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Abstract

We experimentally manipulate agents' information regarding the rationality of others in a setting in which previous studies have found irrationality to be present, namely the asset market experiments introduced by Smith, Suchanek, and Williams (*Econometrica*, 1988). Recent studies suggest that mispricing in such markets may be an artefact of confusion, which can be reduced by training subjects to understand the diminishing fundamental value. We reconsider this view, and argue that when it is made public knowledge that training has occurred, this may also reduce uncertainty over the behavior of others and facilitate the formation of common expectations. Our design disentangles the direct effect of training from the indirect effect of its public knowledge, and our results indicate a distinct effect of public knowledge over and above that of training alone.

JEL Classification: C92, D84, G12.

Keywords: asset market experiment, mispricing, confusion, common expectations.

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In a wide range of decision problems, the optimal course of action depends critically on agents' expectations regarding the behavior – and therefore implicitly the rationality – of others. This is the case not only in many applied problems of business strategy, but also in the corpus of theory that economists have developed to model such interactions. As is well known, standard solution concepts such as rationalizability and backward induction demand high levels of mutual knowledge of rationality, resulting in stark equilibrium predictions that frequently fail in the experimental laboratory (Nagel, 1995; McKelvey and Palfrey, 1992). Yet, what is less commonly acknowledged is that these settings arguably also stretch the limits of the experimental method itself. For while an experimenter can control such features as the set of players, the strategies at their disposal and resultant material payoffs, it is far more difficult for the experimenter to credibly control the *epistemic* conditions that are also required for equilibrium predictions to obtain – in particular the beliefs that players hold regarding the rationality of their counterparts. Nonetheless, when experimental findings fail to confirm equilibrium predictions, it is tempting to conclude that this might reflect some failure of rationality itself rather than the common knowledge thereof.

In this paper, we reconsider these issues in the context of the mispricing observed in the asset market experiments introduced by Smith, Suchanek, and Williams (1988, hereinafter SSW). In particular, we reexamine the recent suggestion that this mispricing is due to confusion, and can be ameliorated by training subjects to understand fundamental value (FV). We reassert the importance of subjects' expectations, by proposing that the coordination of expectations may also be facilitated when it is made public knowledge that such training has taken place. To test this conjecture, we manipulate whether or not it is public knowledge that all traders in a market have undergone training. We can thus distinguish the direct effect of training from the indirect effect of its public knowledge, and we find that there is a distinct effect of public knowledge in addition to that of training itself.

The phenomenon of price "bubbles and crashes" in SSW-style asset market experiments was for many years considered a paradox or anomaly. Over the quarter century that followed publication of SSW, a large body of research sought to identify and eliminate the sources of this mispricing, with only limited success.[1] SSW's original interpretation of their discovery was that differences between

[1] King, Smith, Williams, and van Boening (1993), van Boening, Williams, and LaMaster (1993), Porter and Smith (1995), and Haruvy and Noussair (2006) manipulate aspects of the rules of the institutions that govern exchange. Porter and Smith (1995), Smith, van Boening, and Wellford (2000), Noussair, Robin, and Ruffieux (2001), and

price and FV "may be due to a lack of common, not irrational, expectations" (p. 1120), and that "it is the failure of the assumption of common expectations, *not backward induction incompetence by subject agents* that explains bubbles" (p. 1148, emphasis added). That is, although the dividend structure of the asset was made public knowledge by the experimenter, each subject might still have been uncertain as to how others would use that information. However, in SSW's interpretation, it was not necessary that subjects actually failed to comprehend the information itself.

More recently, a new interpretation has proposed that mispricing in SSW markets is largely a product of confusion over the FV process, which declines over time due to the finite life of the dividend-paying experimental asset (and may thus be inconsistent with subjects' homegrown expectations derived from real-world assets). Consistent with this view, several recent studies find that when care is taken to train subjects to correctly understand declining FV, mispricing in SSW markets is substantially diminished.[2] Huber and Kirchler (2012, p. 89) summarize these results by stating that "all bubble reducing factors have one common feature: they allow subjects to understand the non-intuitive declining FV-process of the SSW-model better and thus reduce subjects' confusion".[3]

The first conclusive evidence of confusion was provided by Lei, Noussair, and Plott (2001, hereinafter LNP). They made explicit the implication of SSW's conjecture that mispricing arises from uncertainty over the behavior of others – namely that some subjects must doubt the rationality of others, and thus perceive an opportunity for speculation. To test this, LNP designed treatments in which speculation was not possible (by prohibiting subjects in the role of buyers from reselling, and subjects in the role of sellers from repurchasing), and nonetheless observed many transactions

Hussam, Porter, and Smith (2008) manipulate aspects of the dividend process of the experimental asset. James and Isaac (2000) study the effect of incentives; Dufwenberg, Lindqvist, and Moore (2005) study the effect of experience; Haruvy, Lahav, and Noussair (2007) elicit subjects' price predictions; and Cheung and Palan (2012) study the effect of group decision-making. Recent surveys of this literature include Porter and Smith (2008), and Palan (2013).

[2]Noussair and Tucker (2006) sequentially open a complete set of futures markets, in reverse order of maturity, prior to opening the spot market; they state explicitly that this is intended to facilitate backward-induction reasoning over the FV. Lei and Vesely (2009) introduce a pre-market phase in which subjects passively experience a flow of dividends. After this they ask subjects to state, for each period, the value of an asset that pays dividends in every remaining period of its life. Kirchler, Huber, and Stöckl (2012) introduce a new framing ("stocks of a depletable gold mine") intended to call to mind a declining FV. Huber and Kirchler (2012) present FV information in a graph instead of a table, and ask subjects to state their estimate of the FV before the start of each period. Each of these protocols is found to produce patterns of mispricing that are less pronounced than is typical in SSW-style markets.

[3]Thus, for example, the well-known result that mispricing in SSW markets is diminished with repetition – which SSW interpreted to show that subjects came to form common expectations by learning the behavior of others through experience – is reinterpreted to indicate that subjects were instead learning to understand FV. It follows that an appropriate training protocol could serve as a substitute for such experience (Lei and Vesely 2009, p. 258).

at prices that were as a consequence certain to be unprofitable. From this, LNP were careful to conclude that "the lack of common knowledge of the rationality of market participants ... can be ruled out as being the *only* cause of the bubble phenomenon" (p. 834, emphasis in original).[4]

In this paper, we reaffirm the role of common expectations, as first emphasized by SSW, in the wake of the finding of confusion in SSW markets. Since LNP establish that doubts over the behavior of others are well-founded, it follows that protocols that facilitate common expectations cannot wholly substitute for ones that address the underlying confusion. However, it does not follow that the two may not be complements. Nonetheless, we submit that all recent training protocols in fact share a second common feature – namely the fact that *it is public knowledge that all subjects in the market have been jointly exposed to the protocol.*

We suggest that in making the training of declining FV public knowledge, these recent studies may also have the effect of reducing uncertainty over the behavior of others and resolving the problem of coordinating subjects' price expectations – and that this may in itself have contributed to the finding of diminished mispricing.[5] To evaluate this conjecture, we report new experiments in which we manipulate both whether or not subjects are trained to understand FV, and whether or not it is public knowledge that all subjects in the market have undergone this training.[6] Through this design, we are able to disentangle the direct effect of training in reducing confusion from the indirect effect of its public knowledge in facilitating common expectations.

Our results replicate the finding of previous studies that find that when all subjects are trained to understand diminishing FV, and this is made public knowledge, mispricing is significantly less than when training has not occurred. We introduce a new treatment in which all subjects in the market have been trained, but this is not public knowledge, and find that this results in an intermediate

[4] Smith (2010, p. 6) acknowledges that SSW's original interpretation of their finding was falsified by the LNP result; however he does not wholeheartedly endorse the notion that the subjects were "confused".

[5] Noussair and Tucker (2006, p. 169) acknowledge that their futures market protocol cannot discriminate between the effects of coordinating expectations and reducing confusion. Lei and Vesely (2009, p. 256) are less circumspect, asserting that "individual rationality induced in the pre-market phase was so profound that uncertainty about the behaviour of others ... never became strong enough to divert market prices away from the fundamental values". This overlooks the possibility that behavioral uncertainty might itself have been diminished as a byproduct of the protocol.

[6] We speak of *public knowledge of training* to make clear that we do not claim that this suffices to establish *common knowledge of rationality*. This is because the formal concept of common knowledge involves higher-order beliefs, about which we have no direct evidence. Nonetheless, since LNP establish the presence of *actual irrationality in the absence of training*, we assert that common knowledge of rationality is impossible when i) confusion has been reduced through training, however ii) this is not public knowledge.

level of mispricing. We interpret this to indicate that there is a distinct effect of public knowledge over and above that of training alone, and we submit that this possibility may have been neglected in the recent literature on confusion.[7]

Our paper is organized as follows. Section 1 outlines our design, including details of our training and public knowledge manipulations. Section 2 presents our results, and Section 3 concludes.

1 Design

1.1 Market environment

In each market, ten subjects trade shares of a dividend-paying asset in exchange for experimental currency units (ECU) in a computerized double auction over fifteen four-minute trading periods. The distribution of initial endowments is summarized in Appendix A; valued at FV, each subject has the same initial wealth. After each period, each share pays a common dividend which, following SSW's classic "Design 4" parameters, takes values of 0, 8, 28, or 60 ECU, each with equal probability. After the fifteenth period, shares expire without any terminal value. The FV of a share is thus given by the product of its expected dividend per period (24 ECU) and the number of dividends remaining. In particular, the FV is 360 in period one, and declines by 24 in each subsequent period. We follow standard practice in the SSW literature by making FV information public knowledge in the form of an "average holding value table" which is contained within the instructions.

1.2 Training protocol

Our protocol to train subjects in the FV process consists of two sets of control questions – one framed from the perspective of buying a share, and the other framed from the perspective of selling. We include fifteen questions in each frame, ordered from period fifteen to period one. In the buyer frame subjects were asked, for $t = \{15, 14, \ldots, 1\}$:

[7] We do not contest the view, also advanced in the recent literature, that the SSW design has some unusual features that may lack external validity (Oechssler 2010, Kirchler, Huber, and Stöckl 2012). In addition to declining FV, these include a finite horizon, increasing cash-to-asset ratio, and high dividend yields.

> Suppose that you buy one share in period t and that you keep it until the end of the market (i.e. until period 15). What is the average total dividend that you will receive from this share?

Similarly, in the seller frame subjects were asked, for $t = \{15, 14, \ldots, 1\}$:

> Suppose that you sell one share in period t and that you do not buy it back. What is the average total dividend that you give up on this share?

We require subjects to answer both sets of questions, thereby effectively requiring them to enter the FV values from the average holding value table twice, from the bottom up. In each of the sessions in which subjects were required to answer these questions, the experiment did not commence until all of the subjects who were required to do so had answered all of the questions correctly.[8]

Prior to constructing these control questions, we conducted a thorough search of the literature for appropriate precedents, and identified very few. Given that control questions are typically only included in working papers and do not find their way into final publications, we do not claim that they are seldom used, but it would appear that their use is not universal. Moreover, many of the examples we identified relate to features of the market institution that are novel to a specific paper, for example, the futures markets of Noussair and Tucker (2006), as opposed to the standard SSW environment itself. In short, the existing literature provides little clear guidance as to what to include in an appropriate set of control questions.

Since the purpose of our training protocol was to redress confusion over the declining value of shares, our control questions naturally place a heavy emphasis on checking subjects' understanding of the FV process.[9] Because of this, we acknowledge that our results would not necessarily be invariant to the content of the control questions, and in particular we do not claim that our results would hold under some other protocol that did not stress FV in this way. For example, one of the referees

[8] Unfortunately, we do not have data on subjects' performance in answering the control questions as we did not record the number of attempts or the time individual subjects needed to successfully answer them.

[9] In fact, by presenting the questions in reverse order from period fifteen to period one our protocol was actually intended to highlight the *backward induction* of FV, as opposed to its declining value *per se*. In this respect, it was modeled upon the futures market protocol of Noussair and Tucker (2006).

suggests that our buyer control questions might draw subjects' attention to a buy-and-hold strategy. One could equally well conceive of an alternative set of questions that might highlight the possibility of a speculative strategy, and we would certainly not expect this to yield the same results. However, such a set of questions would not serve our original purpose of reducing confusion over FV.

At the time that we developed our procedure, we were unaware of several of the more recent training protocols described in footnote 2. Nonetheless, we consider the various procedures to be comparable, in that they all seek to reinforce subjects' understanding of the FV information in the instructions.

1.3 Treatments

We operated two markets in each session, for a total of twenty subjects. Our design consists of four treatments, which differ in whether or not subjects were required to complete the training task before the experiment could begin, and whether or not this was public knowledge. In the two treatments in which subjects answered control questions, it was always the case that all ten subjects in a market were required to do so; these treatments therefore differ only in whether or not this was public knowledge. We collected a total of six observations (markets) in each treatment. We thus have the same number of markets in each treatment as the recent papers by Kirchler, Huber and Stöckl (2012) and Huber and Kirchler (2012), and a larger number than the seminal paper by LNP.

In the Public Knowledge (PK) treatment, all subjects were required to successfully complete the training task, and this was public knowledge. Subjects were informed that the experiment would not begin until all twenty subjects in the session had correctly answered all of the questions.

To obtain the treatments we refer to as NPK and WAIT, we informed all twenty subjects in a session that some of them would be asked to answer some control questions, and that those subjects would have to answer all of the questions correctly before the experiment could begin. The remaining subjects would not be asked any questions, and would simply wait for the experiment to begin.

Of the twenty subjects in these sessions, we required ten to answer the full set of questions. Through a message on their computer screens, we informed these subjects that exactly ten of the subjects in the session would be required to answer the questions. What they were not told is that all ten would be grouped together to trade in the same market. This market thus consisted of ten subjects who had all completed the training task successfully but who did not know that all others in the market had also done so. We refer to this treatment as Not Public Knowledge (NPK).

As a byproduct of NPK we also had ten subjects in these sessions who did not complete the training task and were simply required to wait for the others to finish. These ten subjects were grouped together to make up the second market in the session. Through a message on their computer screens, we informed these subjects that when the experiment began, none of the subjects in their market would have answered any questions.[10] We refer to this treatment as WAIT.

Finally, in our BASE treatment none of the subjects in the session were required to complete the training task, and they did not have to wait for others to do so before the experiment could begin.

Thus, to reiterate the key feature of our design: In both treatments PK and NPK, all subjects in the market were trained to understand the declining FV process by requiring them to correctly answer the control questions; however only in treatment PK was this made public knowledge.

1.4 Procedures

We conducted our experiments at the University of Copenhagen between October 2009 and June 2010. No subject had taken part in any previous asset market experiment. We recruited subjects using ORSEE (Greiner 2004), and the experiment was programmed in z-Tree (Fischbacher 2007).

At the start of each session, we distributed and read aloud the first part of the instructions dealing with the mechanics of using the computer interface to make price offers and to buy and sell shares.[11] This was followed by a ten-minute practice period, which did not count toward subjects' earnings. To minimize any anchoring effect of the practice prices, subjects completed the practice task before being told the dividend structure of the asset or how their earnings would be determined.

We next circulated and read aloud the remainder of the instructions, dealing with the dividends,

[10] We did this to control these subjects' expectations with respect to the training history of their counterparts, and thereby enable us to test for the pure effect of waiting time in WAIT compared to BASE.

[11] See the Online Appendix for the full instructions, which also include a screen shot of the double auction interface.

average holding value table and calculation of earnings. Following this, some subjects were required to complete the training task as appropriate to the treatment (as detailed above).

Upon conclusion of the experiment, subjects completed a questionnaire consisting of some basic demographic items, the three-item Cognitive Reflection Test (Frederick 2005), and a ten-item test of financial literacy derived from van Rooij, Lusardi, and Alessie (2011). Sessions lasted up to 2.5 hours, and the average earnings were DKK 239 (approximately USD 48 as of November 2009).

1.5 Hypotheses

Our design allows us to tease apart the effects of requiring subjects to wait before the experiment can begin (under treatment WAIT), of requiring all subjects in the market to successfully complete the training protocol when this is not public knowledge (under treatment NPK), and of making it public knowledge that all subjects successfully completed the training (under treatment PK). The existing literature indicates that there is likely to be substantial mispricing under BASE, and considerably less under PK. By examining the decomposition of this difference, as seen through the intermediate treatments WAIT and NPK, we expect to be able to shed light upon the mechanism through which training results in diminished mispricing.

Our first testable hypothesis concerns the effect of requiring our WAIT subjects to wait for others to complete the training task before the experiment can begin. It is possible that simply giving these subjects more time in which to think through the information in the instructions might itself reduce mispricing, even in the absence of any training. We formulate this hypothesis in light of the evidence for a positive effect of time to think on decision quality, as documented in, e.g. Kocher and Sutter (2006) or Russo and Shoemaker (1990).

Hypothesis 1: Mispricing is less severe under WAIT compared to BASE.

Our second hypothesis states that we expect to replicate the recent finding that mispricing is reduced when all subjects have been trained to understand FV, and this fact is made public knowledge.

Hypothesis 2: Mispricing is less severe under PK compared to WAIT and BASE.

Table 1: Measures of mispricing and overvaluation

Measure	Definition
Relative Absolute Deviation	$RAD = \frac{1}{T}\sum_t \lvert\bar{P}_t - f_t\rvert \,/\, \lvert\bar{f}\rvert$
Relative Deviation	$RD = \frac{1}{T}\sum_t (\bar{P}_t - f_t) \,/\, \lvert\bar{f}\rvert$

Note: T = total number of trading periods; $\bar{P}_t$ = mean transaction price in period t; f_t = fundamental value in period t; $\bar{f}$ = mean fundamental value over the life of the asset.

Our next two hypotheses are concerned with disentangling the effect of training from that of its public knowledge. Insofar as there is a direct effect of training *per se*, we would expect this to be observed when it is not public knowledge that all subjects successfully completed the training.

Hypothesis 3: Mispricing is less severe under NPK compared to WAIT and BASE.

On the other hand, insofar as there is an indirect effect of public knowledge, this would only be evident in the PK treatment and not in NPK. This motivates our final hypothesis.

Hypothesis 4: Mispricing is less severe under PK compared to NPK.

1.6 Measures of mispricing and overvaluation

We follow the recent literature in reporting the measures of *Relative Absolute Deviation* (RAD) for mispricing, and *Relative Deviation* (RD) for overvaluation, as introduced by Stöckl, Huber, and Kirchler (2010). The formal definitions of these measures are stated in Table 1, while Appendix Table B2 reports values of these measures for each of our markets.[12] RAD measures the average *absolute* deviation of price from FV, and may thus be interpreted as a measure of the overall severity of mispricing without regard for sign. On the other hand, RD measures the average *direction* of price deviations, permitting periods of over and undervaluation to cancel out.

Since we express our hypotheses in terms of mispricing, our preferred measure is RAD: if the effect

[12]In addition, we follow Kirchler, Huber, and Stöckl (2012) in also reporting a variety of other measures from the earlier SSW literature. These additional measures are defined and reported in Appendix Table B1, and their values are also reported in Appendix Table B2.

of our treatments were to reduce the incidence of both over and undervaluation, this would be clearly evident in the form of a lower RAD, but the same would not necessarily be true of RD.

2 Results

Figure 1 provides an overview of the period-wise median price trajectories in each of the individual markets (gray lines) together with the corresponding treatment means (thick black lines), with each treatment depicted in a separate panel.[13] For comparison, the lower stepped line depicts the time path of FV while the upper, dashed, stepped line represents the maximum dividend value of a share (in the event that the maximum dividend of 60 is realized in every remaining period).

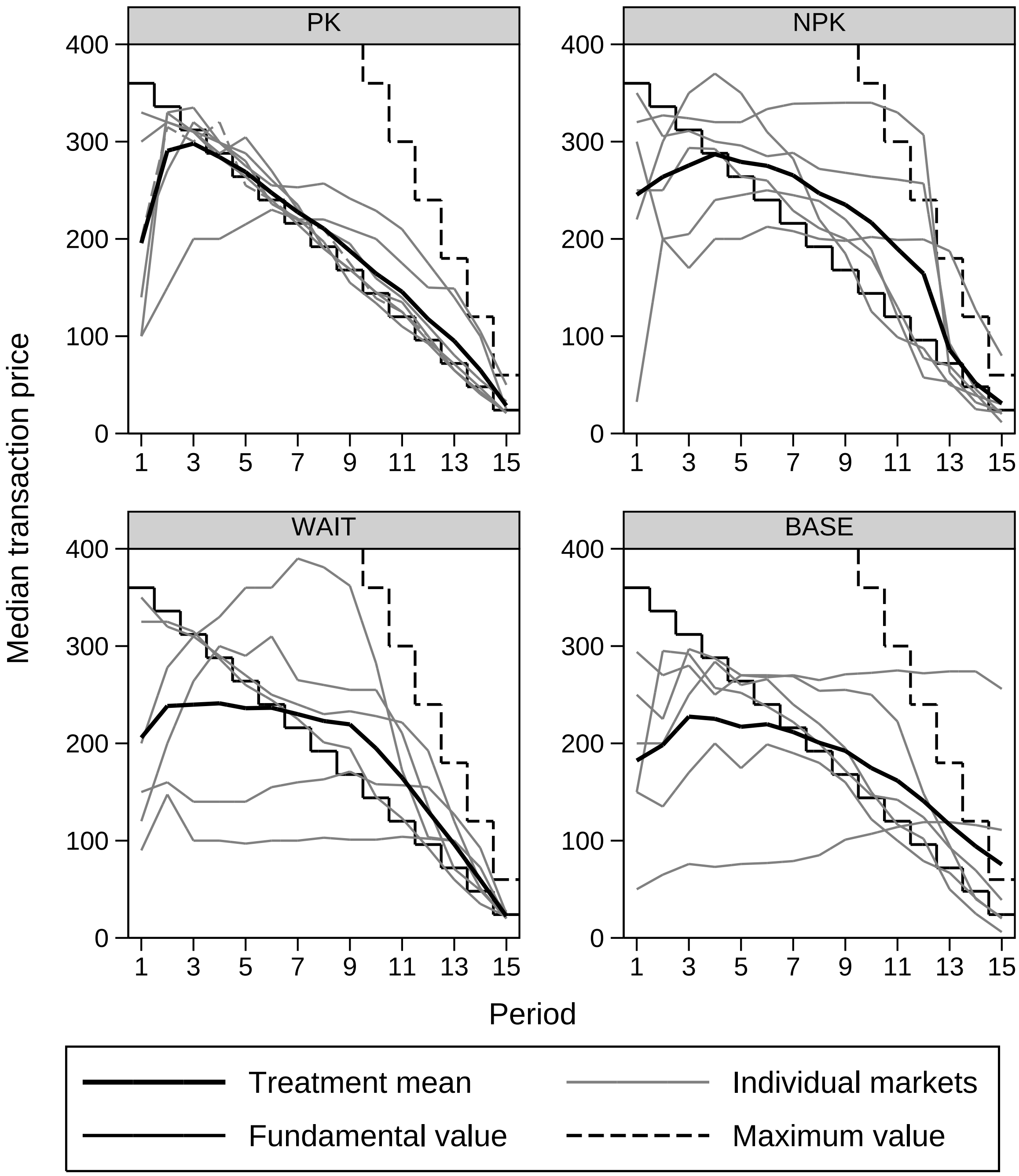
Looking firstly at the thick black line that represents the treatment mean, while it is evident that this tracks most closely to FV under treatment PK, it is also clear that it does not differ all that greatly across the four treatments. As is usual, it is the case *on average* in each treatment that prices tend to be moderately undervalued in the early periods and somewhat overvalued in the middle to later periods. This provides a first indication that there do not appear to be strong differences between the treatments in terms of *average overvaluation*.

However, turning to the gray lines that depict the price paths in individual markets, it is equally evident that there are clear differences between treatments in the degree of dispersion of the individual market trajectories around the treatment means. In particular, in some of the treatments in which average overvaluation is mild, this only holds because we observe both some markets that exhibit pronounced overvaluation and others that are characterized by dramatic undervaluation – and these cancel out in computing the treatment means. That is to say, we do indeed observe substantial *total mispricing*, and moreover this indeed appears to vary across the treatments.

In particular, it is clear that the price paths of the individual markets typically track FV more closely in the PK treatment than under either WAIT or BASE. Interestingly, however, it is less obvious that this is the case in the NPK treatment. This suggests that simply training subjects to

[13] In the PK treatment, we have partial data for a seventh market in which we experienced a fatal server crash after the end of the twelfth period. The available data from this market are shown in Figure 1 as a dashed gray line. We do not include this market in our main analysis of the RAD and RD measures below.

Figure 1: Median price trajectories in individual markets



Note: This figure shows the period-wise median transaction price trajectories in each of the six individual markets in each of our four treatments. (In the PK treatment, the gray dashed line shows partial data for a seventh market in which we experienced a fatal server crash after the end of the twelfth period.) In each panel, the thick black line represents the treatment mean, while the lower solid stepped line represents the time path of FV and the upper dashed stepped line represents the maximum dividend value of a share.

understand FV, without making this public knowledge, may not diminish mispricing as effectively as when it is also made public knowledge that all subjects have completed the training.

To formalize these observations, Table 2 reports an analysis of the measures *RAD* (for mispricing) and *RD* (for overvaluation). The top panel reports means of these measures for each of our four treatments. For example, in treatment PK the mean *RAD* of 0.182 indicates that prices in these markets deviate from FV in absolute terms by an average of 18.2%, while the mean *RD* of −0.028 indicates that the markets are on average undervalued by 2.8%. The treatment means of *RAD* are ranked in the expected order, with the greatest mispricing observed in BASE followed by WAIT and then NPK, and the lowest value observed under PK. The mispricing under PK is roughly half of that observed in BASE and WAIT. The mean overvaluation is clearly closest to zero in the PK treatment; there is no obvious interpretation for the ranking of *RD* across the remaining treatments, which display tendencies toward both over and undervaluation.[14, 15]

In the lower panel of Table 2, we report results of formal tests of our four hypotheses using the Fisher-Pitman exact permutation test for independent samples. This is a more powerful but computationally demanding alternative to the Mann-Whitney U test (Kaiser 2007). Since we state our hypotheses in one-sided terms, we report corresponding one-sided p-values. Our preferred measure is the *RAD*, because we state our hypotheses in terms of mispricing, and because we believe that this measure more accurately accounts for the possibility that our treatments might reduce the incidence of both over and undervaluation. Nonetheless, we also report corresponding tests for *RD* in the second column.[16] In Appendix Table C1 we report the corresponding one-sided p-values for some alternative statistical tests. In both Mann-Whitney U tests and in two-sample t-tests (with

[14]The PK treatment also exhibits the lowest (absolute) mean for each of the additional measures in Appendix Table B2, with the exception of *Share Turnover*. Smith, van Boening, and Wellford (2000, p. 577) note that *when prices are close to FV*, as is the case under PK, a high level of turnover may indicate that the market is highly competitive.

[15]As a robustness check of our results, we also examined the influence of individual subject characteristics. In OLS regressions of the *RAD* and *RD* measures on questionnaire items (with standard errors clustered by markets), we find a positive association between *RAD* and the proportion of postgraduate subjects in the market, while there are no significant effects in a regression of *RD*. In a regression of subjects' earnings, we find a significant positive effect of subjects' Cognitive Reflection Test and financial literacy scores, and a significant negative coefficient on a dummy for females. We also compared the subject composition of our treatments in terms of their observable characteristics. In pairwise comparisons between treatments, the only significant differences are that there are fewer females in PK and NPK compared to BASE, subjects in PK have lower financial literacy than those in NPK and WAIT, and that there are fewer postgraduate students in WAIT than BASE. We do not think it plausible that these differences would account for our results.

[16]The p-values in the second column of Table 2 thus correspond to tests of hypotheses, analogous to the ones stated in Section 1.5, in which the words "Mispricing is less severe" are replaced by the words "Overvaluation is lower".

Table 2: Analysis of mispricing and overvaluation

	RAD	RD
Treatment means		
PK	0.182	−0.028
NPK	0.299	0.078
WAIT	0.331	−0.044
BASE	0.375	−0.096
Permutation test p-values (one-sided)		
H1: WAIT vs. BASE	0.352	0.618
H2: PK vs. WAIT + BASE	0.027 **	0.629
H3: NPK vs. WAIT + BASE	0.272	0.858
H4: PK vs. NPK	0.063 *	0.135

Note: The top panel of this table reports the treatment means of RAD (mispricing) and RD (overvaluation) for the six markets in each of our four treatments. The bottom panel reports exact one-sided p-values for Fisher-Pitman independent samples permutation tests (Kaiser 2007) comparing these measures across treatments and groups of treatments as per our four hypotheses. * $p < 0.10$; ** $p < 0.05$.

unequal variances), we obtain precisely the same significance pattern of results as that shown in Table 2, confirming that our results are robust to the choice of test.

Result 1: Mispricing is not significantly lower under WAIT compared to BASE. Hypothesis 1 is not supported.

Our first hypothesis concerns the possibility that simply allowing subjects in WAIT more time to think might itself have the effect of reducing mispricing. However, we clearly cannot reject the null hypothesis that RAD is at least as great under WAIT as under BASE ($p = 0.352$). We can thus pool the data from WAIT and BASE in our tests of hypotheses 2 and 3; however as we show below, our results do not depend upon doing so.[17]

Result 2: Mispricing is significantly lower under PK compared to WAIT and BASE. Hypothesis 2 is supported.

Our second hypothesis states that we expect to replicate the finding of previous studies which show that when all subjects are trained to understand the FV process, and this is public knowledge,

[17] The two-sided p-values for the null hypotheses of equality of WAIT and BASE are 0.703 (RAD) and 0.766 (RD).

mispricing is less than when training is absent. We indeed find that *RAD* is significantly lower under PK than under WAIT and BASE pooled ($p = 0.027$). If we instead compare PK to either WAIT or BASE individually rather than pooled, then we obtain one-sided p-values of 0.062 and 0.044 respectively (see Appendix Table C2). These results confirm that our training protocol produces results that are comparable to those of other recent studies.

Result 3: Mispricing is not significantly lower under NPK compared to WAIT and BASE. Hypothesis 3 is not supported.

Having established that training has a significant effect when it is public knowledge, our next hypothesis concerns whether the effect is still observed when all subjects in the market are trained, but this is not public knowledge. This is what we would expect if the effect of training operated directly through reducing subjects' confusion, rather than indirectly through facilitating the coordination of expectations. As it turns out, while the mean observed level of mispricing is smaller in NPK, we cannot reject the null hypothesis that *RAD* is at least as great under NPK as under WAIT and BASE pooled ($p = 0.272$). If we compare NPK to WAIT or BASE individually, we obtain one-sided p-values of 0.365 and 0.235 respectively. Thus when training is not public knowledge, we do not find significantly less mispricing than in markets in which training is absent.

As noted by one of the referees, one factor that may contribute to the lack of support for Hypothesis 3 is that the observed level of mispricing in our BASE markets (mean *RAD* of 0.375 in a sample of $n = 6$ markets) is toward the low end of the reported literature. Thus Stöckl, Huber, and Kirchler (2010) report a mean *RAD* of 0.599 in $n = 4$ markets, while Kirchler, Huber, and Stöckl (2012) and Huber and Kirchler (2012) both report a mean *RAD* of 0.414 in what appears to be the same sample of $n = 6$ baseline SSW markets. On the other hand, Cheung and Coleman (2012) report a mean *RAD* of 0.377 in their $n = 6$ inexperienced baseline markets, which is very similar to the value we report here. Although we have no explanation for why we obtain a comparatively low value, we acknowledge that had our BASE markets been more typical of the reported literature, it is likely we might have found stronger support for Hypothesis 3. More generally, since non-rejection of the null hypothesis does not imply that the alternative is false, we do not interpret Result 3 to indicate that there is *no* direct effect of training – only that any such effect is not statistically significant in our sample. Indeed, as the same referee also observes, the mean RAD of 0.299 in our NPK treatment is mild relative to the baseline values cited above, and this may itself be interpreted as anecdotal evidence in support of a direct effect of training.

Result 4: Mispricing is marginally significantly lower under PK compared to NPK. There is mild support for Hypothesis 4.

Our final hypothesis concerns the effect of making it public knowledge in treatment PK that all subjects have been trained to understand the FV process, compared to treatment NPK in which all subjects have been trained but this is not public knowledge. Note that the significance of this comparison is inhibited by the limited number of observations, and the presence of considerable within-treatment heterogeneity in NPK in particular. Nonetheless, we find that RAD is marginally significantly lower under PK than under NPK ($p = 0.063$).

As noted in footnote 13, our main analyses of the RAD and RD measures in Table 2 excludes the data from a seventh PK market in which we experienced a fatal server crash after the twelfth period. Using the available data from this market, we can compute a twelve-period RAD measure of 0.127, and RD of -0.053. If we include this observation in the test of Hypothesis 4, the difference in RAD between NPK and PK increases in significance to $p = 0.041$. Alternatively, when we use the crashed market to replace the PK market with the lowest observed RAD value, thereby keeping the number of observations in the test unchanged, we find that the difference between NPK and PK remains marginally significant with $p = 0.071$. Thus although our main result is only marginally significant, we interpret these sensitivity analyses to give increased confidence that it is robust.

3 Conclusion

We interpret our results to indicate that, in addition to its direct effect in reducing confusion, much of the effect of training may also require that it be known to the market that confusion has indeed been reduced. It appears that when it is public knowledge that everyone has undergone training, subjects may perceive less uncertainty over the behavior of others and – since they may be less inclined to doubt the rationality of others – less opportunity for speculation. In short, the effect of making training public knowledge may be to facilitate the coordination of expectations on FV as the equilibrium price path. This indirect effect of training cannot operate in the absence of public knowledge, even when all subjects in fact have a correct understanding of FV.

Our interpretation is consistent with SSW's original conjecture that mispricing could occur even when all traders were sophisticated, if that fact was not common knowledge. Indeed, we believe that our NPK treatment represents a reasonable approximation to the conditions that SSW originally postulated. These conditions did not hold in SSW's original experiments because of the very real possibility of confusion, as first demonstrated by LNP. In our NPK treatment, while training can address confusion at an individual level, the common knowledge of rationality is nonetheless rendered impossible. Under these conditions, we continue to observe discernible mispricing.

We note that our interpretation aligns with the conclusion of a recent study by Xiong and Yu (2011), who examine the sources of a bubble in Chinese put warrants in the period 2005–2008. They take advantage of the finite life of these warrants to derive an upper bound on FV using a form of backward induction logic. Their preferred explanation for this bubble combines constraints on short sales (also present in standard SSW markets) with heterogeneous beliefs, and they explicitly interpret their data in terms of SSW's hypothesis of the non-common knowledge of rationality.

Our results are also consistent with the few other experimental studies we are aware of that credibly manipulate subjects' expectations regarding the rational play of their counterparts. Thus, Fehr and Tyran (2001) find that subjects exhibit substantially more pronounced money illusion when playing a price-setting game with other humans than with computerized agents who they know to have been pre-programmed to play optimally. They interpret this to show that the greater part of money illusion operates indirectly through strategic uncertainty over the behavior of others, which is absent in the computerized condition. Likewise, Palacios-Huerta and Volij (2009) study the play of student subjects and chess players in a laboratory centipede experiment. They find that students in the role of the first mover are ten times more likely to stop the game at the first decision node (as predicted by subgame perfect equilibrium) when playing a chess player as opposed to another student. Conversely, chess players are less likely to stop the game when their opponent is a student as compared to another chess player. They interpret these results to indicate that players' assessment of the rationality of opponents is critical in determining whether subgame-perfect play emerges.

In summary, we offer a richer account of the role of confusion – and the effects of training subjects to reduce it – in understanding mispricing in SSW markets. We submit that by not only training subjects to understand declining FV, *but also making this public knowledge*, recent studies may have not only resolved the problem of confusion, but also the problem of coordinating expectations. We provide a new experimental design that makes it possible to disentangle the effect of training *per se* from that of its public knowledge, and report evidence to indicate that public knowledge has a distinct effect over and above that of training alone.

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Appendices

A Experiment parameters

Table A1: Endowment and exchange rate parameters

Endowment type	I	II	III
Number of traders of this type	3	4	3
Initial stock	2	4	6
Initial cash	1,890	1,170	450
Endowment value (ECU)		2,610	
Exchange rate (DKK/ECU)		1/11	
Endowment value (DKK)		237.27	
Total Stock of Units (TSU)		40	

Note: One DKK is approximately equal to 0.20 USD (as of November 2009).

B Additional bubble measures

Table B1: Additional bubble measure definitions

Measure	Definition
Relative Absolute Deviation[a]	$RAD = \frac{1}{T}\sum_t \lvert \bar{P}_t - f_t \rvert \, / \, \lvert \bar{f} \rvert$
Relative Deviation[a]	$RD = \frac{1}{T}\sum_t \left(\bar{P}_t - f_t\right) / \, \lvert \bar{f} \rvert$
Share Turnover[b]	$ST = \left(\sum_t q_t\right) / q$
Price Amplitude[b]	$PA = \max\left[\left(\bar{P}_t - f_t\right) / f_1\right] - \min\left[\left(\bar{P}_t - f_t\right) / f_1\right]$
Total Dispersion[c]	$TD = \sum_t \left\lvert \tilde{P}_t - f_t \right\rvert$
Average Bias[c]	$AB = \sum_t \left(\tilde{P}_t - f_t\right) / T$

Note: T = total number of trading periods; $\bar{P}_t$ = mean transaction price in period t; $\tilde{P}_t$ = median transaction price in period t; f_t = fundamental value in period t; $\bar{f}$ = mean fundamental value over the life of the asset; q_t number of transactions in period t; q = total number of shares outstanding. [a] Introduced by Stöckl, Huber, and Kirchler (2010); [b] Introduced by King (1991); [c] Introduced by Haruvy and Noussair (2006).

Table B2: Additional bubble measure results

Treatment	Market	RAD	RD	ST	PA	TD	AB
PK	1	0.059	−0.051	11.43	0.265	68.0	−3.60
	2	0.125	−0.060	8.63	0.578	347.5	−9.43
	3	0.192	−0.049	5.40	0.731	472.0	−4.33
	4	0.386	−0.119	9.83	0.837	1,104.0	−20.40
	5	0.101	−0.065	4.78	0.555	342.0	−14.73
	6	0.230	0.175	8.33	0.408	664.5	34.03
Mean		0.182	−0.028	8.06	0.562	499.7	−3.08
NPK	1	0.461	0.398	4.20	0.749	1,328.5	78.50
	2	0.225	0.043	3.25	0.634	648.5	9.23
	3	0.303	0.266	5.25	0.534	832.0	49.07
	4	0.149	−0.053	4.13	0.445	389.5	−8.03
	5	0.436	−0.097	7.18	1.124	1,322.0	−17.60
	6	0.223	−0.086	5.30	0.499	636.0	−18.00
Mean		0.299	0.078	4.88	0.664	859.4	15.53
WAIT	1	0.198	0.128	6.00	0.466	516.0	30.67
	2	0.531	−0.484	10.73	0.731	1,559.0	−96.13
	3	0.417	−0.270	9.23	0.728	1,213.5	−52.43
	4	0.057	−0.013	3.05	0.245	138.0	−1.67
	5	0.331	0.063	5.98	0.887	982.5	8.30
	6	0.450	0.310	4.25	0.907	1,268.5	54.70
Mean		0.331	−0.044	6.54	0.661	946.3	−9.43
BASE	1	0.320	−0.320	5.50	0.569	892.5	−59.50
	2	0.600	0.410	8.58	0.957	1,585.5	78.77
	3	0.167	−0.081	3.80	0.630	444.0	−12.53
	4	0.266	0.097	9.18	0.627	769.5	18.30
	5	0.680	−0.525	14.78	1.082	1,962.0	−100.80
	6	0.217	−0.159	8.13	0.530	549.5	−21.03
Mean		0.375	−0.096	8.33	0.733	1,033.8	−16.13

C Robustness checks

Table C1: Alternative statistical tests

	RAD	*RD*
Mann-Whitney p-values (one-sided)		
H1: WAIT vs. BASE	0.469	0.650
H2: PK vs. WAIT + BASE	0.033 **	0.590
H3: NPK vs. WAIT + BASE	0.341	0.753
H4: PK vs. NPK	0.066 *	0.294
t-test (unequal variances) p-values (one-sided)		
H1: WAIT vs. BASE	0.351	0.612
H2: PK vs. WAIT + BASE	0.016 **	0.670
H3: NPK vs. WAIT + BASE	0.243	0.883
H4: PK vs. NPK	0.063 *	0.147

Note: The top panel of this table reports exact one-sided p-values for Mann-Whitney U tests comparing the measures of of *RAD* (mispricing) and *RD* (overvaluation) across treatments and groups of treatments as per our four hypotheses. We compute these using the `mwtest` command documented in Kaiser and Lacy (2009). The bottom panel reports the corresponding one-sided p-values for two-sample t-tests (with unequal variances). * $p < 0.10$; ** $p < 0.05$.

Table C2: Pairwise treatment comparisons

	RAD	*RD*
Permutation test p-values (one-sided)		
H2: PK vs. WAIT	0.062 *	0.557
H2: PK vs. BASE	0.044 **	0.686
H3: NPK vs. WAIT	0.365	0.794
H3: NPK vs. BASE	0.235	0.853

Note: This table reports exact one-sided p-values for Fisher-Pitman independent samples permutation tests (Kaiser 2007) for the measures of of *RAD* (mispricing) and *RD* (overvaluation) in pairwise comparisons between treatments. * $p < 0.10$; ** $p < 0.05$.

Online Appendix (not for publication): Experiment Instructions

General Instructions

This is an experiment on decision making in a market. The instructions are simple and if you follow them carefully and make good decisions, you may earn a considerable amount of money which will be paid to you in cash at the end of the experiment.

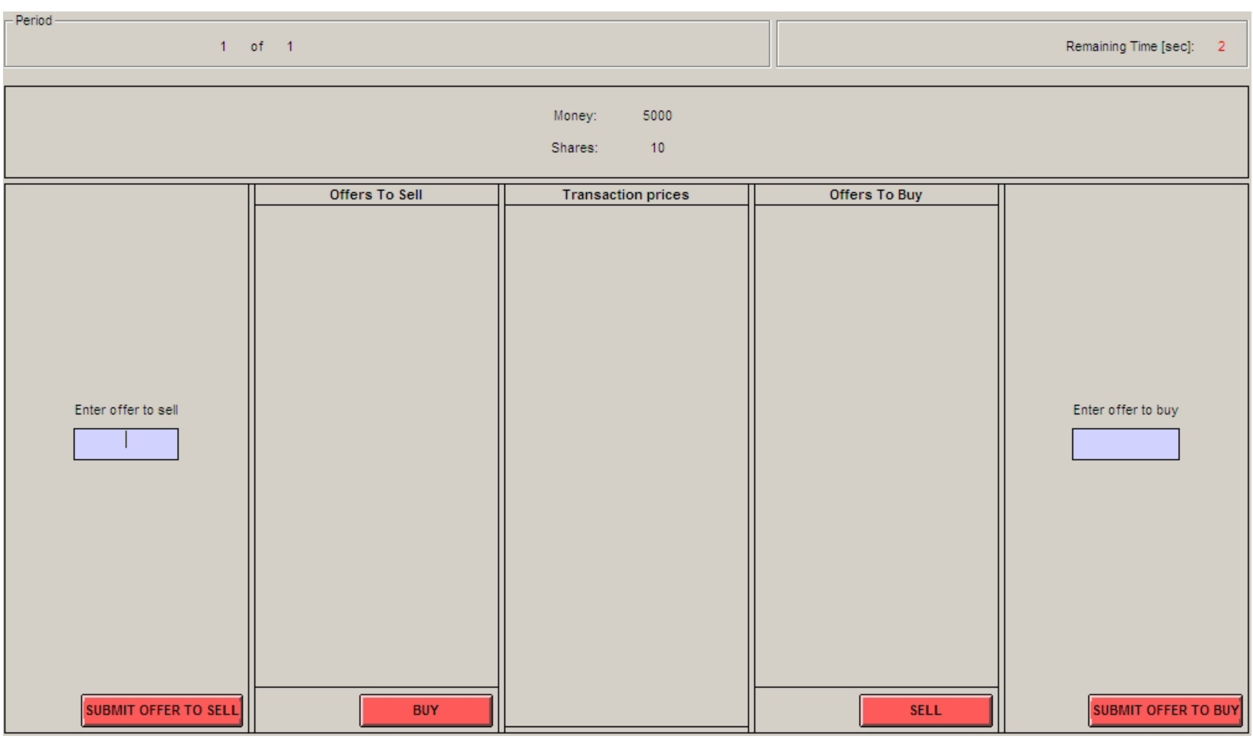
Please do not communicate with other participants during the experiment. If you have a question please raise your hand, and an experimenter will assist you.

In this experiment, you have the opportunity to buy or sell in a market. The money used in this market is 'Experimental Currency Units' (ECU). All trading will be done in terms of ECU. The cash payment to you at the end of the experiment will be in Danish kroner. The conversion rate will be **11 ECU to 1 krone**.

You will then be asked to complete a questionnaire, after which you will receive your payment. The entire experiment will last approximately two-and-a-half hours, including half an hour for instructions and practice.

How to use the Computerized Market

On the top right of the screen you will see how much time is left in the current trading period. The items you can buy and sell in the market are called shares. In the center of your screen you will see the number of shares and the amount of money you currently have.



The screen can be used to participate in the market in one of four ways.

Making an offer to sell a share, by entering the price at which you would like to sell:

To offer to sell a share, enter the price at which you would like to sell in the box labeled ‘Enter offer to sell’ on the left of the screen, then click on the button ‘Submit offer to sell’.

The second column from left will show a list of offers to sell, each submitted by a different participant. The lowest offer-to-sell price will always be on the bottom of the list. Your own offer will appear in blue. Submitting a new offer will replace your previous one.

Making an offer to buy a share, by entering the price at which you would like to buy:

To offer to buy a share, enter the price at which you would like to buy in the box labeled ‘Enter offer to buy’ on the right of the screen, then click on the button ‘Submit offer to buy’.

The second column from right will show a list of offers to buy, each submitted by a different participant. The highest offer-to-buy price will always be on the bottom of the list. Your own offer will appear in blue. Submitting a new offer will replace your previous one.

Buying a share, by accepting an offer to sell:

You can select an offer to sell in the second column from left by clicking on it. If you click the ‘Buy’ button at the bottom of this column, you will buy one share at the selected price. However you are not allowed to buy a share from yourself.

When you accept an offer to sell, it will disappear from the list. If you had also placed an offer to buy, it will disappear from the offers to buy list because you have just bought a share.

Selling a share, by accepting an offer to buy:

You can select an offer to buy in the second column from right by clicking on it. If you click the ‘Sell’ button at the bottom of this column, you will sell one share at the selected price. However you are not allowed to sell a share to yourself.

When you accept an offer to buy, it will disappear from the list. If you had also placed an offer to sell, it will disappear from the offers to sell list because you have just sold a share.

Transaction prices

When you buy a share your money decreases by the price of the purchase. You can only buy a share if you have enough money to pay for it.

When you sell a share your money increases by the price of the sale. You can only sell a share if you owned one to begin with.

In the middle column of the screen, labeled ‘Transaction prices’, you will see the prices at which shares have traded in the current period.

Practice period

You now have ten minutes to practice buying and selling shares. Your actions in this practice period will not influence your earnings or your position later in the experiment. The only goal is to master the use of the interface.

Please make sure that you successfully submit offers to buy and offers to sell. Also make sure that you successfully accept other people's offers to buy and sell shares.

If you have any questions, please raise your hand and an experimenter will assist you.

Specific Instructions for this Experiment [*Distributed after completion of practice period.*]

In each market there are ten participants. *Although there may be more than ten participants in the lab today, you will always be in the same market of ten participants, consisting of yourself and the same set of nine others.*

The market will consist of fifteen trading periods. In each period there will be four minutes during which you can trade shares in exchange for ECU.

At the beginning of the first trading period, your screen will display your initial holdings of money and/or shares. These will not necessarily be the same for all participants in the market.

Any trade that you make will change your holdings of money and shares. These holdings will carry over from one trading period to the next.

Dividends

Recall that the market consists of fifteen trading periods. Shares are assets with a life of fifteen periods. Each share will pay a dividend to its current owner at the end of each period.

The dividend is randomly determined by the computer, and will be the same for all shares. In particular, each share that you own at the end of a period will pay:

- a dividend of 0 ECU with probability 1/4;
- a dividend of 8 ECU with probability 1/4;
- a dividend of 28 ECU with probability 1/4; and
- a dividend of 60 ECU with probability 1/4.

Since each outcome is equally likely, the average dividend is (0+8+28+60) / 4 = 24 ECU in every period.

Dividends will be added to your money balance automatically at the end of each period. After the dividend is paid at the end of the fifteenth trading period, all shares will be worthless and there will be no further earnings possible from them.

Average Holding Value Table

You can use your AVERAGE HOLDING VALUE TABLE to help you make decisions.

The first column indicates the Ending Period of the market. The second column indicates the Current Period for which the average holding value is being calculated. The third column gives the Number of Holding Periods from the Current Period to the Ending Period.

The fourth column gives the Average Dividend per Period for each share that you hold. The fifth column gives the Average Holding Value per Share that you hold from the Current Period until the end of the market.

That is, for each share that you hold for the remainder of the market, you will earn on average the amount listed in column five. The value in column five is calculated by multiplying the values in columns three and four.

AVERAGE HOLDING VALUE TABLE

Ending Period	Current Period	Number of Holding Periods	×	Average Dividend Per Period	=	Average Holding Value Per Share
15	1	15		24		360
15	2	14		24		336
15	3	13		24		312
15	4	12		24		288
15	5	11		24		264
15	6	10		24		240
15	7	9		24		216
15	8	8		24		192
15	9	7		24		168
15	10	6		24		144
15	11	5		24		120
15	12	4		24		96
15	13	3		24		72
15	14	2		24		48
15	15	1		24		24

Your Earnings

At the end of the market, your earnings will equal the amount of money you have at the end of period fifteen, after the last dividend has been paid.

This amount of money will be equal to:

Any money you had at the beginning of period one
\+ *Any money you received from sales of shares*
− *Any money you spent on purchases of shares*
\+ *Any dividends you received*

At the conclusion of the experiment this amount will be converted into Danish kroner at the rate specified on page one of these instructions, and paid to you in cash.