A test of the Modigliani-Miller invariance theorem and arbitrage in experimental asset markets

Gary Charness & Tibor Neugebauer*

Abstract: Modigliani and Miller (1958) show that a repackaging of asset return streams to equity and debt has no impact on the total market value of the firm if pricing is arbitrage-free. We test the empirical validity of this invariance theorem in experimental asset markets with simultaneous trading in two shares of perfectly-correlated returns. Our data support value invariance for assets of identical risks when returns are perfectly correlated. However, exploiting price discrepancies has risk when returns have the same expected value but are uncorrelated, and we find that the law of one price is violated in this case. Discrepancies shrink in consecutive markets, but seem to persist even with experienced traders. In markets where overall trader acuity is high, assets trade closer to parity.

Keywords: Modigliani-Miller theorem, asset market, twin shares, experiment, limits to arbitrage, perfect correlation, experience, cognitive reflection test, risk aversion

JEL Codes: C92, G12

* Tibor Neugebauer is at the University of Luxembourg. Gary Charness is at University of California, Santa Barbara. The authors have no conflicts of interest to disclose. The authors obtained IRB approval for data collection. We gratefully acknowledge the helpful comments of Bruno Biais (the Editor), the anonymous Associate Editor, two anonymous reviewers, Peter Bossaerts, Martin Duwfenberg, Darren Duxbury, Catherine Eckel, Ernan Haruvy, Chad Kendall, Roman Kräussl, Ulf von Lilienfeld-Toal, Raj Mehra, Luba Petersen, Jianying Qiu, Kalle Rinne, Jean-Charles Rochet, Jason Shachat, Julian Williams for helpful comments. We also thank seminar participants at Durham Business School, University of Luxembourg, Radboud University of Nijmegen, and at the conferences Experimental Finance in Nijmegen, the Netherlands, Experimental Finance in Tucson, Arizona, the International Meeting on Experimental and Behavioral Sciences in Toulouse, France, Conference on Behavioural Aspects of Macroeconomics & Finance at House of Finance, Frankfurt, Germany, Barcelona GSE Summer Forum, Spain, and CESifo in Munich, Germany. The scientific research presented in this publication received financial support from the National Research Fund of Luxembourg (INTER/MOBILITY/12/5685107), and University of Luxembourg provided funding of the experiments through an internal research project (F2R-LSF-PUL-10IDIA).

Forthcoming in The Journal of Finance

In their seminal paper, Modigliani and Miller (1958, 1963) showed mathematically that the market value of the firm is invariant to the firm's leverage; different packaging of contractual claims on the firm's asset returns does not impact the total market value of the firm's debt and equity. The Modigliani and Miller – henceforth MM – value-invariance theorem suggests that the law of one price prevails for assets of the same "risk class". The core of the theorem is an arbitrage proof: If two assets, one leveraged and one unleveraged, represent claims on the same cash flow, any arising market discrepancies are arbitraged away. But due to its assumption of perfect capital markets and the no-limits-to-arbitrage condition (requiring perfect positive correlation of asset returns, no fees on the usage of leverage, etc.), the MM theorem has not been tested in a satisfactory manner on real-world market data. Thus, its empirical significance has been unclear.[1]

Nevertheless, such a test is feasible in the laboratory, and providing an empirical test of the MM theorem is a primary purpose of this laboratory study. Since perfect return-correlation is rare in naturally-occurring equities, we also check how limits to arbitrage affect the empirical validity of the MM theorem with regards to cross-asset pricing. In particular, we address the question of whether a perfect positive correlation between asset returns is necessary for the empirical validity of value invariance or if the same expected (rather than identical) future return is sufficient, as suggested for our setting by the capital asset pricing model. Our data indeed suggest that perfect correlation is necessary for the law of one price to prevail.

[1] The perfect-capital-markets assumption requires, among other things, that no taxes and transaction fees are levied, and that the same interest rate applies for everyone. Lamont and Thaler (2003) present several real-world examples where the law of one price is violated. They argue that these violations result from limits to arbitrage. There was an early objection concerning the applicability of value invariance in relation to the variation of payout policy. Modigliani and Miller (1959) replied to this objection by stating that the dividend policy is irrelevant for the value of the company. However, it is now widely-accepted that dividends impact empirical valuations (for a recent discussion of the *dividend puzzle*, see DeAngelo and DeAngelo 2006). The dividend-irrelevance theorem was thus empirically rejected and is considered as being of theoretical interest only. Still, the value-invariance theorem and its proof remain widely-accepted in the profession even without an empirical test.

Our main design adapts the experimental asset-markets research of Smith, Suchanek and Williams (1988), featuring multi-period cash flows, zero interest rates, and a repetition of markets with experienced subjects.[2] However, in contrast to the standard, single-asset market approach of Smith et al. and in line with MM, we have simultaneous trading taking place in two shares of the same "equivalence class". These *twin shares*, which we call the L-shares and the U-shares, are claims on the same underlying uncertain future cash flows. In one treatment the returns of the L-share and the U-share are perfectly correlated and so any price discrepancies that might arise can be arbitraged away at no risk; in a second treatment, where returns of the L-share and the U-share are uncorrelated, we study the impact of limits to arbitrage. In both cases, the expected stream to shareholders of L-shares and U-shares differs by a constant amount, i.e., the *synthetic* value of debt as we discuss below, so the L-share and the U-share represent accounting "leveraged" and "unleveraged" equity streams.

By comparing the market prices of shares, we thus present a very simple test of the MM theorem.[3] At any point in time when the price deviates from *parity*, in other words if the difference between the L-share and the U-share is not the same as the synthetic debt value, each market participant can exploit the price discrepancy. Since short-selling and borrowing is costless a trader can make a riskless arbitrage gain by going short the expensive share and long the inexpensive share. Exploited pricing discrepancies thereby undo the divergence of market values.

[2] See Palan (2013) for a recent literature survey. The literature is mainly concerned with the measuring of mispricing in the single-asset market. The conclusion is that confusion of subjects and speculation are the main sources of the price deviations from fundamentals in the laboratory (e.g., Smith et al. 1988, Lei, Noussair and Plott 2001, Kirchler, Huber and Stoeckl 2012). Mispricing occurs in the single asset market, and also when assets are simultaneously traded in two markets (Ackert et al. 2009, Chan, Lei and Veseley 2013). Smith and Porter (1995), Noussair and Tucker (2006) and Noussair et al. (2016) report reduced mispricing when a futures market enables subjects to arbitrage price discrepancies of underlying asset and futures contract.

[3] We show in Section 3 how our design maps into the MM theorem.

Our data provide support for the MM theorem since average prices are close to parity, even though some price discrepancies and deviations from the risk-neutral value persist throughout the experiment. In our *perfect-correlation* treatment, we observe that perfect correlation is essential for value indifference as we control for variations in correlation; in our control *no-correlation* treatment, we consider independent draws of dividends of the two simultaneously traded shares. Here, L-shares and U-shares have the same *expected* dividend and idiosyncratic risk as in the perfect-correlation treatment, but an asset swap has risk.

We find a clear treatment effect: We observe a higher level of price discrepancies in the no-correlation treatment. With perfect correlation our measures of *cross-asset price discrepancy*, *relative frequency of discrepant limit orders*, and *deviation from fundamental dividend value* indicate smaller deviations from the theoretical benchmarks than in the no-correlation treatment.[4] The market corrects relative mispricing with perfectly-correlated returns but not as well with independent asset returns.

Hence, although potential price discrepancies never disappear in absolute quantitative terms for the perfect-correlation treatment, our data provide rather strong qualitative support for the equilibrium through the comparison of our treatments. That said, as with evidence observed with experienced subjects in single-asset market studies (e.g., Haruvy, Lahav, and Noussair 2007; Dufwenberg, Lindqvist, and Moore 2005), the price deviation from fundamental dividend values declines in consecutive markets in both treatments. The movement towards the theoretical benchmarks, nonetheless, seems to be more rapid in the perfect-correlation treatment than in the no-correlation treatment, both in decline of price discrepancies and in deviation from

[4] In line with the studies that apply a zero discount rate in the Smith et al. (1988) experimental framework, we define the fundamental dividend value by the sum of discounted expected future dividends.

fundamental dividend value. Nevertheless, some potential price discrepancies persist in both treatments, even with experienced subjects.

We consider the impact of traders' acuity, as measured by the cognitive reflection task (CRT; Frederick, 2005), on the level of price discrepancy; the literature suggests that smart traders search and eliminate price discrepancies.[5] Our measure correlates with the reduction in price discrepancy on the overall sample. While there are some price discrepancies in markets with higher trader aptitude, these are substantially less common and smaller. However, we find no evidence of subjects specializing in arbitrage, so we conclude that existing discrepancies are dissolved by the equilibration dynamics of the market rather than by skilled individual traders. Even so, low trader aptitude leads to pricing discrepancies in the no-correlation treatment.

Overall, we provide evidence regarding how limits to arbitrage (such as our no-correlation treatment) impact value invariance. One main contribution is that we are able to empirically validate value invariance under perfect correlation. A second main contribution is the observation of a treatment effect, since price discrepancies in the market increase substantially when one moves from perfect positive correlation to zero correlation. The deviations appear to be driven by relative overvaluation of the U-share. In the perfect-correlation treatment these are evened out, but in the no-correlation treatment they prevail in markets with overall low trader aptitude. Finally, we observe that high trader acuity significantly reduces the price discrepancy in the market and shares trade closer to fundamental dividend value.

We also conduct additional treatments to attempt to understand the patterns that we observe in the initial perfect-correlation and no-correlation treatments. In one treatment, we

[5] See the discussions in Shleifer (2000) and Lamont and Thaler (2003).

specifically investigate 10-period markets with only a single asset present. We find no systematic mispricing in this treatment. In a final treatment, we have two assets traded in a single period. Here again we do not find significant differences from parity pricing. Earlier experimental results have shown that in simple settings, double-auction market prices easily average around the competitive-equilibrium prediction.[6] Thus, we have proposed a more extreme test of MM in the context of long-lived assets and declining dividend value, which is known for being cognitively-demanding. Thus, we find differential behavior in the perfect-correlation treatment versus the no-correlation treatment.

The remainder of the paper is organized as follows. In Section 2 we explain the experimental design. In section 3 we discuss the MM theorem in light of our design, and in Section 4 we present our measures of price discrepancies and hypotheses. Our experimental results are presented in Section 5. We present robustness tests with data from the additional treatments in section 6 and we conclude the paper in Section 7.

.

1. Literature Review and Experimental Design

1.A. Literature review

Our study contributes to the modest experimental literature that tries to evaluate the market's ability to reduce or eliminate arbitrage opportunities.[7] The observation of persistence in price discrepancies confirms earlier empirical results. O'Brien and Srivastava (1993) replicate

---

[6] In the classic study of Vernon Smith (1962), market equilibrium dynamics were strong in small markets. Gode and Sunder (1993) found strong equilibrium dynamics even in simulations with randomized algorithms ("zero intelligence traders").

[7] See the surveys in Cadsby and Maynes (1998), and Sunder (1995).

portfolios of options, stocks and cash in a multiple-asset experimental market with two stages and information asymmetries. The authors report that if the information asymmetry cannot be resolved, price discrepancies frequently persist.

Oliven and Rietz (2004) investigate the data of the 1992 Iowa presidential election market (IEM), a large-scale experiment conducted for several months on the Internet. Arbitrage opportunities in this market were quite easy to spot; if the value of the market portfolio deviated from the issue price, any trader could make an arbitrage gain by selling or buying at said issue price. Oliven and Rietz report a substantial number of price discrepancies, but find that these were quickly driven out. Rietz (2005) reports on a laboratory prediction-market experiment with state-contingent claims. Similarly to the IEM, arbitrage opportunities were easily spotted, but trading was over 100 minutes rather than 100 days. Rietz concludes that this market is prone to violate the no-arbitrage requirement. However, if (as in one treatment) the experimenter automatically eliminates each price discrepancy, this automatic arbitrageur was involved in most trades in the experiment. Abbink and Rockenbach (2006) report that, even after hours of experience, both students and professional traders left arbitrage opportunities unexploited in an individual investment-allocation task of cash to options, bonds and stock.

To the best of our knowledge, Levati, Qiu, and Mahagaonkar (2012) conduct the only other experimental study to test the MM theorem. Their design forecloses any arbitrage possibility or (homemade) leveraging and unleveraging.[8] Levati et al. examine evaluations for eight independent lotteries with varying degree of risks in a sequence of experimental single-asset call auction markets, where the risks represent different levels of company leverage. In contrast to our perfect-correlation treatment, and similar to our results obtained in the no-correlation

[8] Stiglitz (1969) proves MM value invariance within a general equilibrium approach, without explicit arbitrage assumptions.

treatment, the market data in Levati et al. show no support for value invariance. The authors acknowledge the foreclosure of any arbitrage possibility as a potential reason for this result.

1.B. Experimental design

In our experiment, subjects could buy and sell multi-period-lived assets in continuous double-auction markets. The assets involved claims to a stochastic dividend stream over a lifespan of ten periods, $T = 10$, after which these assets had no further value. The instructions can be found in the Online Appendix.

Trading occurred in two asset classes named *L-shares* and *U-shares*.[9] We follow design 4 from Smith et al. (1988), where the dividend paid on an L-share was independently drawn {0, 8, 28 or 60 cents} with equal probability at the end of every period, so that the expected dividends per period were 24 cents on L-shares. The possible dividends paid on the U-share were 24 cents higher, and so the expected dividends per share were 48 cents on U-shares. The interest rate was zero and thus the discounted sum of expected dividends conventionally referred to as fundamental dividend value of L-shares and U-shares were initially 240 and 480 cents and decreases by 24 and 48 cents per period (see Table 1). After the last dividend payment, all shares were worthless and the final cash balance was recorded.

Our treatment variation between subjects is the correlation (ρ_{LU}) between the dividends on L-shares and U-shares. In the perfect-correlation treatment, ($\rho_{LU} = 1$), the U-share paid exactly 24 cents more in each period than does the L-share. In the no-correlation treatment, ($\rho_{LU} = 0$), the dividend on the U-share was independently drawn {24, 32, 52, 84 cents} with equal probability.

[9] In the experiment we said "*A-share*" and "*B-share*" instead of L-share and U-share.

Table 1. Initial individual endowments and moments

The first column shows the individual initial unit endowments with shares and cash. The second and third columns show first and second moments of dividend value distributions; ‡expected dividend value and total variance are per unit and period, $t \leq T = 10$. The initial total variance is 5360 for each share; the initial expected payoffs of the L-share and the U-share are 240 and 480. Variances, expected dividend values decrease linearly over time.

	initial unit endowment	Expected dividend value/unit‡	Initial total variance/unit‡
L-shares	2	24 $(T - t + 1)$	536 $(T - t + 1)$
U-shares	2	48 $(T - t + 1)$	536 $(T - t + 1)$
Cash units	1200	1	-

Each market participant could electronically submit an unlimited number of bids and asks in the two simultaneous markets. Submitted bids and asks were publicly visible and could not be cancelled. The best outstanding bid and ask were available for immediate sale and purchase through confirmation by the other market participants. Upon a transaction, all bids of the buyer and all offers of the seller were cancelled in both markets and the price was publicly recorded. However, the buyer may also have placed offers to sell and the seller may have placed bids to buy; any such offers and bids remained in the market. So, any arbitrageur who wanted to exploit a price discrepancy could immediately buy low and sell high by pressing the buttons.

Market participants were initially endowed with two L-shares and two U-shares, and 1200 cents cash (see Table 1). Traders were able to borrow up to 2400 cents for the purchase of assets and could short-sell up to four L-shares and up to four U-shares without any margin requirements.[10] The trading flow was unaffected (i.e., there was no message indicating a short

[10] Subjects would not exhaust their borrowing capacity for strategic reasons. Only 0.1 percent of subjects' end-of-period records indicate a cash balance below -2,200. All these records correspond to individual bankruptcies. Bankruptcy rarely occurred and never occurred in the last two rounds of the experiment.

sale rather than a long sale) by short sales and borrowings, which were displayed as negative numbers.[11] Each subject had thus a wide scope for financial decision-making.

In order to reduce confusion and consequential pricing discrepancies, subjects were reminded on screen about the expected future dividends and the sum of expected dividends for the remaining periods. Dividends, prices (open, low, high, closing and average), number of transactions, and portfolio compositions in each past period were recorded in tables. Since we were interested in the effects of experience, an experimental session involved four consecutive *markets*, each designed for nine subjects. The period length in the first market was 180 seconds, and was 90 seconds in the subsequent markets.[12] One of the four markets was chosen (with equal probability) for payment at the end of the session. A subject determined the payoff-decisive market by a die roll. Subjects received their final cash balance in the payoff-decisive market plus the payoff from two pre-market tasks (further detailed below) and a show-up fee of $15 in an envelope at their cabin. In case of a negative cash balance, the subject's show-up fee would be correspondingly reduced, but by not more than $5.

Subjects were recruited from a pool of economics and science students at UCSB via ORSEE (Greiner 2004). Each person drew a number from a tray upon arriving at the laboratory. This number indicated one's cabin number. Before the instructions were read for the trading markets, subjects engaged in two pre-market tasks to reveal to us their relevant personal traits. Information on payoffs from these tasks was only communicated at the end of the session. The first task was an investment game to assess the subjects' degree of risk aversion (Charness and

[11] Subjects would typically not exhaust their short-sale capacities for strategic reasons. Overall, 8% of subjects' end-of-period records indicate negative share-holdings. The short-sale limit of -4 shares was reached in 2% of records of which more than 50% correspond to individual bankruptcies.
[12] We allowed more time in the first market for people to get accustomed to the decisions. There is no evidence (including questionnaire reports) that subjects were short of time in the shorter intervals.

Gneezy, 2010). They chose an amount $0 \leq X \leq \$10$ to allocate to a risky asset that paid with equal probability 0 or $2.5\,X$ and to a safe asset $\$10 - X \geq 0$, to be paid out with certainty. One of the nine participants in the asset market would receive the payoff from the first experiment. The second task was the CRT, where the three questions were asked in a random order.[13] Subjects had 90 seconds to answer the questions and were rewarded with $1 per correct answer.

As part of the instructions, subjects had to successfully complete four practice exercises: a dividend questionnaire, a trading round, a forecasting reward questionnaire,[14] and a second trading round. After the four markets, subjects were debriefed in a questionnaire on personal details. The experiment was computerized with the software ztree (Fischbacher 2007). Written instructions were distributed, and verbal instructions were played from a recording.

After conducting these two initial treatments, we conducted two additional treatments to further investigate patterns found in the first treatments. These consisted of 10-period asset markets with a single asset and one-period asset markets with two assets. The design and results can be found in Section 6.

[13] The questions were: (1) A hat and a suit cost $110. The suit costs $100 more than the hat. How much does the hat cost? (2) If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? (3) In a lake, there is a patch of lily pads. Every day the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of it?

[14] Prior to each period, subjects made predictions about the average prices at which the assets would be transacted during the period. By asking to reflect and predict market outcomes prior to the period we aimed at deeply engaging subjects in the experiment. Similar to the design in Haruvy et al. (2007), subjects initially submitted a forecast of the average price of each asset for each future period. In our design, however, subjects updated only their price forecasts of the current period prior to subsequent market openings. Subjects received salient rewards for accurate forecasts; the mean percentage deviation of the forecast from the realized average price was subtracted from one and the remainder is multiplied by $6. The deviation in any period is capped at 100%. Periods without transactions did not count in the determination of the payoff, in either the numerator or the denominator. Subjects were rewarded either for the accuracy of initial forecasts or for the accuracy of updated forecasts, with equal probability. The decision was made by computerized random draw after the last market period. The reward from forecasting was included in the subject's final cash balance.

2. Modigliani-Miller invariance theorem

In this section we discuss the invariance theorem of Modigliani and Miller (1958), hereafter MM, in light of our experimental design, in particular, the perfect-correlation treatment.[15] We begin by restating the MM invariance theorem (without taxes) and sketching the proof (Modigliani and Miller 1958, p. 268f).

> "**[Invariance theorem]**: Consider any company j and let $\bar{X}_j$ stand for the ... expected profit before deducting interest. Denote by D_j the … value of the debts of the company; by S_j the market value of … common shares; and by $V_j \equiv S_j + D_j$ the market value of all securities.... Then … we must have in equilibrium:
>
> $$V_j \equiv S_j + D_j = \bar{X}_j / r_k, \textit{ for any firm } j \textit{ in class } k.\text{"} \quad (1)$$

The expected cash flows are discounted by the *market-required return* on assets (r_k), which is determined by the equivalence class *k* of the company's assets and the risk-attitude of the market. The MM invariance theorem states that the total *market* value of the firm is invariant to its capital structure. It implies that identical cash flows are priced the same in equilibrium.[16]

To prove the implication of the invariance theorem, MM compare two companies with the same total cash-flow X over the infinite horizon. The first company U is entirely equity financed (so that at the end of the period the equity holders get the entire return X). The other company L is leveraged, with debt of face value D, so that at the end of the period, debt-holders

[15] Comments from Bruno Biais, Peter Bossaerts and an associate editor helped us to significantly improve this section.

[16] Since in the MM world without taxes and riskless debt the assets are the same for the unleveraged (U) and the leveraged (L) firm, the required expected total return on assets must be the same independently of the company's debt ratio, (see MM 1958 equation (4), p. 268); $\frac{\bar{X}_j}{V_U} = \frac{\bar{X}_j}{S_U} = \frac{\bar{X}_j}{S_L + D_L} = \frac{\bar{X}_j}{V_L} = r_k$.

get rD, and equity holders get $X - rD$. MM then show by arbitrage that one must have: $V_U = S_U = S_L + D = V_L$ (where S_U and S_L are the *market* values of equity of the two companies, and V_U and V_L are the total values of the unleveraged and the leveraged companies). Starting on page 269, MM analyze the arbitrage trade when the total market value of company L is larger than that of company U, i.e. $V_L = S_L + D > V_U$. To take advantage of this situation, the arbitrageur sells shares in the leveraged company, borrows, and buys shares in the unleveraged company. So he is long a portfolio whose value is that of the unleveraged company minus debt service, and he is short the equity of the leveraged company. The former is a synthetic version of the latter. Since cash flows on both portfolios are the same the investor is not exposed to risk. But, at the inception of the position, the portfolio purchased is cheaper than the portfolio sold. Hence, there is an arbitrage profit. Since arbitrage cannot exist in equilibrium, the proof concludes.

How does our experimental setting map into this? We consider two stocks L and U. The dividend on the U-share is 24, 32, 52 or 84 with equal probability. The dividend on the L-share is 24 lower than that of the U-share (in each state). To map this into MM, assume the two companies hold the same real asset. The payoff generated by that real asset is 24, 32, 52 or 84 with equal probability. The U-share can be thought of as the equity share of the unleveraged firm, getting the entire payoff. And the L-share can be thought of as the equity share in the leveraged company, holding the same asset, but having issued debt with value 24 cents (D in MM), so that its shares get the cash flow minus 24. The MM arbitrage argument states that the market value of the unleveraged company (S_U, the price of the U-share) must be equal to the value of the leveraged company, which is the market value of equity (S_L, the price of the L-share) plus the value of the debt (D).

$$V_U = S_U = S_L + D = V_L \tag{2}$$

An arbitrage opportunity similar to that analyzed on page 269 of MM is one in which, infringing on equation (2), the total market value of the leveraged firm is larger than that of the unleveraged firm, as $S_L + D > S_U$. The immediate, *riskless* gain from going long the unleveraged U-share and short the leveraged L-share equals the difference between the two, $(S_L + D - S_U > 0)$. This arbitrage gain results due to the fact that there are no margin or repurchase requirements in our experiment, all shares are cancelled at maturity T, and debt (D) is a synthetic debt stream without real impact.[17] In equilibrium arbitrage cannot exist. The prices of U-share and L-share must therefore differ exactly by the value of debt.

Modigliani and Miller (1958) discuss expected future streams and suggest one discount rate for same risks, but the proof of the theorem requires *identical* future streams. One key research question that we raise in our experimental control treatment (with zero correlation) is whether empirical validation of the invariance theorem actually requires identical asset returns.[18] Our research question is two-fold:

i) Are the prices of two portfolios equal when they have perfectly-correlated payoffs (modulo a shift) and short sales and borrowing are unconstrained?

ii) Are the prices of two portfolios equal when their payoffs are uncorrelated but with the same distribution (modulo a shift) and short sales and borrowing are unconstrained?

[17] Debt is a pure accounting stream not noted by experimental subjects. Trading costs and interest rates are zero and the shares need not be repurchased but are simply cash at the end of the market sequence. The accounting exercise is easy because cash-financing and debt-financing have the same consequences in the experiment.

[18] MM (1958, p. 266) assume identical risk classes "such that the return on the shares … in any given class is proportional to (and hence perfectly correlated with) the return on the shares ... in the same class..." These conditions are satisfied in our perfect-correlation treatment. Conditions are similar in our no-correlation treatment, since expected returns of assets are the same as in the perfect-correlation treatment. However, returns are independent in the no-correlation treatment so that, in the strict sense of the definition, L-shares and U-shares are not in the identical risk class. Our experimental test checks if the strict implementation of the definition is critical for achieving empirical support for value invariance. In the laboratory it is not so clear if notable differences can be detected. On one hand, earlier research has suggested that human behavior may disregard correlations between asset returns (e.g, Kroll, Levy and Rapoport 1988). On the other hand, our setting favors pricing at expected dividend value, since subjects are informed about the fundamental dividend values in the instructions thereby possibly reducing confusion of subjects and mispricing of assets (e.g., Kirchler et al. 2012).

Question i) is the direct test of the MM (1958) theorem. For question ii), the relevant theoretical result is that (in perfect markets) two portfolios with the same payoffs should have the same equilibrium price (and this holds irrespective of utility functions). This argument can be motivated through the capital asset-pricing model as described in the experimental finance literature (Asparouhova, Bossaerts, Plott 2003; Bossaerts, Plott, Zame 2007).

$$\begin{bmatrix} S_{Ut} \\ S_{Lt} \end{bmatrix}^* = \begin{bmatrix} F_{Ut} \\ F_{Lt} \end{bmatrix} - K \begin{bmatrix} \sigma_{Ut}^2 & \rho_{LU}\sigma_{Lt}\sigma_{Ut} \\ \rho_{LU}\sigma_{Lt}\sigma_{Ut} & \sigma_{Lt}^2 \end{bmatrix} \begin{bmatrix} \bar{Z}_U \\ \bar{Z}_L \end{bmatrix} \tag{3}$$

This formula, here adapted to two risky assets, takes account of market supply and demand. The equilibrium price is determined by the mean holdings of the market portfolio $\bar{Z}$ of risky assets (as indicated in Table 1), the expected dividend payoffs of risky assets (F_{jt}, $j = U, L$), the (harmonic mean) risk aversion of the market *(K),* and the covariance matrix. With equal supplies, (i.e., $\bar{Z}_U = \bar{Z}_L$), equal payoff variances of L-shares and U-shares, (i.e., $\sigma_L^2 = \sigma_U^2$), and zero correlation, ($\rho_{LU} = 0$), the equilibrium-price difference equals the difference of the expected dividend payoffs, (i.e., $S_{Ut}^* - S_{Lt}^* = F_{Ut} - F_{Lt}$, $t = 1,..,T$).[19] Hence, for both treatments theoretical arguments are available to expect parity pricing in the market. We consider a perfect (no-fee) capital-market setting and ask whether actual human beings conform to the predictions of theory.

Riskless exploitation of price discrepancies is impossible where payoffs are only identically distributed rather than perfectly correlated. So in this case smart traders may require a risk premium to keep relative prices in balance, thereby allowing for a larger deviation from

[19] Note in equation (2), the same is true for perfect correlation, ($D = F_U - F_L$). The formula generally holds if the agent buys for keeps (not speculatively), but it does not generally extend to other utility functions (unless payoffs are Gaussian); variance is not the right measure of risk in general. However, the CAPM formula provides a valid first-order approximation to prices if total risk is small (see the proof in Judd and Guu 2001).

parity pricing.[20] Well-documented evidence from real-world markets shows that relative mispricing of twin shares can occur over an extended horizon when there are limits to arbitrage (e.g., Froot and Dabora 1999). Shleifer (2000) suggested that in pricing of twin shares "noise trader risk" can result in limits to arbitrage. The risk exists that discrepancies may widen instead of narrow when traders follow price trends that move away from equilibrium.

3. Measures and hypotheses

3.A. Measures

We now formulate our measures and testable hypotheses. Let, as above, F_{Ut} and F_{Lt}denote the fundamental dividend values, and let S_{Ut} and S_{Lt} denote the share prices of the U-share and the L-share in period t, respectively. We measure the relative *difference from parity* pricing of the U-share and the L-share as follows.

$$\Delta_t = \frac{S_{Ut}}{S_{Lt} + D_t} - 1 = \frac{(S_{Ut} - S_{Lt}) - (F_{Ut} - F_{Lt})}{S_{Lt} + (F_{Ut} - F_{Lt})} \tag{4}$$

The first ratio in (4) relates the value of the "unleveraged" company U to the value of the "leveraged" company L, where the difference between fundamental dividend values in the second denominator stands for the synthetic value of debt ($D_t = F_{Ut} - F_{Lt}$). The right-hand side in (4) is obtained by replacing the synthetic debt value. The invariance theorem requires pricing at parity, $\Delta_t = 0$ (see Hypothesis 1). Thus, market values shall differ by as much as but not more than fundamentals. In our data analyses, market value is the average market price of the period.

[20] We note that markets are incomplete in our no-correlation treatment, since a self-financing replicating portfolio cannot be formed. Therefore, in incomplete markets (riskless) arbitrage is generally impossible. However, if market prices confirmed fundamentals or if the deviations from fundamentals were the same in both of our markets, no cross-asset price discrepancy would arise and the law of one price would still obtain.

We define the measure *cross-asset price discrepancy* (*PD*) as follows, where $T = 10$ is the number of market periods.

$$PD = \frac{1}{T}\sum_{t} |\Delta_t| \tag{5}$$

The *PD*-value is the average absolute percentage deviation from parity pricing during the course of a market. Price discrepancy indicates potential gains by selling high and buying low based on average prices. The value is usually positive even if the average difference from parity pricing is zero. The *PD*-value is zero if L-share and U-share prices are equal to fundamental dividend values, or if prices deviate by the same amount from the fundamental dividend values.

Even with zero *PD*-value, prices can deviate from fundamental value. To measure price deviations from fundamentals we define the *relative absolute deviation from fundamentals* **(***DF)*. The *DF*-value here measures the *expected excess return* of buying and selling at prices off the known fundamental dividend value (j = 1, 2 indicates L-shares and U-shares, J = 2).

$$DF_{jt} = \left|\frac{S_{jt}}{F_{jt}} - 1\right|$$

$$DF = \frac{1}{JT}\sum_{j=1}^{J}\sum_{t=1}^{T} DF_{jt} \tag{6}$$

The *DF*-value represents expected gains by purchasing one share if the price is below or selling one share if the price is above the fundamental dividend value. It compares the price to the predicted value under the equilibrium hypothesis, and the *PD* compares the price to the predicted value under the no-discrepancy hypothesis. By comparing the two, we can address the question if mispricing is more severe with regards to dividend value or with regards to the other asset. Note that *DF* and *PD* can be compared, allowing us to assess if the dividend value or the arbitrage opportunity guides the equilibration dynamics in our experiment. Similar measures for mispricing have been proposed in the literature for the single-asset market (e.g, Stöckl, Huber and Kirchler 2010). In particular, the relative deviation *RD* of asset $j = L, U,$ where F_j is the average cumulative dividend value, has been used to measure magnitudes of bubbles in experiments.

$$Relative\ Deviation = RD_j = \frac{1}{TF_j}\sum_{t=1}^{T} S_{jt} - F_{jt} \tag{7}$$

Potential discrepancies can arise in real-time in submitted orders. If the adjusted bid exceeds the best outstanding offer of the twin share, an arbitrageur can sell high and buy low, thereby realize a (expected) gain. Under perfect correlation, the gain from eliminating a price discrepancy is riskless (arbitrage); under no-correlation the expected gain is risky, since differences in future dividends can induce losses or gains. We call *discrepant* each limit order that (upon submission) leads to a cross-asset discrepancy in outstanding orders, i.e., opens a potential expected gain for another trader.[21] We measure the *relative frequency of discrepant limit order submission*, $RFDLO_i$, for each subject i and aggregate as follows; where $\#LO_i$ denotes the number of limit orders submitted by the subject, and $\#DLO_i$ is the number of discrepant limit orders.

$$\begin{aligned} RFDLO_i &= \frac{\#DLO_i}{\#LO_i} \\ RFDLO &= \frac{\sum_i \#DLO_i}{\sum_i \#LO_i} \end{aligned} \tag{8}$$

[21] In a different trading environment, Biais and Pouget (1999, p. 15) refer to similar trades as "a noise trading [cf. Shleifer and Summers 1990] component in the order flow … as they [tend] ... to lose money."

We measure a *trader's aptitude* is measured by the reverse of the frequency of submitting a discrepant limit order, (1 - $RFDLO_i$). The trader's aptitude is therefore high if $RFDLO_i$ is small and vice versa.

Our empirical measure of acuity is the individual's CRT score, i.e., the number of correct answers in the pre-market cognitive reflection task (Frederick 2005). Recent results in single-asset market experiments have shown systematic effects of CRT scores (Corgnet et al. 2015, Akiyama, Hanaki, and Ishikawa 2013; Breaban and Noussair 2014, Noussair et al. 2016). Below, we also measure the effect of gender and risk aversion (as elicited in the investment game) on our mispricing measures (4) – (8). Recent research from experimental single-asset markets suggests that price levels may be lower if the level of risk aversion increases (Breaban and Noussair 2014) and also if the share of female traders increases (Eckel and Füllbrunn 2015).

3.B. Testable Hypotheses

With measures (4) – (8) at hand we next formulate our testable hypotheses. Our most efficient theoretical benchmark (which is standard in the experimental studies that apply the Smith et al. 1988 design, but is unlikely to obtain with inexperienced subjects in the laboratory) requires prices to be equal to risk-neutral fundamental dividend values.

Hypothesis 0 (Risk-neutral pricing): There will be no deviations of prices from fundamentals in either share class, i.e., $DF = 0$.

In line with the literature, we also look at a weak form of Hypothesis 0, that is, we investigate if the *RD* measure is equal to zero on average. Risk-neutral pricing would require that the price equals the fundamental dividend value in each period. In fact, risk-neutral pricing is a sufficient (but not necessary) condition for obtaining MM invariance (Modigliani and Miller

1958). Indeed, previous experimental evidence (Palan 2013) shows that pricing deviations from fundamental dividend value can be expected to occur in experimental asset markets in both directions from above and below. This evidence does not invalidate the MM law of arbitrage-free pricing. For arbitrage-free pricing and the MM theorem we require that our L-share and U-share are priced at parity, which is indeed a much weaker requirement than risk-neutral equilibrium pricing.

Hypothesis 1A (MM invariance theorem): The adjusted market values of L-shares and U-shares will be the same, i.e., $\Delta_t = 0$. **1B** (No cross-asset price discrepancy): $PD = 0$. **1C** (No discrepant orders): $RFDLO = 0$.

Hypothesis 1A requires that an L-share and a U-share (adjusted for fundamentals) are priced the same on average. Hypothesis 1B (which implies 1A) requires that eventual deviations from fundamentals occur simultaneously and equally in both shares. Hence, we investigate three levels of market efficiency; the first is based on the absence of differences from parity pricing on average (H_{1A}), the second is based on the absence of price discrepancies (H_{1B}) and discrepant orders (H_{1C}), and the third requires that prices coincide with risk-neutral dividend values (H_0).

These levels of market efficiency are tested in two treatment dimensions. First, we consider perfect correlation where elimination of pricing discrepancies is riskless, since dividend streams differ by a constant and can be arbitraged. Second, we consider the case of no correlation, where elimination of pricing discrepancies is risky because dividends are independent of each other. Since the L-share and the U-share have identical idiosyncratic risks in both treatments, and based on earlier evidence that suggests insensitivity of behavior to changes in return correlations (e.g., Kroll et al. 1988), one might expect no treatment effect in the laboratory. However, the MM arbitrage result concerns only the relative pricing of two assets, not the absolute level of their prices. Thus, it could be that both assets are mispriced in absolute terms but their relative pricing is aligned, so there is no arbitrage opportunity. Given that mispricing has been frequently observed in the considered experimental design, we expected to observe some mispricing in our experiments and state the following testable hypothesis:

Hypothesis 2. (Hedging effect): The cross-asset price discrepancy (*PD*) will be larger in the no-correlation treatment than in the perfect-correlation treatment.

The hypothesis suggests that markets can eliminate some price discrepancies in the absence of risk even without a dedicated (automatic) arbitrageur, and so decrease the magnitude of the *PD*-value in the presence of perfect correlation. Hypothesis 2 suggests the relative validity of MM invariance under perfect correlation, relative to no correlation.

Anticipating from experimental evidence of mispricing in early markets (e.g., Smith et al. 1988, Dufwenberg et al. 2005), we expect a decrease of mispricing in our consecutive markets.

Hypothesis 3. (Experience effect): The cross-asset price discrepancy (*PD*), the deviation from fundamental dividend value (*DF*), and the relative frequency of discrepant orders (*RFDLO*) will decrease over time (in consecutive markets).

It is an oft-cited conjecture that smart investors eliminate mispricing in the market (see e.g., Shleifer 2000). Although our design has no dedicated arbitrageur, we can nevertheless address the predictive power of this argument in our data. We compare the magnitude of price discrepancies vis-à-vis characteristics of market participants, suggesting a relationship between acuity and aptitude of traders.

Hypothesis 4. (Smart trader effect) The cross-asset price discrepancy (*PD*), deviation from fundamental dividend value (*DF*), and the relative frequency of discrepant orders (*RFDLO*) will decrease with the market participants' degree of measured acuity.

We are also interested in the equilibration dynamics of the market. When a price discrepancy arises, that is, when the L-share and the U-share are mispriced relative to one another, at least one share (if not both) are mispriced vis-à-vis the fundamental dividend value. Exploitation of this mispricing leads to expected gains. We anticipate that the more profitable trade will happen first in both treatments. The more profitable trade is the one whose expected return is the larger one within the discrepancy pair. It can be either a long or short position. Moreover, we also expect, especially in the perfect-correlation treatment (but also in the no-correlation treatment), that subjects exploit price discrepancies by simultaneously taking long and short positions, thus eliminating the discrepancy and booking an arbitrage (expected) gain. The latter behavior is what MM suggest in their arbitrage proof.

Hypothesis 5A (Equilibration dynamics) Where cross-asset price discrepancy arises the *higher expected return twin share* trades first. **5B** (Asset swap/MM arbitrage) The price discrepancy is exploited through (almost) simultaneous trade in the twin shares by the same subject.

Finally, we investigate the effect of return correlation on individual portfolio diversification. Portfolio diversification reduces fluctuations in portfolio returns when correlations are smaller. Financial economics suggests that people are averse to return volatility. Therefore, we conjecture that the difference between the number of U-shares and L-shares is smaller in the no-correlation treatment than in the perfect correlation treatment.[22] Let Z_{Uit} and Z_{Lit} be the number of U-shares and L-shares in the portfolio of subject *i* at the end of period *t*, and $dZ_{it} = |Z_{Uit} - Z_{Lit}|$ is the absolute difference of the two numbers. We compare the average

[22] Our measure of diversification is very simple. We look at individual deviations from the market portfolio. Investor subjects are perfectly diversified in our design if they hold an equal number of L-shares and U-shares. Different from the no-correlation treatment, return volatility does not decrease with increased diversification in the perfect-correlation treatment.

individual portfolio diversification over subjects and periods, which we denote by $dZ(\rho_{LU})$, across markets and treatments.

Hypothesis 6 (individual portfolio diversification) Subjects are more diversified in the no-correlation treatment (0) than in the perfect correlation treatment (1), $dZ(0) \leq dZ(1)$.

4. Results

In total, 174 students participated, earning an average of $50 in 3 hours. Each participated in only one session. We have 12 sessions in the perfect-correlation treatment and eight sessions in the no-correlation treatment.[23] Each session is comprised of four consecutive markets.[24]

4.A. MM invariance theorem

Observation 1. *L-share and U-share prices are close to parity and in line with the MM invariance theorem in the perfect-correlation treatment. U-share prices are above parity in the no-correlation treatment.*

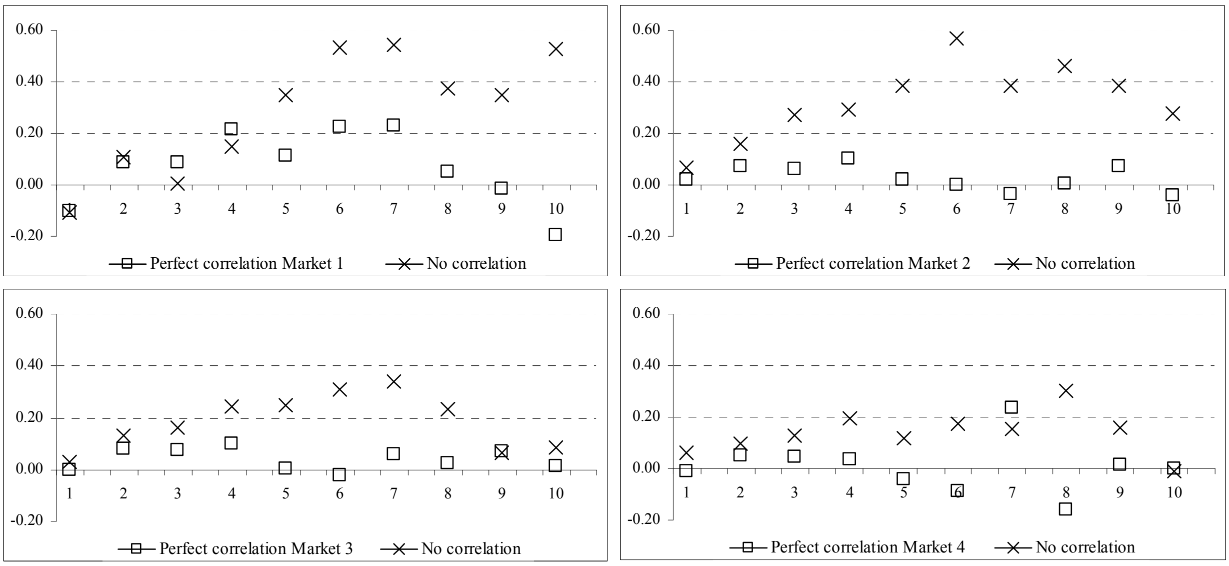
Support. In Figure 1, we see the difference of L-share and U-share prices relative to parity for each market, period, and treatment, aggregated separately over the 12 sessions of the perfect-correlation treatment and the eight sessions of the no-correlation treatment. In the case of a

[23] We do not have the same number of sessions across treatments. Unfortunately, we discovered a problem in the software after conducting the first four sessions. Owing to this problem, the L-share holders received no cash dividend; instead, both L-share and U-share dividends were distributed to U-share holders. Due to logistical and timing problems, we were unable to book the lab to conduct these four sessions and so have only eight cohorts in the no-correlation treatment rather the planned 12 cohorts.

[24] In the fourth market of one session of the perfect-correlation treatment, one subject submitted an obviously erroneous bid (instead of an asking price) for $20 on the L-share worth $0.24 that led to a transaction. We have eliminated the period from the data as the transaction impacts our average estimates. In the second market of the two other perfect-correlation sessions, the data for period 10 are missing (due to a server crash). One session of the perfect-correlation treatment and two sessions of the no-correlation treatments had only seven participants.

missing price of L-share or U-share, the period in the session is treated as a missing observation.[25] The prices in the perfect-correlation treatment are close to parity, whereas the prices in the no-correlation treatment are above parity. In view of our theoretical discussion, we note that no direction of deviation could be predicted. In contrast, the data suggest that the deviation of relative market valuation has a direction, indicating a relatively higher price for the U-share than the L-share.[26] Below we further investigate the pattern.

Figure 1. Evolution of relative difference from price-parity of L-shares and U-shares Δ_t
The abscissa shows the periods 1 to 10 and represents price parity between L-share and U-share prices. The ordinate indicates the relative difference from parity from -0.2 to 0.6. Each chart represents the relative difference from price-parity for one market of 10 periods; markets 1-4 are represented top left, top right, bottom left, and bottom right.



[25] On average we have 11% and 5% missing observations in the perfect-correlation and no-correlation treatments, respectively.
[26] In this context the dividend puzzle comes to mind (DeAngelo and DeAngelo 2006), that is, the empirical observation that the market value of dividend-paying stock is higher than of zero dividend stock. Whereas the U-share always pays a positive dividend, the L-share indeed pays a zero dividend 25% of the time.

Table 2 reports the average differences from parity for each market in both treatments jointly with the *Z*-statistics of the two-tailed Wilcoxon one-sample test that indicates significant deviations from zero. The recorded average results for the perfect-correlation treatment are not significantly different from parity in any market, $\Delta_t = 0$; the overall average price difference from parity per period is 3.67%. By period, we find that only three of 40 (= 4 markets x 10 periods) differences from parity in the perfect-correlation treatment are significant at the five percent level; the pricier share is once the L-share and twice the U-share.[27] In the no-correlation treatment, parity is rejected for each market since the U-share is always pricier than the L-share (see Table 2); the overall average price difference from parity per period is 23.32 percent in the no-correlation treatment, but only 3.67 percent in the perfect-correlation treatment.[28]

So, the differences from price parity are much smaller in the perfect-correlation treatment than in the no-correlation treatment; these differences are four to fifteen times larger in the no-correlation treatment in the four markets. Overall, the average difference from parity per period across the treatments is highly-significant, with the *p*-value of the two-tailed Mann-Whitney test being 0.004. We conclude that our data are in line with the MM theorem (and thus with Hypothesis 1A) in the perfect-correlation treatment, but reject Hypothesis 1A for the no-correlation treatment.

The observation indicates that riskless exploitation of discrepancies, as in the perfect-correlation treatment, is required to approximate value invariance. In the no-correlation treatment the discrepancy thus seems to have two complementary potential sources. One, the exploitation of discrepancies is risky, since the future dividend streams do not allow riskless

[27] The L-share is significantly pricier than the U-share in the first period of the first market ($p = 0.041$), and the U-share is pricier in period 10 of the first market ($p = 0.041$) and in period 7 of the third market ($p = 0.039$).
[28] Since outliers could be affecting the averages, we redid the analyses with median prices instead of simple average prices. The results are very similar and are shown in Tables A1, A2A, and A2B in the Online Appendix.

arbitrage. The risky exploitation of a discrepancy requires a risk premium, implying larger discrepancies. Second, the market discounts the U-share less than the L-share. In principle, this could indicate the existence of a certainty effect in the data (Kahneman and Tversky 1979) concerning non-zero payoffs: while the U-share dividend is strictly positive by definition, the L-share dividend can be zero in every period. However, our follow-up treatments (see below) do not find supporting evidence, and so the reason for this behavior is not completely clear to us.

Table 2. Average difference from parity Δ in percent

*The table records the difference from parity (4) averaged over ten periods and all sessions for each market and both treatment. The first column records the averages for the perfect correlation treatment and the second column for the no-correlation treatment. The bottom line shows the average difference from parity over all periods. Asterisks indicate significant differences from parity measured by the two tailed, one-sample Wilcoxon signed-ranks test. The last column shows the p-value resulting from the two tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; ***p < 0.01, **p < 0.05, *p < 0.10.*

Treatment	Perfect correlation ($n = 12$)	No correlation ($n = 8$)	Mann-Whitney test (between treatments), p-value, [Z-statistic]
Market 1	6.98 [0.39]	27.72** [2.24]	0.031** [2.16]
Market 2	1.72 [0.04]	29.92** [2.52]	0.001*** [3.28]
Market 3	3.49 [1.49]	17.94** [2.10]	0.165 [1.39]
Market 4	2.62 [1.26]	14.49** [2.52]	0.064* [1.85]
Average	3.67 [1.53]	22.52** [2.52]	0.006*** [2.78]

4.B. Pricing discrepancies and deviation from fundamentals

Observation 2. *Absolute difference from parity (PD), the deviation from fundamentals (DF) and the relative frequency of discrepant orders (RFDLO) are reduced in the perfect-correlation treatment relative to the no-correlation treatment.*

Support. The data in Table 3A, Table 3B and Table 3C record the averages of *PD*-value, *DF*-value and RFDLO by treatment, respectively.[29] From Table 3A we see that by buying the lower-priced share and selling the higher-priced twin share (both at the average price in a randomly-determined period), the average riskless return would be 23 percent of the trading value in the first market of the perfect-correlation treatment. This number is relatively large if compared to the second market, where the average *PD*-value drops to one half of the first market.[30] However, the amount of the decrease is rather small relative to the first market in the no-correlation treatment. As indicated by the one-tailed Mann-Whitney test recorded in the table, the *PD*-value is larger overall (at the 10% significance level) than in the no-correlation treatment.

We see in Table 3B that the results for the *DF*-value point in the same direction. The overall average absolute deviation from the fundamental dividend value is 19.13 percent in the perfect-correlation treatment and 27.39 percent in the no-correlation treatment. Nevertheless, the differences between the two treatments are only significant (at the five percent level) for the second market. The results suggest that pricing relative to fundamentals is (relatively) independent of whether there are perfect arbitrage conditions. In line with the literature, both treatments trend towards the fundamental dividend value with repetition. We conclude that the data rather support than reject Hypothesis 2 and Observation 2 vis-à-vis the *PD* and *DF* values, particularly for Market 2, although the evidence is not statistically-significant for all markets.

Table 3C records the average number of discrepant orders relative to the submitted orders in a market. The overall average number of discrepant limit orders is 5.70 percent in the perfect-

[29] These values have been computed according to equations (5), (6) and (8). A missing average price of L-share or U-share in a certain period is treated as a missing observation in the corresponding session. The minimum *PD*-value overall markets and the first quartile in the perfect-correlation treatment is 1.1 percent and 8.4 percent, and in the no-correlation treatment 5.0 percent and 13.7 percent, respectively.

[30] The decrease from the first to the second market is significant at the one percent level ($Z = 2.353$, $p = 0.009$, one-tailed Mann-Whitney test, $n = 12$).

correlation treatment and 10.22 percent in the no-correlation treatment. The differences are significant across treatments.

In terms of absolute numbers of arbitrage and mispricing, Hypothesis 0 and Hypothesis 1B and 1C must be rejected given that no session and market involves a zero *DF*-value, a zero *PD*-value, or zero *RFDLO*-value.

Table 3A: Average absolute deviation from parity *PD* in percent

The table records the price discrepancy (5) for the ten market periods averaged over all sessions for each market and treatment. The bottom lines show the average price discrepancy over all periods, and the one-tailed Page trend test that checks for a significant decline in repeated markets. The last column shows the p-value resulting from the two tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. ^One-tailed tests.

	Perfect correlation ($n_1 = 12$)	No correlation ($n_2 = 8$)	Mann-Whitney test (between treatments), *p*-value, [Z-statistic]^
Market 1	24.59	39.52	0.316 [1.00]
Market 2	15.25	30.36	0.045^{**} [2.01]
Market 3	15.87	22.34	0.32 [1.00]
Market 4	21.65	17.35	0.758 [-0.31]
Total	19.34	27.39	0.563 [0.58]
p-value (one-tailed Page trend test)^	0.159 [-1.00]	0.011^{**} [-2.30]	

Table 3B: Average absolute deviation from fundamentals *DF* in percent

The table records the deviation from fundamentals (6) for the ten market periods averaged over all sessions for each market and treatment. The bottom lines show the average deviation from fundamentals over all periods, and the one-tailed Page trend test that checks for a significant decline in repeated markets. The last column shows the p-value resulting from the two tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. ^One-tailed tests.

	Perfect correlation ($n_1 = 12$)	No correlation ($n_2 = 8$)	Mann-Whitney test (between treatments), p-value [Z-statistic]
Market 1	24.59	39.52	0.316 [1.00]
Market 2	14.42	30.36	0.030** [2.16]
Market 3	15.87	22.34	0.316 [1.00]
Market 4	21.65	17.35	0.622 [-0.31]
Total	19.13	27.39	0.143 [1.47]
p-value (one-tailed Page trend test)^	.159 [-1.00]	.001*** [-3.30]	

Observation 3. *The consecutive-markets analysis suggests reduced price discrepancies with experienced subjects, which occurs earlier in the perfect-correlation treatment than in the no-correlation treatment. We observe no change in the relative frequency of discrepant limit orders.*

Support. Eyeballing the data in Tables 3A and 3B (see also Tables A2A and A2B in the Online Appendix, which is also appended to this working paper), our measures indicate a decrease of price discrepancies in the first three consecutive markets, but not always from the third to the fourth market. The non-parametric Page trend test ($n = 12$, $k = 4$) as reported in the bottom line of Table 3A suggests no significant decrease of the *PD*-value over four markets in the perfect-correlation treatment;[31] on the other hand, the test results reported in Table 3B do indicate a decrease for the deviation from fundamentals that is significant at the 10 percent level. The results of the one-tailed Page test are significant at the five percent level for the no-correlation treatment, indicating a reduction over time of the *PD*-value and *DF*-value.

Table 3C: Average rel. freq. of discrepant limit orders RFDLO in percent

The table records the frequency of discrepant limit orders relative to submitted limit orders (8) by session for each market and treatment. The bottom lines show the average relative frequency of discrepant limit orders over all periods, and the one-tailed Page trend test that checks for a significant decline in repeated markets. The last column shows the p-value resulting from the two tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. *^One-tailed tests.*

	Perfect correlation ($n_1 = 12$)	No correlation ($n_2 = 8$)	Mann-Whitney test (between treatments), *p*-value, [Z-statistic]
Market 1	6.76	10.90	0.190 [1.31]
Market 2	4.96	10.58	0.123 [1.54]
Market 3	4.81	9.61	0.105 [1.62]
Market 4	5.51	9.81	0.037** [2.08]
Total	5.51	10.23	0.045** [2.01]
p-value (one-tailed Page trend test)^	0.242 [-.70]	0.460 [-.10]	

We have no significant evidence for the continuous convergence to the theoretical predictions over all the four markets in the perfect-correlation treatment, since convergence

[31] In the fourth market in some sessions, subjects push prices to irrationally high levels in late periods. Even if these data points were ignored, however, the price discrepancies would not be reduced from the level of Market 3. That said, the reported *p*-value of the Page test suggests that a declining *PD*-value in the perfect-correlation treatment would then be significant at the five percent level.

seems to occur all at once from the first to second market, after which deviations remain at the same level until the end. Thus, the perfect-correlation treatment exhibits a significant decline at the five percent level in *PD*-value and *DF*-value between the first and the second markets, but not between any other consecutive markets.[32] On the other hand, the main decline in the no-correlation treatment occurs from Market 2 to Market 3.[33] Thus, the data suggest a more rapid adjustment to the theoretical benchmarks in the perfect-correlation treatment, i.e., when an exploitation of price discrepancies is riskless.

Table 3C suggests no decline of the *RFDLO* by the one-tailed Page test.

Observation 4. *The differences between PD and DF suggest that pricing discrepancies are a more focal driver of behavior than the deviation from fundamentals in the perfect-correlation treatment when compared to the no-correlation treatment.*

Support. Comparing Tables 3A and 3B, the *PD*-values are smaller than the *DF*-values for each market of the perfect-correlation treatment and larger for each market of the no-correlation treatment.[34] The average difference between *PD*-values and *DF*-values over all markets is -0.029 in the perfect-correlation treatment and 0.017 in the no-correlation treatment. The difference between treatments is significant; per the one-tailed Mann Whitney two-sample test, the p-value is 0.071. Hence, the data suggest that the pricing vis-à-vis the other asset is focal in the perfect-

[32] The p-values of the one-tailed Wilcoxon signed ranks test that suggest a decline between first and second market are as follows; *PD*-value ($p = 0.010$, $Z = 2.316$), and *DF*-value ($p = 0.009$, $Z = 2.353$).
[33] The *PD*-value ($p = 0.034$, $Z = 1.820$) and the *DF*-value ($p = 0.034$, $Z = 1.820$) decrease significantly at the five percent level between Market 2 and Market 3 only, but not between Market 1 and Market 2 ($p = 0.116$, $Z = 1.193$), ($p = 0.104$, $Z = 1.260$).
[34] On average over all markets, the difference between *PD*-value and *DF*-value is significantly different from zero in the perfect correlation treatment. Per the two-tailed Wilcoxon signed ranks test, the p-value is 0.0995. In the no-correlation treatment we have no significant differences between *PD*-value and *DF*-value.

correlation treatment compared with the no-correlation treatment. The results reported in section 5.5, where we examine at the equilibrating dynamics, underline this effect.

4.C. Bubble magnitude

Following the standard line of experimental asset-market research, we report the standard measure of bubble magnitude *RD* (relative deviation) for our treatments by asset. Given that the literature on the single-asset market has shown that mispricing of assets frequently occurs (Palan 2013), we have a surprising result:

> ***Observation 5.*** a) *Asset prices average close to the dividend value in the perfect-correlation treatment. The RD measures of the L-share and the U-share are near zero. b) The price of the U-share in the no-correlation treatment exceeds the dividend value, i.e., the RD measure is significantly positive, while the average price of the L-share is close to the expected dividend value.*

Support. Figure 2 and Figure 3 show the average price trajectories for all sessions and overall. Table 4 records for each market (by treatment) the standard measure of mispricing the relative deviation *RD* measure for the L-share and the U-share (and their differences) as described in section 4 and as commonly applied in the literature (Stöckl, Huber and Kirchler 2010). The figure illustrates observations 5a) and 5b). The overall average RD measures are -0.041 and 0.171 for the L-share and the U-share in the no-correlation treatment and -0.025 and 0.023 for the perfect-correlation treatment. The deviations from expected cumulative dividend value are statistically significant for the U-share in the no-correlation treatment, where the *RD* measure is significantly different from (i.e., larger than) zero for each market and overall. In the perfect-

Figure 2. Average prices in markets 1-4 (left to right, top to bottom) of the Perfect Correlation treatment

The trajectories of all 12 cohorts are displayed for both assets L-share and U-share, and the average for each asset is indicated in bold. The dividend values are displayed by cascading short horizontal lines, but here they are almost covered by the lines that indicate the overall averages. The ordinate indicates the average prices from 0 to 700, and the abscissa the periods 1 to 10.

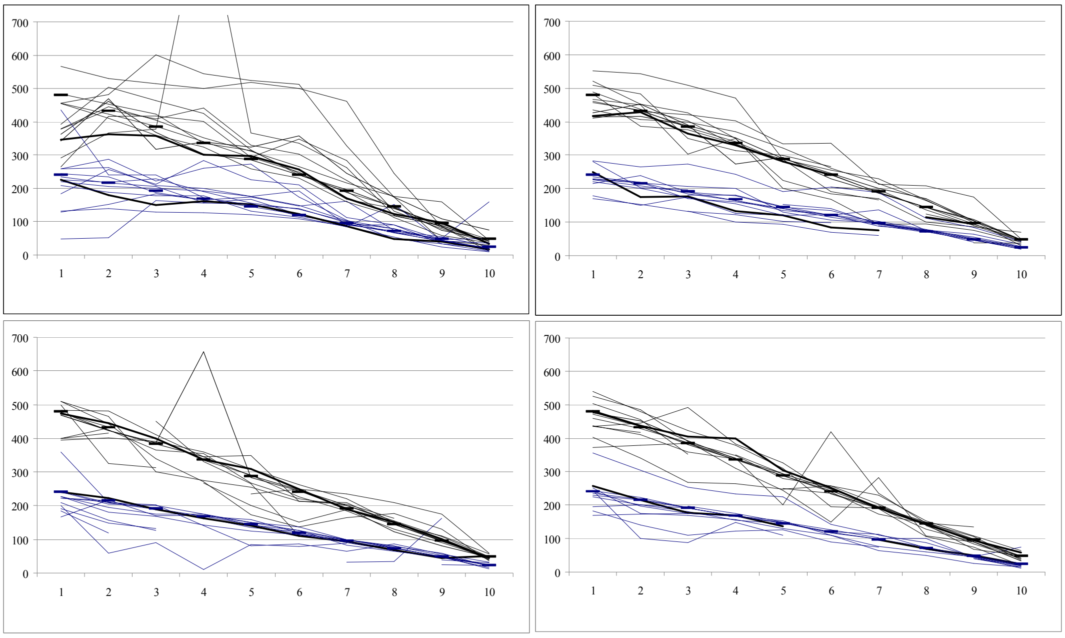


Figure 3. Avg prices in markets 1-4 (left to right, top to bottom) of the No Correlation treatment

The trajectories of all 8 cohorts are displayed for both assets L-share and U-share, and the average for each asset is indicated in bold. The dividend values are displayed by cascading short horizontal lines, which are well visible for the U-share. They are almost covered for the L-share by the lines that indicate the overall averages. The ordinate indicates the average prices from 0 to 700, and the abscissa the periods 1 to 10.

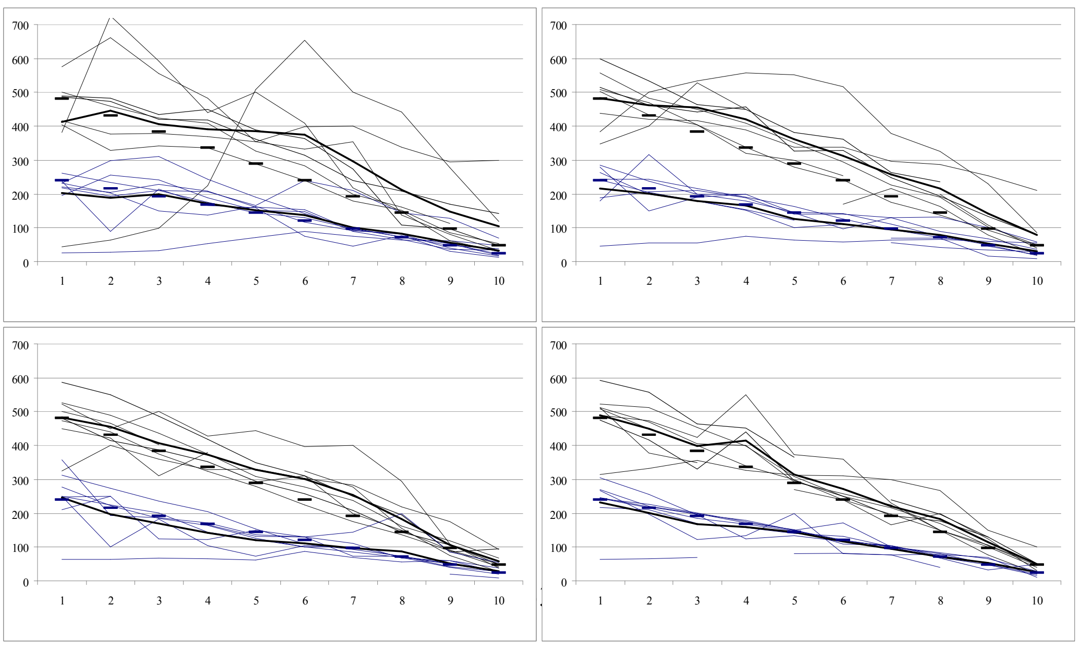


Table 4: Average *RD* measure and difference between shares in percent

The table records the average relative deviation (7) for each market, assets L, and U, and treatment. The bottom line shows the average over all markets. The third and sixth columns show the differences between RD values of assets L and U. The RD-values and their differences between assets L and U are tested. Z-statistics are recorded in brackets. Significant differences from zero resulting from the two tailed, one-sample Wilcoxon signed ranks test are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

	Perfect correlation ($n_1 = 12$)			No correlation ($n_0 = 8$)		
	RD_L	RD_U	RD_U-RD_L	RD_L	RD_U	RD_U-RD_L
Market 1	2.80	9.48	6.68	0.24	22.13^{**}	21.90^{*}
	[0.31]	[1.02]	[0.16]	[0.56]	[2.38]	[1.68]
Market 2	-5.53	-0.01	4.92	-5.03	22.49^{***}	27.52^{***}
	[-1.10]	[-0.39]	[1.02]	[0.14]	[2.52]	[2.52]
Market 3	-11.00^{**}	-2.19	8.82^{**}	-5.91	12.57^{**}	18.48^{*}
	[-2.28]	[-0.83]	[2.28]	[-0.21]	[2.24]	[1.40]
Market 4	3.68	2.44	-1.24	-5.54	11.40^{**}	16.94^{**}
	[-0.24]	[0.39]	[0.24]	[0.14]	[2.38]	[2.38]
Total	-2.51	2.28	4.79	-4.06	17.15^{**}	21.21^{***}
avg.	[-.78]	[1.26]	[0.94]	[0.14]	[2.52]	[2.52]

correlation treatment, the overall *RD* measure is not significantly different from zero; a significant (negative) *RD* measure is only recorded for the L-share in market 3.

Comparing across assets, we find that the *RD* measures of the L-share and the U-share are significantly different in the no-correlation treatment; in the perfect-correlation treatment there are no significant differences between *RD* measures of the L-share and the U-share.[35] If we compare *RD* measures between treatments and shares, we find that only the *RD* measure of the U-share in the no-correlation treatment is significantly different from (i.e., larger than) the others.[36] This misbehavior of the U-share is puzzling and seems to mainly drive the reported violation of parity pricing in the no-correlation treatment. The Spearman rank correlation

[35] Based on the two-tailed Wilcoxon signed ranks test results we reject the null hypothesis of equal RD measures between L-share and U-share for the no-correlation treatment; the *p*-value is 0.012. In the perfect-correlation treatment the *p*-value is 0.347 and so we conclude that the *RD* measures do no significantly differ across assets.

[36] The *p*-values of the two-tailed Mann-Whitney two-sample test (on the total average) are as follows; between U-shares ($p = 0.000$), between L-shares (0.227); between the difference of U-share and L-share (0.007).

between the RD_U and Δ are 0.168 and 0.810 in the perfect-correlation and the no-correlation treatments, respectively; the latter indicates a significant correlation (p-value is 0.015) whereas the former does not (p-value is 0.602).[37] The question here is why the deviations occur in the U-share of the no-correlation treatment and not in the L-share. Below we investigate this finding further by looking at the market behavior and at the valuations of the L-share and the U-share in the single-asset market.

4.D. Smart traders

Observation 6. *Individual performance (i.e. payoff) correlates with measured acuity.*

Support. Table 5A reports regression results. We use the subject's average payoff over the four markets as the individual-performance measure. As stated above, we measure individual acuity by the CRT score, i.e., the individual's number of correct answers. The average CRT score in our sample was 0.97.[38] As a risk-aversion measure we use the individual percentage allocation to the safe asset in the investment game; the average allocation was 60 percent. Finally, we consider gender, assigning value one for a female investor and zero otherwise. The share of females is 49 percent in our subject pool (see subject-pool composition details in Table A3 in the Online Appendix which is also appended to this paper).[39]

[37] Note also that the differences in *RD*-values between L-share and U-share and deviations from parity are highly correlated. The Spearman rank correlation coefficient is 0.862 on the overall sample, which is significant at any commonly accepted significance level (p-value is 0.000).

[38] This is a bit lower than average (see Frederick (2005), who finds an average in a very large sample of 1.24). One might have expected a better result here for CRT because of incentivization. The lower number could have resulted from the fact that our subjects were time constrained whereas Frederick's were not.

[39] As in many previous studies (see Charness and Gneezy, 2012), our measure finds that females are significantly more financially risk averse than males ($Z = 2.366$, $p = 0.018$, two-tailed test). In line with the results of Frederick (2005), we also find that males in our sample have significantly higher CRT scores ($Z = 3.838$, $p < 0.001$, two-tailed test).

Table 5A. Regression results

The first two columns record the outcomes of the cluster regression with robust standard errors of the within-group individual payoff and aptitude (the reciprocal of the relative frequency of a limit order that opens up an arbitrage opportunity) on the individual CRT-score (8), Risk Aversion, and FEM gender dummy. The other columns record the OLS regression results of measures (4)-(8) on the group average of the individual measures. The results of two-tailed t-tests are indicated [Z-statistics in brackets]; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p \leq 0.10$.

Variable	$Payoff_i$	$1\text{-}RFDLO_i$	Δ	*PD*	*DF*	*RFDLO*	RD_U
Intercept	2717.9*** [22.8]	0.976*** [138.9]	0.925** [0.02]	0.949* [2.04]	0.811** [2.73]	0.345*** [3.42]	0.514** [2.77]
CRT-score	161.8*** [3.67]	0.005** [2.07]	-0.643*** [-3.84]	- 0.69*** [-3.10]	- 0.579*** [-4.06]	- 0.142*** [-2.98]	-0.30*** [-3.34]
Female	-241.0*** [-2.90]	0.008 [1.31]	-0.198 [-1.20]	-0.164 [-0.75]	0.114 [0.81]	-0.086* [-1.82]	-0.058 [-0.66]
Risk aversion	-0.76 [-0.59]	-0.000 [-0.52]	0.001 [0.12]	0.002 [0.29]	-0.000 [-0.09]	-0.001 [-0.75]	-0.000 [-0.12]
perf. correlation treatment dummy			-0.824*** [-3.10]	-0.81** [-2.29]	-0.506** [-2.23]	-0.154* [-2.04]	-0.369** [-2.60]
CRT-score x treatment dummy			0.646** [2.42]	0.697* [1.95]	0.427* [1.87]	0.102 [1.35]	0.218 [1.53]
#obs.	174	174	20	20	20	20	20
#clusters	20	20					
R-squared	.125	.039	.653	.484	.640	.591	.728

The outcomes of the ordinary-least-square regression of the individual performance measure on the CRT score, Female, and Risk aversion are recorded in the first column of Table 5A. The OLS regressions use clustered standard errors (clustered by independent session). As indicated, the CRT score is a significant determinant of the individual performance in this regression, but gender and the risk-aversion measure are not significant. We find no significant differences across treatments.[40] The evidence clearly supports Observation 6.

Instead of measuring performance by payoff as in Observation 6, we can measure performance by the number of bad decisions in trading. As explained above, we compute the ratio of the number of discrepant limit orders to total limit orders, $RFDLO_i$. This ratio is next compared to the individual CRT score.

Observation 7. *The trader's aptitude is correlated with the measured individual acuity.*

Support. The trader's aptitude is reversely related to the relative frequency of submissions of discrepant limit orders to the trader's total number of submitted limit orders, (1 - $RFDLO_i$). As characterized above, a discrepant limit order offers an arbitrageur's opportunity to realize a (expected) gain by swapping assets. The measured trader's aptitude is regressed on individual characteristics. The second column of Table 5A records OLS regressions with clustered standard errors (clustered by independent cohort). We find that the individual acuity measured by the CRT score thus explains much of the bidding data, supporting Observation 7.

Observation 8. *When there is high overall trader acuity, price discrepancies are smaller, fewer discrepant orders are submitted, and the price of assets is closer to parity.*

Support. In Table 5A we also record four OLS regressions of our measures (4)-(6), (8) on the group aggregate personal measures. In these regressions the average CRT score always shows up significantly. The difference of price from parity, deviation from fundamentals, and price

---

[40] The two-tailed Mann-Whitney test between treatments returns the following *p*-values on the individual-level data, 0.559 (risk aversion), 0.809 (Female) and 0.987 (CRT score), and similar *p*-value results on the session-level data, 0.165 (risk aversion), 0.754 (Female), and 0.529 (CRT score).

discrepancy as well as the relative frequency of discrepant orders decrease in markets with overall high trader acuity. In the regression, we also control for treatment effects of CRT. As the interaction with the treatment dummy indicates, the CRT-effect is strong for the no-correlation treatment. In Table 5B we report Spearman rank-correlation coefficients of the CRT score with our measures of market efficiency. The coefficients also hint at a treatment effect. The correlation coefficients usually have the same sign in both treatments, but the magnitude of the coefficients is larger for the no-correlation treatment. The reported test results indicate that the reported CRT effects could be driven by the no-correlation treatment. So, as for Hypothesis 4 and Observation 8, we conclude that acuity could actually affect reduction of price discrepancy; however, potential discrepancies do not disappear completely.

Table 5B. Spearman rank correlation coefficient with CRT score
Correlation coefficients are reported for CRT effect in measures (4)-(8) for each treatment separately. The results of two-tailed Spearman rank correlation test are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p \leq 0.10$

	Δ	PD	DF	$RFDLO$	RD_U
Perfect correlation treatment, n = 12	-0.236	-0.271	-0.236	-0.268	-0.168
No-correlation treatment, n = 8	-0.732**	-0.634*	-0.634*	-0.683*	-0.927***

Observation 9. *When there is low overall trader acuity, or low trader aptitude, U-share prices exceed fundamentals in the no-correlation treatment. There is no significant impact of trader acuity, risk aversion, or gender composition with either the L-share in the no-correlation treatment or either share in the perfect-correlation treatment.*

Support. In the last column of Table 5A, we record a simple OLS regression of our acuity measure on the group aggregate personal measures for the no-correlation treatment. The result shows that the *RD* measure increases with decreasing CRT-score. This effect is present in the U-share of the no-correlation treatment only, as the effect in L-share and in the perfect-correlation treatment is not significantly different from zero (see also Table 5B).[41]

4.E. Equilibrating dynamics

Observation 10. *The relative frequency of discrepancies in the perfect-correlation treatment is lower than in the no-correlation treatment. Where a pricing discrepancy arises, subjects trade the high-expected-return twin share first rather than the less profitable one. Individual subjects rarely exploit pricing discrepancies by asset swaps.*

Support. As reported in Table 3C, on average 5.5 percent and 10.23 percent of all submitted orders lead to discrepancies in the order book for the perfect-correlation treatment and the no-correlation treatment.[42] We observe equilibrating dynamics as discussed in Section 4. When a discrepancy arises, a trade in one twin share typically leads to a higher expected return than a trade in the other.

As recorded in Table 6, the relative frequencies at which the high-return trade is enacted first are 0.886 and 0.916 in the perfect-correlation treatment and the no-correlation treatment. The results of the Wilcoxon signed-ranks test show that the equilibration dynamics work as suggested above, thus confirming Hypothesis 5A. It is significantly more likely that a

[41] A significant correlation of RD_U measure and CRT-score is only notable in the no-correlation treatment. Conducting an OLS regression (with CRT as the only explanatory variable) separately for each treatment results in *p*-values of 0.318 (perfect correlation) and 0.026 (no correlation) for the slope of the CRT score. The other explanatory variables including our measure of risk aversion and the gender share have no significant impact on any *RD* in the two treatments.

[42] The difference between treatments is in the expected direction, and significant as reported above. The share of discrepancies and the *PD*-value in a session are highly correlated. The OLS regression (on the level of the independent session) of the *RFDLO*-value on the *PD*-value indicates a significantly positive slope (z-statistic of 2.43, i.e., p = 0.026) and a determination coefficient of 0.25. When a discrepancy arises, the median response time is 4 seconds.

transaction occurs first in the high-expected-return twin-share, thus eliminating the price discrepancy without necessarily booking an arbitrage gain. So prices are pushed in the direction of fundamental dividend value.[43]

Table 6: Equilibrating dynamics: relative frequency of market orders in the high-expected-return share of given discrepancy

The Table records the relative frequency in percent by which the market trades the more-profitable rather than the less-profitable share when a discrepancy arises. Significant differences per the one-tailed Wilcoxon signed-ranks tests are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. *Numbers in brackets are z-statistics.*

	Perfect correlation ($n_1 = 12$)	No correlation ($n_2 = 8$)	Two tailed Mann-Whitney test (between treatments), p-value [Z-statistic]
Market 1	85.0***	95.8***	0.054**
	[2.99]	[2.52]	[2.47]
Market 2	88.4***	98.1***	0.086*
	[3.07]	[2.55]	[1.71]
Market 3	93.8***	81.8**	0.332
	[3.10]	[2.25]	[-0.97]
Market 4	87.0***	90.9***	0.586
	[3.07]	[2.52]	[0.56]
Total	88.6***	91.7***	0.758
	[3.06]	[2.52]	[0.31]

In fact, subjects rarely exploit the price discrepancy (as arbitrageurs would do) by selling the relatively expensive twin share and simultaneously buying the cheap one. Such hedging trades happen about 1% of the time when a discrepancy arises. Our hypothesis 5B is not supported by the data; 99% of discrepancies were eliminated by independent trades in the market rather than by individuals engaging in arbitrage; no arbitrageur subjects dominate the market. We conclude that the described market equilibration dynamics imply a narrowing of the gap between prices of twin shares in our treatments rather than high-skilled individual traders.

[43] The effect is a bit stronger in the no-correlation treatment than in the perfect correlation treatment. However, equilibration dynamics are strong in both treatments. The difference is possibly related to the above-described fact that relative pricing is a more important driver of price behavior in the perfect correlation treatment than in the no-correlation treatment.

4.F. Individual portfolio diversification

Despite the fact that diversification can lower the return volatility in the no-correlation treatment, but not in the perfect-correlation treatment, the average individual difference in subjects' portfolios of L-shares and U-shares is 2 (= dZ) in each treatment. The difference in individual portfolio diversification between treatments is close to zero.[44] Hence, we have no evidence in support of Hypothesis 6.

4.G. Summary of experimental results

We do not observe pricing of assets at fundamental dividend value, but we have support for Hypothesis 1A (MM invariance theorem) in our perfect-correlation treatment, where exploitation of a price discrepancy is riskless. This differs in relative terms when we compare the data to the no-correlation treatment, where exploitation of price discrepancies across assets has risk. Hypothesis 1B (no price discrepancy) is not supported in either of our treatments as price discrepancies seem to persist. Under perfect correlation, however, adjustment to a level where discrepancies stabilize is fast (achieved by the second market) whereas under no correlation adjustment takes at least more time. Hypothesis 2 (hedging effect) is confirmed; the results on price discrepancies of the perfect-correlation treatment are closer to the theoretical prediction than those of the no-correlation treatment.

Support for Hypothesis 3 (experience effect) is no surprise; experienced subjects trade closer to the theoretical predictions than inexperienced subjects. Nonetheless, unlike the results of the single-asset market (Smith et al. 1988) our data do not suggest a complete convergence on fundamental dividend values or vanishing price discrepancies in repeated markets. Moreover,

[44] The p-value of the one-tailed Mann-Whitney between-sample tests is 0.47.

discrepant order submissions do not cease with repetition, as indicated by the stable relative frequency of discrepant limit orders. We find some support for Hypothesis 4 (smart trader effect) as our measure of aptitude increases in our measure of acuity and market mispricing decreases with the measured level of acuity. To the extent that experience may substitute for initial acuity, ours confirm other results in the experimental-asset-markets literature (Palan 2013).

Our data support our equilibration Hypothesis 5A. When a discrepancy arises, the more profitable transaction is enacted first thus pushing price dynamics towards fundamentals. These dynamics occur in both treatments. In the perfect-correlation treatment market equilibration both vis-à-vis fundamentals and vis-à-vis the balancing of relative prices is strong. Both effects seem to lead to the confirmation of MM invariance in the perfect-correlation treatment. We observe that discrepancies are fewer than in the no-correlation treatment. Hypothesis 5B is not supported by our data since individual arbitraging through asset swaps is rare when discrepancies arise.

There are two surprising effects here. First, we observe no significant deviations from dividend value in the perfect-correlation treatment and for the L-share in the no-correlation treatment. This result stands in opposition to the reports of mispricing in the single-asset market (see the survey by Palan 2013). Second, we observe a positive significant deviation from dividend value only for the U-share in the no-correlation treatment in particular when trader aptitude is low.[45] This effect seems to be driving the deviation from price parity in the no-correlation treatment in the direction as reported in Observation 1. Regarding the first effect, we must address the question of whether the co-existence of two assets tends to limit the occurrence of bubbles. In fact, it has been suggested in the literature that trading in two assets can lead to smaller bubbles (Lee, Noussair, Plott 2001). Other papers suggest that also a careful explanation

[45] The correlation of the U-share bubble with low trader aptitude could be a spurious effect.

of the environment can lead to smaller bubbles (Kirchler and Huber 2012, Palan 2013). If not spurious, the second effect could result from a behavioral bias such as the *certainty effect* (Kahneman and Tversky 1986).[46] L-shares involve a possible zero dividend, since with 0.25 of probability a zero dividend can occur in each period. In this context it seems noteworthy that the cumulative prospective utility of the lotteries corresponding to the L-share and the U-share differs by more than the difference in expected value when the parameter estimates of Tversky and Kahneman (1992) are applied. There is a small risk of not receiving any dividend pay at all. Given the received literature and given our no-correlation treatment data, we test the following hypothesis.

Hypothesis 7 (certainty effect). Share prices are higher when possible dividends are strictly positive than when dividends can be zero.

We address the hypothesis in the following section in a between-subjects single-asset market experimental design. In one treatment subjects trade the L-share and in the other treatment they trade the U-share. We expect that the *RD* measure is greater for the U-share than for the L-share. In case we do not observe this effect in the single asset market setting, we must conclude that the reported U-share price results from relative market valuation in the no-correlation treatment.

5. Further Experiments

We conducted two additional treatments to further investigate patterns found in our two initial treatments; we describe these below.

[46] Tversky and Kahneman (1986) reported evidence on the common ratio effect. In binary-choice tasks with a safe pay and a risky lottery, they found that subjects had a preference for the safe pay if the risky lottery involved the possibility of a zero pay.

5.A. Single-asset market experiments

We proceeded with a third treatment with only one asset to try to cleanly address the question of whether the above reported non-existence of bubbles results from the two-asset environment or rather from our general settings.[47] This also helps us to address Observation 5b) to see if the higher relative market valuation of the U-share in the no-correlation treatment is a behavioral artifact, similar to the dividend puzzle, according to which shares with non-zero dividends fetch a higher price (DeAngelo and DeAngelo 2006).

The experimental procedures in the single-asset market experiments were the same as those described above (see the instructions in the Online appendix which is appended to this paper). However, instead of simultaneous trading in two assets, subjects traded in a single-asset market involving 10 end-of-period dividend draws. Applying between-subjects variation, subjects traded the shares with the equally likely dividends {0, 8, 28, 60} in the L-share treatment and they traded the shares with the equally likely dividends {24, 32, 56, 84} in the U-share treatment. Different from the original sessions, subjects participated in only two (and not in four) consecutive repetitions of the asset market.[48] The expected payoff for participation in the experiment was the same across all treatments. In the L-share treatment, subjects were endowed with six shares, and in the U-share treatment subjects were endowed with three shares. Subjects were able to short-sell double the number of shares with which they were endowed.

[47] Besides the two-asset setting, there are some other features in our design that could have contributed to the non-existence of bubbles. Subjects had to pass several comprehension tests and the expected dividend value was exhibited in each period on the subjects' screens. This transparency may have decreased the lack of common knowledge (Cheung, Hedgard, Palan 2014). Short-selling is possible, which can increase downward pressures of prices (Haruvy and Noussair 2006). Also, the number of periods is 15 periods in the standard setting whereas it is 10 in ours.

[48] In total, subjects traded three times in 10 periods in continuous double-auction markets. Subjects traded in the L-share or the U-share twice. The third market of trading involved another experiment with single-period markets, which we describe further below. In half of the sessions, we had the two single-asset markets first and then the single period markets. In the other half the ordering was reversed. We found no order effects, so we pool the data.

The data include eight independent cohorts in each of the L-share and U-share treatments, with each trading for two consecutive market rounds in the continuous double auction. 138 subjects participated in these sessions. As before, the market size was nine.[49]

Observation 11. *L-share and U-share average prices are close to dividend value in the single-asset environment.*

Support. Table 7 records the *RD* measures for the two single asset treatments. The recorded results of the Wilcoxon signed ranks test indicate that deviations of the *RD* measures are not significantly different from zero for any market of the single-asset treatments. Compared to the *RD* measure of the U-share in the no-correlation treatment, the *RD* measures in the single-asset market treatments are significantly smaller. Compared to each other, compared to the L-share in the no correlation treatment, and compared to the L-share and the U-share in the perfect correlation treatment, all differences are insignificant according to the results of the two-tailed Mann-Whitney test of two independent samples.[50]

Observation 11 indicates that we have no systematic mispricing in the single-asset treatments. In the literature, it has been suggested that a careful introduction of subjects to the environment could amend mispricing (Palan 2013). In our experiment, subjects were informed in each period about the remaining dividend draws and the expected sum of remaining dividends. It is quite possible that that information influenced the decision making of subjects in our

[49] In each of the two treatments, we had one cohort with seven, another with eight, and six with nine participants.
[50] The *p*-values of the test between single-asset market treatments are recorded in the table. The *p*-values between *RD*s for the U-share in the no-correlation treatment and the U-share treatment are 0.046 and 0.016 for the first and second market; for the L-share treatment and the U-share in the no-correlation treatment these p-values are 0.074 and 0.002, respectively. All other between-treatment comparisons, as listed in the text, show *p*-values above 0.480. The U-share price trajectories of the no-correlation treatment behave differently than all other price trajectories.

markets.[51] Given Observation 11, we find no support for Hypothesis 7 in the single-asset environment. The relatively high valuation of the U-share in the no-correlation treatment seems to be an artifact of relative valuation. To see whether this result and also the confirmation of the MM theorem is an artifact of the described environment (Smith et al. 1988), we report in the following section another presumably-simpler experimental treatment with single-period markets.

Table 7: RD-value in single-asset market experiment (in percent)

The table records the RD-values for the single-asset sessions. No significant differences from zero per the two-tailed Wilcoxon signed-ranks tests. Numbers in brackets are z-statistics.

	L-share treatment ($n_1 = 8$)	U-share treatment ($n_2 = 8$)	Mann-Whitney test (between treatments), *p*-value [*Z*-statistic]
Market 1	3.57 [0.42]	-2.00 [0.42]	1.000 [0.00]
Market 2	-6.33 [-0.42]	-2.29 [0.00]	0.529 [-0.63]
Total average	-1.38 [-0.14]	-2.15 [0.28]	0.753 [-0.32]

5.B Single-period market experiments

Our fourth treatment has single-period asset markets involving four repetitions with a 10-fold dividend.[52] The overall procedures were the same as described above besides the following differences. Subjects were endowed in every period with two L-shares and two U-shares that they traded in the double-auction market. These shares paid one dividend at the end of the

[51] We chose to provide this information to the subjects to try to help manage the cognitive burden in this rather complex environment. This may have served to reduce bubbles, but this was not the primary point of the paper.

[52] In total there were 10 periods. The described assets were traded in periods 1, 4, 7 and 10. In the other periods, assets with the same expected values were traded. For ease of presentation we skip the discussion of these assets here. However, the analysis of those assets are completely in line with the reported data. Please see the instructions in the Online Appendix for futher details. Subjects traded in a total of three consecutive markets of 10 periods, one involving a single-period treatment and the other two markets involving a single asset treatment. We found no order effects in the data, so we pool the data by treatment.

period and then were cancelled. In the experiment, one period would be selected for payment. Possible dividends on an L-share were {0, 80, 280, 600}. In the perfect-correlation treatment, the possible dividends on the U-share were always 240 cash units higher than the dividend on the L-share. In the no-correlation treatment, the dividends of the L-share and the U-share were independent; the equally likely dividends per U-share were {240, 320, 520, 840}.

Including the pilot session, we have nine independent cohorts participating in the perfect correlation treatment, and eight independent cohorts for the no-correlation treatment.

Observation 12. *We find significant differences from parity pricing neither in any period nor in any single-period asset market treatment. Between treatments we observe no significant differences in any period.*

Support. Table A4 in the Online Appendix (appended to this paper) records the average difference from parity for the repetitions. The test results show that the difference from price parity between assets is not significant in any treatment and period.[53] There are no significant differences between the two treatments.

Given Observation 12, on average subjects trade at fundamentals and at parity. However, there are significant deviations from parity within cohorts. In some cohorts the L-share is valued relatively above the U-share; in other cohorts the U-share is valued above the L-share. Hence, we find no support for Hypothesis 7 in the single-period market; the high relative valuation of the U-share of the no-correlation treatment is not reproduced in this comparatively simple market environment. In line with Observation 8, the shares in the single-period treatment trade closer to

[53] For completeness, the average absolute deviations from parity are 14 percent and 12 percent and the deviations from fundamentals are 16 and 13 percent for the perfect-correlation treatment and the no-correlation treatment, respectively. The absolute deviations and the deviations from fundamentals do not decline significantly between repetitions in either treatment.

fundamentals with higher acuity in the market. In contrast to Observations 1 and 2, there are no significant differences between treatments in the single-period treatment. This result suggests that the differences between arbitrage dynamics and equilibrium dynamics cannot easily be detected in a simple market setting. To detect the differences, one may need a rather extreme test like the one we have used in the context of bubbles and declining fundamental values (Smith et al 1988).

6. Conclusions

Our data support the MM invariance theorem in the following way. As proposed in the arbitrage proof of the original paper (Modigliani and Miller 1958), we observe the same average market values for shares of perfectly correlated returns. The data suggest that perfect correlation of future returns (where price discrepancies can be arbitraged away without risk) is required to obtain the law of one price in our setting. This necessary condition is recommended through our control treatment where perfect correlation is removed. The deviations from parity pricing and the observed pricing discrepancies are significantly larger in the control treatment than in the perfect-correlation treatment. Despite the fact that earlier research reported an insensitivity of behavior to changes in return correlations (e.g., Kroll et al. 1988), we do find a significant effect. Under no correlation, i.e., with independent multi-period lived assets, we do not obtain the law of one price for the same idiosyncratic risks of our single assets. With limits to arbitrage, the market manifests a risk premium. The (“leveraged”) L-share is less pricey than the (“all equity”) U-share. This could be a spurious effect; our research in this respect is not conclusive. We investigated if this effect may be driven by a behavioral bias in relative valuation similar to the certainty effect (Kahneman and Tversky 1986). The fact that the dividend in the U-share is strictly positive (whereas the L-share dividend can be zero) could possibly bias subjects to push up U-share prices when trader acuity is low. We observe this effect only in the two-asset setting of the no-correlation treatment. The higher market valuation of the U-share compared to the L-share treatment in the no-correlation treatment is not observed in our single-asset market treatments. For existing limits to arbitrage, our data (in the no-correlation treatment) suggest that capital structure may indeed affect the market value of the company. The finding that value invariance holds under perfect correlation but not necessarily with imperfect correlation is a key contribution of the paper. This result is intriguing, as the CAPM suggests the law of one price for our zero-correlation setting. This result along with the result that single-asset markets behave differently from multiple-assets markets show that more experimental research is needed to uncover the trading dynamics and interactions of multiple multi-period-lived assets.

Our study does not provide a test of the unrestrained validity of MM invariance, since we consider a zero interest rate and non-traded debt. Nonetheless, we note that we obtain this result in the standard setting of Smith et al. (1988), known for producing mispricing among inexperienced subjects. We found that value invariance can also be obtained in simpler settings, including those with constant fundamental dividend value. For the single-period setting our data suggest that the law of one price can be obtained on average even if returns are not perfectly correlated but only the same in expected terms. In more complex settings, as suggested in our experiments with two multi period lived assets, MM invariance seems to require perfect correlation of future return streams.

Our results also shed light on different levels of market efficiency. We find support of market efficiency for the case of riskless arbitrage. We also find that market efficiency increases with repetition. Price discrepancies decrease and prices move towards fundamental dividend values with repetition, and we document the existence of equilibrium dynamics when price discrepancies arise. We observe that high overall acuity, as measured by the CRT score, reduces price discrepancy in the market. Deviations from theoretical predictions become smaller when we look at markets with overall high trader acuity. Nonetheless, the deviations are never entirely removed. So for the data from our design, we have the sense that Rietz (2005) is correct when he states that without a dedicated arbitrageur taking advantage of any mispricing, the laboratory market does not completely eliminate price discrepancies.

As is frequently observed with market studies, we conclude that the data suggest a movement of behavior in the *direction* of the predictions of financial economic theory, but do not provide full quantitative support of the theoretical benchmark. Hence, as price discrepancies do not cease during the experiment our data hint to significant potential gains for arbitrageurs. In the real world, exploitation of price discrepancies requires the arbitrageur to take risk (as has been shown in the twin-share LTCM cases, Shleifer 2000). So in the real world, arbitrageurs may require deep pockets and patience to lock in their arbitrage gains.

References

Abbink, Klaus and Bettina Rockenbach, 2006. Option pricing by students and professional traders: a behavioural investigation, *Managerial and Decision Economics* 27(6), 497-510.

Ackert, Lucy F., Narat Charupat, Richard Deaves, and Brian D. Kluger, 2009. Probability judgment error and speculation in laboratory asset market bubbles. *Journal of Financial and Quantitative Analysis* 44(3), 719–744.

Akiyama, Eizo, Nobuyuki Hanaki, and Ryuichiro Ishikawa, 2013. It is Not Just Confusion! Strategic Uncertainty in an Experimental Asset Market. *Aix Marseilles School of Economics WP* 2013 - Nr 40.

Biais, Bruno, and Pouget, Sébastian (1999). Microstructure, incentives and convergence to equilibrium in experimental financial markets. Unpublished working paper, Toulouse University, France.

Breaban, Adriana and Charles Noussair, 2014. Fundamental Value Trajectories and Trader Characteristics in an Asset Market Experiment. (*CentER Discussion Paper*; Vol. 2014-010). Tilburg: Economics.

Cadsby, Bram C., and Elisabeth Maynes, 1998. Laboratory Experiments in Corporate and Investment Finance: A Survey. *Managerial and Decision Economics* 19(4/5), Laboratory Methods in Economics, 277-298.

Chan, Kenneth S., Vivian Lei, and Filip Vesely, 2013. Differentiated Assets: An Experimental Study on Bubbles. Economic Inquiry 51(3), 1731–1749.

Charness, Gary and Uri Gneezy, 2010. Portfolio choice and risk attitudes: An experiment. *Economic Inquiry* 48(1), 133-146.

Cheung, Stephen L., Morten Hedegaard, and Stefan Palan, 2014. To see is to believe: Common expectations in experimental asset markets. *European Economic Review* 66, 84-96.

Corgnet, Brice, Roberto Hernán-González, Praveen Kujal, and David Porter, 2015. The Effect of Earned vs. House Money on Price Bubble Formation in Experimental Asset Markets. *Review of Finance* 19(4): 1455-1488.

DeAngelo, Harry, and Linda DeAngelo, 2006. The irrelevance of the MM dividend irrelevance theorem. *Journal of Financial Economics* 79(2), 293-315.

Dufwenberg, Martin, Tobias Lindqvist, and Evan Moore, 2005. Bubbles and Experience: An Experiment. *American Economic Review*, 95(5): 1731-1737.

Eckel, Catherine C., and Sascha Füllbrunn, 2015. Thar 'She' blows? Gender, Competition, and Bubbles in Experimental Asset Markets. *American Economic Review* 105(2), 906-920.

Fischbacher, Urs, 2007. z-Tree: Zurich toolbox for ready-made economic experiments. *Experimental Economics* 10(2), 171-178.

Frederick, Shane, 2005. Cognitive Reflection and Decision Making. *Journal of Economic Perspectives* 19(4), 25-42.

Froot, Kenneth A., and Emil M. Dabora, 1999. How are stock prices affected by the location of trade?. *Journal of financial economics* 53(2), 189-216.

Greiner, Ben, 2004. An Online Recruitment System for Economic Experiments. In: Forschung und wissenschaftliches Rechnen 2003. *GWDG Bericht* 63, 79-93.

Haruvy, Ernan, and Charles N. Noussair, 2006. The effect of short selling on bubbles and crashes in experimental spot asset markets. *Journal of Finance* 61(3), 1119-1157.

Haruvy, Ernan, Yaron Lahav, Charles Noussair, 2007. Traders' Expectations in Asset Markets: Experimental Evidence. *American Economic Review* 97(5), 1901-1920.

Judd, Kenneth L. and Sy-Ming Guu, 2001. Asymptotic methods for asset market equilibrium analysis. *Economic Theory* 18, 127–157.

Kirchler, Michael, Jürgen Huber, Thomas Stöckl, 2012. Thar she bursts – Reducing confusion reduces bubbles. American Economic Review 102(2), 865-883.

Kroll, Yoram, Haim Levy and Amnon Rapoport, 1988. Experimental test of the Separation Theorem and the Capital Asset Pricing Model. *American Economic Review* 78(3), 500-519.

Lamont, Owen A., and Richard H. Thaler, 2003. Anomalies: The Law of One Price in Financial Markets. *Journal of Economic Perspectives* 17(4), 191-202.

Lei, Vivian, Charles N. Noussair, and Charles R. Plott, 2001. Nonspeculative bubbles in experimental asset markets: Lack of common knowledge of rationality vs. actual irrationality. *Econometrica* 69(4), 831-859.

Levati, M. Vittoria, Jianying Qiu and Prashanth Mahagaonkar, 2012. Testing the Modigliani-Miller theorem directly in the lab. *Experimental Economics* 15(4), 693–716.

Miller, Merton H., 1988. The Modigliani-Miller Propositions After Thirty Years. *The Journal of Economic Perspectives* 2(4), 99-120.

Modigliani, Franco and Merton H. Miller, 1958. The Cost of Capital, Corporate Finance and the Theory of Investment. *American Economic Review* 48(3), 261-97.

Modigliani, Franco and Merton H. Miller, 1959. The Cost of Capital, Corporate Finance and the Theory of Investment: Reply. *American Economic Review* 49(4), 655-669.

Modigliani, Franco and Merton H. Miller, 1963. Corporate Income Taxes and the Cost of Capital: A Correction. *American Economic Review* 53(4), 433-443.

Noussair, Charles N., and Steven Tucker, 2006. Futures markets and bubble formation in experimental asset markets. Pacific Economic Review 11(2), 167-184.

Noussair, Charles N., Steven Tucker, and Yilong Xu, 2016. Futures markets, cognitive ability, and mispricing in experimental asset markets. Journal of Economic Behavior & Organization 130, 166-179.

O'Brien, John and Sanjay Srivastava, 1991. Dynamic Stock Markets with Multiple Assets: An Experimental Analysis. The Journal of Finance 46(5), 1811-1838.

Oliven, Kenneth and Thomas A. Rietz, 2004. Suckers Are Born but Markets Are Made. *Management Science* 50(3), 336–351.

Palan, Stefan, 2013. A Review of Bubbles and Crashes in Experimental Asset Markets. *Journal of Economic Surveys* 27(3), 570-588.

Porter, David P., and Vernon L. Smith, 1995. Futures contracting and dividend uncertainty in experimental asset markets. Journal of Business 68(4), 509-541.

Rietz, Thomas A., 2005. Behavioral Mis-pricing and Arbitrage in Experimental Asset Markets. *Working Paper*, University of Iowa.

Shleifer, Andrei, 2000. *Inefficient Markets: An Introduction to Behavioral Finance* (Clarendon Lectures in Economics), Oxford University Press.

Shleifer, Andrei and Lawrence H. Summers, 1990. The noise trader approach to finance. *Journal of Economic Perspectives* 4(2), 19-33.

Smith, Vernon L., Gerry L. Suchanek and Arlington W. Williams, 1988. Bubbles, Crashes, and Endogenous Expectations in Experimental Spot Asset Markets. *Econometrica* 56(5), 1119-1151.

Stiglitz, Joseph, 1969. A Re-Examination of the Modigliani-Miller Theorem. *American Economic Review* 59(5), 784-93.

Stöckl, Thomas, Jürgen Huber, and Michael Kirchler, 2010. Bubble measures in experimental asset markets. *Experimental Economics* 13(3), 284-298.

Sunder, Shyam, 1995. Experimental Asset Markets. In: *The Handbook of Experimental Economics* (edited by John Kagel and Al Roth), Princeton University Press, 445-500.

Tversky, Amos, and Daniel Kahneman, 1986. Rational choice and the framing of decisions. *Journal of Business*, S251-S278.

Tversky, Amos, and Daniel Kahneman, 1992. Advances in prospect theory: Cumulative representation of uncertainty. *Journal of Risk and uncertainty* 5(4), 297-323.

Online Appendix
(see Journal of Finance webpage)

Supplement to accompany

"A test of the Modigliani-Miller invariance theorem and arbitrage in experimental asset markets"

by

Gary Charness and Tibor Neugebauer

A **Tables A1-A4**

B **Documentation of data and experimental software**
(downloadable from the webpage of the Journal of Finance)

C **Experimental Instructions**
(downloadable from the webpage of the Journal of Finance)

Table A1. Average median difference from parity Δ in percent

The table records the median difference from parity (4) over ten periods and averaged over all sessions for each market and both treatment. The first column records the averages for the perfect-correlation treatment and the second column for the no-correlation treatment. The bottom line shows the average difference from parity over all periods. Asterisks indicate significant differences from parity measured by the two tailed, one-sample Wilcoxon signed-ranks test. The last column shows the p-value resulting from the two-tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Treatment	Perfect correlation ($n = 12$)	No correlation ($n = 8$)	Mann-Whitney test (between treatments), p-value, [Z-statistic]
Market 1	8.39 [0.67]	28.24** [2.38]	0.031** [2.16]
Market 2	3.37 [1.10]	31.26** [2.52]	0.006*** [2.77]
Market 3	2.54 [1.10]	19.03** [2.24]	0.054* [1.93]
Market 4	0.24 [0.24]	13.73** [2.52]	0.064*** [2.70]
Average	4.41 [1.49]	23.30** [2.52]	0.014** [2.78]

Table A2A: Average median *PD*-value (in percent)

The table records the median price discrepancy (5) for the ten market periods averaged over all sessions for each market and treatment. The bottom lines show the average price discrepancy over all periods, and the one-tailed Page trend test that checks for a significant decline in repeated markets. The last column shows the p-value resulting from the two-tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. ^One-tailed tests.

	Perfect correlation ($n_1 = 12$)	No correlation ($n_2 = 8$)	Mann-Whitney test (between treatments), *p*-value, [Z-statistic]^
Market 1	22.97	39.78	0.061^{*} [1.54]
Market 2	12.62	34.13	0.013^{**} [2.16]
Market 3	13.38	24.06	0.124 [0.97]
Market 4	11.80	20.00	0.109 [1.23]
Total	15.51	29.80	0.045^{**} [1.70]
p-value (one-tailed Page trend test)^	0.212 [-.80]	0.014^{**} [-2.20]	

Table A2B: Average median *DF*-value (in percent)

The table records the deviation from fundamentals (6) for the ten market periods averaged over all sessions for each market and treatment. The bottom lines show the average deviation from fundamentals over all periods, and the one-tailed Page trend test that checks for a significant decline in repeated markets. The last column shows the p-value resulting from the two-tailed, two-sample Mann-Whitney test. Z-statistics are recorded in brackets. Significant differences are indicated; $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$. ^One-tailed tests.

	Perfect correlation ($n_1 = 12$)	No correlation ($n_2 = 8$)	Mann-Whitney test (between treatments), *p*-value [*Z*-statistic]
Market 1	20.75	38.04	0.247 [1.16]
Market 2	14.77	30.70	0.037^{**} [2.08]
Market 3	15.01	21.82	0.396 [0.85]
Market 4	14.82	17.41	0.316 [1.00]
Total	15.93	27.05	0.190 [1.31]
p-value (one-tailed Page trend test)^	$.055^{*}$ [-1.60]	$.000^{***}$ [-3.60]	

Table A3. Subject pool composition

Gender	Field of Studies	Number of subjects	North American	Mexican, South American	Asian	Rest of World, not revealed°	Subject Age 18-32	Payoff Rank 1-9	CRT score 0-3	Risk aversion %safe invest 0-100
Male	Economics, financial math, actuarial sciences	29	10	3	7	9	19.9	4.4	1.2	48.8
	Physics, electrical/mechanical engin., engineering	7	7				20.0	4.6	1.0	59.6
	Mathematics, statistics	7	2	1	3	1	21.7	4.6	1.3	57.1
	Computer science, computer engine.	6	3		2	1	19.0	3.9	1.7	48.3
	Bio-/chemistry, chemical eng, pharmacology, pre-med	13	5	2	4	2	20.6	4.7	1.4	60.4
	Biology, biopsychology, microbiology, bio anthropology, pre-bio, zoology	15	6	3	3	3	19.3	3.9	1.3	56.2
	Psychology	1			1		21.0	4.9	2.0	100.0
	Global/environmental studies/ earth science/geography/ecology, fishery management	7	2	2	2	1	23.9	4.6	0.9	55.7
	Film and arts, music, communication	4	3		1		21.0	6.7	1.0	47.5
	Total | Average	89	38	11	23	17	20.4	4.5	1.3	54.3
Female	Economics, financial math, actuarial sciences	20	6	3	9	2	19.6	5.3	0.6	62.6
	Physics, electrical/mechanical engin., engineering	6	2		3	1	19.8	5.1	1.0	76.7
	Mathematics, statistics	7	2		2	3	19.6	5.0	0.3	61.6
	Computer science, computer engine.	2	1			1	18.5	2.9	1.5	69.5
	Bio-/chemistry, chemical eng, pharmacology, pre-med	5	1	2	2		20.6	6.4	0.2	53.0
	Biology, biopsychology, microbiology, bio anthropology, pre-bio, zoology	29	11	4	6	8	19.7	6.0	0.7	65.0
	Psychology	5	2	1	2		20.0	6.4	0.4	73.4
	Global/environmental studies/ earth science/geography/ecology, fishery management	7	4	1	2		21.3	4.9	0.6	67.9
	Film and arts, music, communication	2			2		19.0	4.9	2.0	52.5
	Not revealed°	2				2	19.0	3.9	0.5	97.0
	Total | Average	85	29	11	28	17	19.8	5.5	0.7	65.6
	(Female share %)	(49%)	(43%)	(50%)	(55%)	(50%)				
	Sample Total | Average	174	67	22	51	34	20.1	5.0	1.0	59.8

° 21 subjects (12%) did not respond to the question of their nationality, two female subjects did not respond to the question of their field of studies

Table A4. Average difference from parity Δ in percent in the single-period asset-market experiment

Treatment	Perfect correlation ($n = 9$)	No correlation ($n = 8$)	Mann-Whitney test (between treatments), p-value, [Z-statistic]
Market 1	-1.53 [-.30]	-11.8 [-1.12]	0.29 [-1.06]
Market 2	3.49 [0.42]	-5.27 [-.84]	0.336 [-.96]
Market 3	0.71 [.12]	-3.42 [-.28]	0.564 [-.58]
Market 4	3.00 [.42]	-0.94 [-.42]	0.85 [-.19]
Average	1.42 [.30]	-5.36 [-1.05]	0.413 [-.82]

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$; significant differences from zero within treatment according to two-tailed Wilcoxon signed-ranks tests. Numbers in brackets are z-statistics.

Appendix B: Documentation of Data and Experimental Software
(SEE JOURNAL OF FINANCE WEBPAGE OR REQUEST FROM AUTHORS)

B1. Experimental Software

The main program for this experiment is included here in the file "Charness Neugebauer market.ztt."

The program "Warming up quizes.ztt" contains the investment game task for the risk aversion elicitation and the CRT for the acuity elicitation.

The programs "quiz dividend … .ztt" contains comprehension questions on the dividend tables.

The program "quiz forecasting.ztt" contains comprehension questions on the belief elicitation task.

The program "questionnaire.ztq" contains the questionnaire that was used for the debriefing of subjects, which includes indications regarding gender, etc.

To run this program, one would need to obtain the zTree software, which can be obtained following the link; http://www.ztree.uzh.ch/en.html

B2. Five data files (described on the following pages):

(1) End of Period Market DATA.xls,
(2) End of Period Subject DATA.xls,
(3) Double Auction Trading DATA.xls,
(4) Subject Characteristics DATA.xls,
(5) Additional treatments EOP Market DATA.xls

B2.1 Data File (1): End of Period Market DATA.xls

This file provides the data of the perfect-correlation treatment and the no-correlation treatment (including for tables 2, 3A, 3B and 4). In total there were 20 independent cohorts, 2 cohorts in 10 sessions, 4 sessions of the no-correlation treatment (140527_0951, 140529_0952, 140606_1501, 140609_1431) and 6 sessions of the perfect correlation treatment (140520_1450, 140521_0936, 140521_1418, 140522_0905, 140530_0948, 140530_1600). The first column (session) indicates the Day-Month-Year_Time-of-Inititiation. E.g., 140527_0951 indicates data collected in the session that was initiated at 9.51 am on May 27, 2014. In the perfect-correlation treatment, four L-share prices from three cohorts are at or above 1000 cash units during the last four periods of the fourth market (framed cells in the data file). We have excluded the average prices of these periods for these cohorts from figure 2. One L-share price is 2000 cash units in the last period of market 4 in cohort 1 of session 140530_1600. This period was eliminated from all analyses. Owed to a server crash in session 140522_0905, the data of the second market include only 9 periods.

The following table describes the columns of data file (1) in more detail.

Table B2.1 Detailed description - Data File (1): End of Period Market DATA.xls

Column		Label	Description
A	1	Session	Indicates the session by the date of initiation.
B	2	Session number	Sessions 1..6 for the perfect correlation, 7...10 no-correlation treatment; each session involved Group 1 and 2 as two independent cohorts
C	3	Market	Indicates market 1..4 for the four markets of 10 periods
D	4	Period	Indicates period 1..10 for the 10 periods per sequence
E	5	NumPeriods	Number of periods in the sequence
F	6	Correlation	1 indicates perfect correlation, 0 no correlation treatment
G	7	Consensus	1 indicates that the median belief is public, 0 private beliefs only
H	8	InitialCash	Cash endowment was 1200 units per period in single period, per sequence in single asset treatment, where it was carried over between periods.
I	9	InitialStockL	Endowment of L-shares per period in single period, per sequence in single asset treatment, where shares were carried over between periods.
J	10	InitialStockU	Endowment of U-shares per period in single period, per sequence in single asset treatment, where shares were carried over between periods.
K	11	NumContracts	Indicates the total number of submitted limit orders in both cohorts
L	12	NumTradesL	Indicates the total number of L-share transactions in both cohort
M	13	NumTradesU	Indicates the total number of U-share transactions in both cohort
N	14	DIV[1]	Indicates the smallest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
O	15	DIV[2]	Indicates the second smallest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
P	16	DIV[3]	Indicates the second largest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
Q	17	DIV[4]	Indicates the largest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
R	18	DividendL	The recorded L-share dividend of the period
S	19	DividendU	The recorded U-share dividend of the period
T	20	TimeAuction	The time-length of market opening in seconds
U	21	OPriceL	First recorded L-share transaction (open) price in the period for cohort 1. Previous period opening price is recorded in a period without transaction.
V	22	HPriceL	Highest recorded L-share price in the period for cohort 1. Previous period high price is recorded in a period without transaction.
W	23	LPriceL	Lowest recorded L-share price in the period for cohort 1. The previous period low price is recorded in a period without transaction.
X	24	MPriceL	Recorded average L-share price of the period for cohort 1, in equation (4) referred to as S_{Lt}. Missing observation in a period without transaction.
Y	25	CPriceL	Last recorded L-share transaction (close) price in the period for cohort 1. Previous period closing price is recorded in a period without transaction.

	Column	Label	Description
Z	**26**	**OPriceU**	First recorded U-share transaction (open) price in the period for cohort 1. Previous period opening price is recorded in a period without transaction.
AA	**27**	**HPriceU**	Highest recorded U-share price in the period for cohort 1. Previous period high price is recorded in a period without transaction.
AB	**28**	**LPriceU**	Lowest recorded U-share price in the period for cohort 1. Previous period low price is recorded in a period without transaction.
AC	**29**	**MPriceU**	Recorded average U-share price of the period for cohort 1, in equation (4) referred to as S_{Ut}. Missing observation in a period without transaction.
AD	**30**	**CPriceU**	Last recorded U-share transaction (close) price in the period for cohort 1. Previous period closing price is recorded in a period without transaction.
AE	**31**	**OPriceG2L**	First recorded L-share transaction (open) price in the period for cohort 2. Previous period opening price is recorded in a period without transaction.
AF	**32**	**HPriceG2L**	Highest recorded L-share price in the period for cohort 2. Previous period high price is recorded in a period without transaction.
AG	**33**	**LPriceG2L**	Lowest recorded L-share price in the period for cohort 2. Previous period low price is recorded in a period without transaction.
AH	**34**	**CPriceG2L**	Last recorded L-share transaction (close) price in the period for cohort 2. Previous period closing price is recorded in a period without transaction.
AI	**35**	**MPriceG2L**	Recorded average L-share price of the period for cohort 2, in equation (4) referred to as S_{Lt}. Missing observation in a period without transaction.
AJ	**36**	**OPriceG2U**	First recorded U-share transaction (open) price in the period for cohort 2. Previous period opening price is recorded in a period without transaction.
AK	**37**	**HPriceG2U**	Highest recorded U-share price in the period for cohort 2. Previous period high price is recorded in a period without transaction.
AL	**38**	**LPriceG2U**	Lowest recorded U-share price in the period for cohort 2. Previous period low price is recorded in a period without transaction.
AM	**39**	**MPriceG2U**	Recorded average U-share price of the period for cohort 2, in equation (4) referred to as S_{Ut}. Missing observation in a period without transaction.
AN	**40**	**CPriceG2U**	Last recorded U-share transaction (close) price in the period for cohort 2. Previous period closing price is recorded in a period without transaction.
AO	**41**	**FLt**	Expected dividend value during the period of L-share
AP	**42**	**FUt**	Expected dividend value during the period of U-share
AQ	**43**	**Delta**	Difference from parity pricing, Δ_t (equation 4), in the period for cohort 1. Where L-share or U-share prices are missing, delta is missing.
AR	**44**	**DeltaG2**	Difference from parity pricing, Δ_t (equation 4), in the period for cohort 2. Where L-share or U-share prices are missing, delta is missing.
AS	**45**	**|Delta_t|**	Absolute difference from parity pricing in the period for cohort 1, $\|\Delta_t\|$ (equation 5). Missing observation in periods without transaction.
AT	**46**	**|DeltaG2_t|**	Absolute difference from parity pricing in the period for cohort 2, $\|\Delta_t\|$ (equation 5). Missing observation in periods without transaction.
AU	**47**	**DFLt**	Absolute difference of L-share price from fundamentals in the period for cohort 1, DF_{Lt} (equation 6). Missing observation in a period without transaction.
AV	**48**	**DFG2Lt**	Abs. difference of L-share price from fundamentals in the period for cohort 2, DF_{Lt} (equation 6). Missing observation in a period w/o transaction.
AW	**49**	**DFUt**	Absolute difference of U-share price from fundamentals in the period for cohort 1, DF_{Lt} (equation 6). Missing observation in a period without transaction.
AX	**50**	**DFG2Ut**	Absolute difference of U-share price from fundamentals in the period for cohort 2, DF_{Lt} (equation 6). Missing observation in a period without transaction.
AY	**51**	**(SLt-FLt)/FL**	Relative deviation of L-share price from fundamentals in the period for cohort 1, $(S_{Lt} - F_{Lt})/ F_L$ (equation 7). Missing observation in a period w/o transaction.
AZ	**52**	**(SLG2t-LG2t)/FL**	Relative deviation of L-share price from fundamentals in the period for cohort 2, $(S_{Lt} - F_{Lt})/ F_L$ (equation 7). Missing observation in a period w/o transaction.
BA	**53**	**(SUt-FUt)/FU**	Relative deviation of U-share price from fundamentals in the period for cohort 1, $(S_{Ut} - F_{Ut})/ F_U$ (equation 7). Missing observation in a period w/o transaction.
BB	**54**	**(SUG2t-FUG2t)/FU**	Relative deviation of U-share price from fundamentals in the period for cohort 2, $(S_{Ut} - F_{Ut})/ F_U$ (equation 7). Missing observation in a period w/o transaction.

B2.2 Data File (2): End of Period Subject DATA.xls

This file provides the data of the perfect correlation treatment and the no-correlation treatment. This file reveals the subject's purchases and sales during a period, the final share and cash holdings. Negative values indicate short positions. In total there were 174 subjects participating in 20 independent cohorts, 2 cohorts in 10 sessions, 4 sessions of the no-correlation treatment (see section B2.1 for more details).

The following table describes the columns of data file (2) in more detail.

Table B2.2 Detailed description - Data File (2): End of Period Subject DATA.xls

Column		Label	Description
A	1	**Session**	Indicates the session by the date of initiation.
B	2	**Correlation**	1 indicates perfect correlation, 0 no correlation treatment
C	3	**Cohort**	Sessions 1..6 for the perfect correlation, 7...10 no-correlation treatment; each session involved Group 1 and 2 as two independent cohorts
D	4	**Market**	Indicates market 1..4 for the four markets of 10 periods
E	5	**Period**	Indicates period 1..10 for the 10 periods per sequence
F	6	**Subject**	Indicates participant's computer terminal 1..18 in the session
G	7	**Cash**	Record of the subject's cash balance after dividend payment at the end of the period. Negative cash holdings indicate debt.
H	8	**StockL**	Record of the subject's L-share units held at the end of the period. Negative numbers indicate a short position.
I	9	**StockU**	Record of the subject's U-share units held at the end of the period. Negative numbers indicate a short position.
J	10	**NumBuysL**	Number of L-shares the subject purchased in the period.
K	11	**NumSellsL**	Number of L-shares the subject sold in the period.
L	12	**NumBuysU**	Number of U-shares the subject purchased in the period.
M	13	**NumSellsU**	Number of U-shares the subject sold in the period.
N	14	**dZit**	Indicates the largest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
O	15	**ProfitTrader**	Record of the subject's final cash balance, i.e., gains from trading shares and collecting dividends in the 10 periods of the market. Losses are cut at -500 CU.
P	16	**ProfitObserver**	Record of the subject's final earnings from forecasting prices in the 10 periods of the market. Maximum gains are 600, deviations are punished; minimum is 0.
Q	17	**ProfitTraderObserver**	Total earnings from market participation during a market sequence; sum of ProfitTrader and ProfitObserver. One market was chosen for payment by the roll of a die. Subjects were rewarded with an envelope containing this amount plus show-up fee and gains from warming-up-quizzes like CRT.

B2.3 Data File (3): Double Auction Trading DATA.xls

This file provides the trading data for the perfect correlation treatment and the no-correlation treatment (including for tables 3C and 6). There were 20 independent cohorts as described in B2.1, 2 cohorts in 10 sessions. Subjects submitted limit orders for one unit; an ask indicates a proposed selling price, a bid indicates a proposed buying price. Transactions completed when one subject hit the immediate-buy button or the immediate-sell button confirming the best outstanding ask or bid. Only one share of asset could be transacted at a time. Each line in this data file corresponds to a limit order submitted for one share of asset. When a transaction completed, all outstanding bids of the buyer and all outstanding asks of the seller were cancelled (for both share classes). The transaction partners sellerID and BuyerID were recorded. Cancelled limit orders are indicated in the data file by a -2, and outstanding orders at the end of the period by -1. Trading was recorded in two independent tables, one for the L-share and one for the U-share. The order-book including the best outstanding bids and asks were not recorded, but were computed afterwards by the authors taking account of the chronological order of limit orders in the two tables and the recorded submission and transaction times.

The following table describes the columns of data file (3) in more detail.

Table B2.3 Detailed description - Data File (3): Double Auction Trading DATA.xls

Column		Label	Description
A	1	**Session**	Indicates the session by the date of initiation.
B	2	**Session number**	Sessions 1..6 for the perfect correlation, 7...10 no-correlation treatment; each session involved two independent cohorts
C	3	**Market**	Indicates market 1..4 for the four markets of 10 periods
D	4	**Cohort**	Indicates the independent cohort that interacted in four markets of 10 periods. Cohorts 1..12 are data of the perfect correlation, 13..20 of the no-correlation treatment.
E	5	**Period**	Indicates period 1..10 for the 10 periods per sequence
F	6	**L**	1 indicates submission in L-share market, 0 indicates U-share market
G	7	**FV**	Dividend value of share in market in the submission period
H	8	**SellerID**	Subject ID 1...18 of the seller / submitter of an offer to sell the share. If negative, there is no seller; -1 indicates the limit order remains open until the end; -2 indicates the limit order is closed automatically amid a purchase / transaction of the limit order submitter.
I	9	**BuyerID**	Subject ID 1...18 of the buyer / submitter of a bid to purchase the share. If negative, there is no buyer; -1 indicates the limit order remains open until the end; -2 indicates the limit order is closed automatically amid a sale / transaction of the limit order submitter.
J	10	**MakerID**	Subject ID 1...18 of the submitter of the limit order
K	11	**Bid**	Recorded bidding (limit) price - price in cash units offered to buy the share
L	12	**Ask**	Recorded asking (limit) price - price in cash units requested to sell the share
M	13	**#ask**	Number of asks outstanding in same cohort / market / share at the time of asking order submission
N	14	**#bid**	Number of asks outstanding in same cohort / market / share at the time of bidding order submission
O	15	**LOL_ID**	L-Share limit order identifier assigned in chronological order; 1 indicates first limit order, 2 indicates the second, ... the highest number indicates the total number of limit orders recorded in the period for the L-share (for both cohorts)
P	16	**LOU_ID**	U-Share limit order identifier assigned in chronological order; 1 indicates first limit order, 2 indicates the second, ... the highest number indicates the total number of limit orders recorded in the period for the U-share (for both cohorts)
Q	17	**TPrice**	Recorded transaction price; if the limit order is accepted via immediate buy/sell, the transaction is recorded at the bidding or asking price of the limit order.

	Column	Label	Description
R	**18**	**TL_ID**	L-share transaction contract identifier assigned in chronological order; 1 indicates the first transaction, 2 the second one, ... the highest number indicates the number of L-share transactions in the period (for both cohorts). Missing observation w/o transaction.
S	**19**	**TU_ID**	U-share transaction contract identifier assigned in chronological order; 1 indicates the first transaction, 2 the second one, ... the highest number indicates the number of U-share transactions in the period (for both cohorts). Missing observation w/o transaction.
T	**20**	**SubmTime**	Recorded time of limit order submission in (remaining) seconds before market close.
U	**21**	**TTime**	Recorded time of transaction in (remaining) seconds before market close.
V	**22**	**CTime**	Computed time of closing the outstanding limit order.
W	**23**	**BBidL**	Computed best L-share bid outstanding in cohort at time of limit order submission.
X	**24**	**BAskL**	Computed best L-share ask outstanding in cohort at time of limit order submission.
Y	**25**	**BBidU**	Computed best U-share bid outstanding in cohort at time of limit order submission.
Z	**26**	**BAskU**	Computed best U-share ask outstanding in cohort at time of limit order submission.
AA	**27**	**SameAsset ArbitrageOpp**	Computed dummy that takes value 1 if an arbitrage opportunities within the same asset exist, that is, when the best bid exceeds the best ask; 0 otherwise.
AB	**28**	**DiscLMU**	Computed size of cross asset discrepancy, L minus U; exploitable (expected) gain by going long the L-share at the best ask and short the U-share at the best bid
AC	**29**	**DiscUML**	Computed size of cross asset discrepancy, U minus L; exploitable (expected) gain by going long the U-share at the best ask and short the L-share at the best bid
AD	**30**	**DiscDummy**	Computed dummy of outstanding discrepant limit order that takes value 1 if DLO exists, 0 otherwise
AE	**31**	**DiscInitiation**	Computed dummy of discrepant limit order initiation (relevant for measure of equation 8) that takes value 1 if DLO is initiated, 0 otherwise
AF	**32**	**DiscSizeInit**	Size of discrepant limit order at initiation; DiscLMU or DiscUML at initiation
AG	**33**	**DiscMaker**	Computed subject ID of DLO submitter (relevant for measure of equation 8)
AH	**33**	**TakerID**	Computed subject ID of DLO (relevant for measure of equation 8)
AI	**33**	**HighExpReturnShare ClosesFirstDummy**	Computed dummy that takes value 1 if the high expected return trade closes first when a discrepant limit order is initiated (hypothesis 5A); 0 otherwise.

B2.4 Data File (4): Subject Characteristics DATA.xls

This file provides the data of the subject characteristics by subject and averages by cohort for the perfect correlation treatment and the no-correlation treatment (including for tables 5A/B). There were 20 independent cohorts, 2 cohorts in 10 sessions, 6 sessions of the perfect correlation treatment (140520_1450, 140521_0936, 140521_1418, 140522_0905, 140530_0948, 140530_1600; names indicate initiation time (see B2.1)) and 4 sessions of the no-correlation treatment (140527_0951, 140529_0952, 140606_1501, 140609_1431).

The following table describes the columns of data file (4) in more detail.

Table B2.4 Detailed description - Data File (4): Subject Characteristics DATA.xls

Column		Label	Description
			Recorded characteristics of each subject
A	1	**Session**	Indicates the session by the date of initiation.
B	2	**Cohort**	There were two independent cohorts per session. Cohorts 1..12 participated in the perfect correlation, cohorts 13...20 in the no-correlation treatment
C	3	**Subject**	ID of the participant in a session. Each session was designed for 18 subjects (9 per cohort). Three cohorts had 7 subjects.
D	4	**AvgPayoff**	Average trading gains of the subject in the four market sequences
E	5	**CRT-score**	Measure of acuity which indicates the subject's number of correct answers {0, 1, 2, 3} in the Cognitive Reflection Task.
F	6	**FEMALE**	Is the self-reported gender dummy; it takes 1 for female, 0 for male subjects
G	7	**RiskAversion**	Measure of risk aversion elicited through the investment game task; it is the number {0, 1, ... , 100} of tokens the subject invested in the safe asset.
H	8	**AGE**	Self-reported measure of participant's age on the day of the experiment.
I	9	**THEO**	Self-reported measure of religiousness {0 secularist, 1 moderately religious, 2 devout religious}.
J	10	**DLO**	Indicates the number of discrepant limit orders submitted by the subject
K	11	**RFDLO_i**	Measure (equation 8) subject's rel frequency of DLO submission
L	12	**APTITUDE**	Measure (1-RFDLO_i)
M	13	**Nationality**	Indicates the self-reported origin of the subject
N	14	**Studies**	Indicates the self-reported department/field of studies
			Average characteristics of each cohort
O	15	**sessionID**	Indicates the session by the date of the session.
P	16	**cohortID**	The data involve 20 independent cohorts. Cohorts 1..12 participated in the perfect correlation treatment, cohorts 13...20 in the no-correlation treatment
Q	17	**treatmentID**	Identifies treatment; 0 no-correlation, 1 perfect correlation treatment
R	18	**CRT-share**	Average CRT-score in cohort
S	19	**CRTxTreatDummy**	CRT-share x treatmentID
T	20	**FemShare**	Average share of females in cohort
U	21	**AvgRiskAversion**	Average measure of risk aversion (number of tokens in safe asset) in cohort
V	22	**AvgAGE**	Average reported age in cohort
W	23	**DiffParity**	Measure (equation 4) - deviation from parity pricing
X	24	**DF**	Measure (equation 6) - relative absolute deviation from fundamentals
Y	25	**PD**	Measure (equation 5) - cross-asset price discrepancy
Z	26	**SumLO**	Total limit orders submitted in cohort
AA	27	**SumDLO**	Total discrepant limit orders submitted in cohort
AB	28	**RFDLO**	Measure (equation 8) relative frequency of discrepant limit order submission
AC	29	**RD_L-Share**	Measure (equation 7) relative deviation for L-share
AD	30	**RD_U-share**	Measure (equation 7) relative deviation for U-share
AE	31	**RD_U-RD_L**	RD difference between U-share and L-share

B2.5 Data File (5): Additional treatments EOP Market DATA.xls

This file provides the data of the single-asset and the single-period treatments. In total there were 12 sessions; 161114_1454, 161115_0934, 161115_1507, 161116_1011, 161116_1658, 161117_0900, 161117_1435, 161118_0900, 170117_1457, 170118_0802, 170118_1443, 170119_1055. The session id indicates DayMonthYear_TimeOfInitiation, e.g., 161114_1454 indicates data collected in the session that was initiated at 2.54 pm on the 14th of November, 2016. In sessions (2), (3), (7), (8) there was a software bug (the dividend on the U-share was the maximum possible dividend always), resulting in invalid data. Data file (5) contains the valid data only. Server problems occurred in sessions 161114_1454 (implying a loss of data for the single asset treatment in period 10 of the second sequence), 170118_1443 (requiring a restart of the single period treatment after five periods), 170119_1055 (requiring a restart of the single period treatment after six periods).

The following table describes the columns of data file (5) in more detail.

Table B2.5 Detailed description - Data File (5): Additional treatments EOP DATA.xls

Column		Label	Description
A	1	**Session**	Indicates the session by the date of the session.
B	2	**Run**	ID 1..3 for three market sequences of 10 periods, including one sequence of the single-period market, two sequences of the single-asset market
C	3	**Period**	ID 1..10 for the 10 periods per sequence
D	4	**NumPeriods**	Number of periods in the sequence
E	5	**Correlation**	1 indicates perfect correlation, 0 no correlation treatment
F	6	**SinglePeriodTreatment**	1 indicates single period, 0 single asset treatment
G	7	**SingleAssetTreatment**	1 indicates L-share, 2 U-share treatment, 0 single period treatment
H	8	**InitialCash**	Cash endowment was 1200 units per period in single period, per sequence in single asset treatment, where it was carried over between periods.
I	9	**InitialStockL**	Endowment of L-shares per period in single period, per sequence in single asset treatment, where shares were carried over between periods.
J	10	**InitialStockU**	Endowment of U-shares per period in single period, per sequence in single asset treatment, where shares were carried over between periods.
K	11	**numContracts**	Indicates the total number of submitted limit orders in both cohorts
L	12	**numTradesG2L**	Indicates the number of L-share transactions in cohort 2
M	13	**numTradesG2U**	Indicates the number of U-share transactions in cohort 2
N	14	**numTradesL**	Indicates the number of L-share transactions in cohort 1
O	15	**numTradesU**	Indicates the number of U-share transactions in cohort 1
P	16	**DIV[1]**	Indicates the smallest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
Q	17	**DIV[2]**	Indicates the second smallest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
R	18	**DIV[3]**	Indicates the second largest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
S	19	**DIV[4]**	Indicates the largest potential dividend in a period. For the single-period treatment, only the potential L-share dividend is indicated.
T	20	**DividendL**	The recorded L-share dividend of the period
U	21	**DividendU**	The recorded U-share dividend of the period
V	22	**TimeAuction**	The time-length of market opening in seconds
W	23	**OPriceL**	First recorded L-share transaction (open) price in the period for cohort 1
X	24	**HPriceL**	Highest recorded L-share price in the period for cohort 1
Y	25	**LPriceL**	Lowest recorded L-share price in the period for cohort 1

Column		Label	Description
Z	**26**	**MPriceL**	Recorded average L-share price of the period for cohort 1, in equation (4) referred to as S_{Lt}. Missing observation in a period without transaction.
AA	**27**	**CPriceL**	Last recorded L-share transaction (close) price in the period for cohort 1
AB	**28**	**OPriceU**	First recorded U-share transaction (open) price in the period for cohort 1
AC	**29**	**HPriceU**	Highest recorded U-share price in the period for cohort 1
AD	**30**	**LPriceU**	Lowest recorded U-share price in the period for cohort 1
AE	**31**	**MPriceU**	Recorded average U-share price of the period for cohort 1, in equation (4) referred to as S_{Ut}. Missing observation in a period without transaction.
AF	**32**	**CPriceU**	Last recorded U-share transaction (close) price in the period for cohort 1
AG	**33**	**OPriceG2L**	First recorded L-share transaction (open) price in the period for cohort 2
AH	**34**	**HPriceG2L**	Highest recorded L-share price in the period for cohort 2
AI	**35**	**LPriceG2L**	Lowest recorded L-share price in the period for cohort 2
AJ	**36**	**CPriceG2L**	Last recorded L-share transaction (close) price in the period for cohort 2
AK	**37**	**MPriceG2L**	Recorded average L-share price of the period for cohort 2, in equation (4) referred to as S_{Lt}. Missing observation in a period without transaction.
AL	**38**	**OPriceG2U**	First recorded U-share transaction (open) price in the period for cohort 2
AM	**39**	**HPriceG2U**	Highest recorded U-share price in the period for cohort 2
AN	**40**	**LPriceG2U**	Lowest recorded U-share price in the period for cohort 2
AO	**41**	**MPriceG2U**	Recorded average U-share price of the period for cohort 2, in equation (4) referred to as S_{Ut}
AP	**42**	**CPriceG2U**	Last recorded U-share transaction (close) price in the period for cohort 2
AQ	**43**	**FLt**	Expected dividend value during the period of L-share
AR	**44**	**FUt**	Expected dividend value during the period of U-share
AS	**45**	**Delta**	Difference from parity pricing (equation 4) in cohort 1
AT	**46**	**DeltaG2**	Difference from parity pricing (equation 4) in cohort 2
AU	**47**	**(SLt-FLt)/FL**	Relative deviation of L-share price from fundamentals in the period for cohort 1, $(S_{Lt} - F_{Lt})/ F_L$ (equation 7). Missing observation in a period w/o transaction.
AV	**48**	**(SLG2t-LG2t)/FL**	Relative deviation of L-share price from fundamentals in the period for cohort 2, $(S_{Lt} - F_{Lt})/ F_L$ (equation 7). Missing observation in a period w/o transaction.
AW	**49**	**(SUt-FUt)/FU**	Relative deviation of U-share price from fundamentals in the period for cohort 1, $(S_{Ut} - F_{Ut})/ F_U$ (equation 7). Missing observation in a period w/o transaction.
AX	**50**	**(SUG2t-FUG2t)/FU**	Relative deviation of U-share price from fundamentals in the period for cohort 2, $(S_{Ut} - F_{Ut})/ F_U$ (equation 7). Missing observation in a period w/o transaction.

Appendix C: Experimental Instructions (SEE JOURNAL OF FINANCE WEBPAGE OR REQUEST FROM AUTHORS)

Experimental Instructions (main experiment)

- Perfect-correlation treatment
- No-correlation treatment

First part of the experiment (additional experiments)

- Perfect-correlation Single-period, first market round
- No-correlation Single-period, first market round
- Single-asset expected dividend 24, first 2 market rounds
- Single-asset expected dividend 48, first 2 market rounds

Further instructions (additional experiments)

- Further Instructions (perfect-correlation single-period)
- Further Instructions (no-correlation single-period)
- Further Instructions single-asset expected dividend 24, market rounds 2, 3

Further Instructions single-asset expected dividend 48, market rounds 2, 3