2.8	2.8
13	3	0		2.8	2.8
14	2	0		2.8	2.8
15	1	0		2.8	2.8