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# Do ambiguity effects survive in experimental asset markets?

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## Abstract

Despite ample evidence of ambiguity preferences in individual decision making, experimental studies of ambiguity effects in financial markets are scarce and inconclusive. Although a number of theoretical studies explain empirical puzzles in finance with ambiguity preferences, it is not a given that individual ambiguity effects survive in markets. We therefore combine the predominant design for ambiguous prospects in individual decision making, the two-color Ellsberg urn, with predominant designs in financial trading, the double auction and the call market, and compare trading in risky and in ambiguous assets. Our results suggest that markets are able to wash out ambiguity effects, which we do observe in an individual decision making control. We find no effects on transaction prices or quotes and also no effects on volume, volatility, or portfolios. This applies both to double auctions and call markets, with and without simultaneous trading of risky and ambiguous assets, and even in the absence of arbitrage.

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# 1 Introduction

Many real life decisions are characterized by ambiguity, where we lack important information, such as objective probabilities of the relevant states. Keynes (1921) proposed a simple thought experiment to illustrate the effects of ambiguity.

Imagine "[...] the two cases following of balls drawn from an urn. In each case we require the probability of drawing a white ball; in the first case we know that the urn contains black and white in equal proportions; in the second case the proportions of each color is unknown, and each ball is as likely to be black as white. It is evident that in either case the probability of drawing a white ball is  $\frac{1}{2}$ , but that the weight of the argument in favor of this conclusion is greater in the first case." (Keynes, 1921, Chapter VI.6)<sup>1</sup>

Ellsberg (1961) used this experimental design, commonly referred to as the ‘two-color Ellsberg urn’, to show that a preference for the risky urn (with known probabilities) over the ambiguous urn (with unknown probabilities) violates Subjective Expected Utility Theory and the Sure-thing Principle of the Savage axioms (Savage, 1954). Since then a large body of individual choice experiments confirmed that decision makers are, on average, ambiguity averse when confronted with the above quoted choice.<sup>2</sup>

Ambiguity preferences are a possible cause for a number of empirical puzzles in financial economics, which expected utility theory would consider to be (behavioral) anomalies. After the development of several non-expected utility models of individual decision making that considered ambiguity preferences (e.g., Gilboa and Schmeidler, 1989; Ghirardato et al., 2004; Klibanoff et al., 2005; Nau, 2006), a growing number of theoretical papers incorporated ambiguity effects into market models to explain long-standing anomalies in finance, like the equity premium puzzle (Epstein and Wang, 1994; Maenhout, 2004; Cao et al., Winter 2005; Leippold et al., 2008), portfolio inertia (Epstein and Wang, 1994; Illeditsch, 2011), the familiarity bias and the home bias in investments (Uppal and Wang, 2003; Huang, 2007; Cao et al., 2011), amplification effects (Routledge and Zin, 2009; Guidolin and Rinaldi, 2010; Illeditsch, 2011), and

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<sup>1</sup>Keynes (1921) did not use the term ambiguity. Instead, he referred to ‘the weight of arguments’, but was not sure about this concept. In fact, at the beginning of the Chapter VI he writes: "[A]fter much consideration I remain uncertain as to how much importance to attach to it." (Keynes, 1921, Chapter VI.1)

<sup>2</sup>For excellent overviews, see Camerer and Weber (1992), Wakker (2010), Etner et al. (2012). Note that individuals can also be ambiguity seeking, for example, if the probability of winning is very low (e.g., Einhorn and Hogarth, 1985; Curley and Yates, 1989; Kahn and Sarin, 1988). Where possible, we therefore refer to the more neutral terms ‘ambiguity effects’ or ‘ambiguity preferences’, although the average empirical response to the two-color Ellsberg urn is ambiguity aversion.

asymmetric reactions to good and bad news (Epstein and Schneider, 2008; Epstein et al., 2010; Illeditsch, 2011).

Yet, it is not a given that ambiguity effects in individual decision making survive when market forces are at work. In markets, subjects' decisions are no longer independent, but are subject to market feedback from other traders. According to the standard neoclassical argument, market mechanisms and incentives should reduce behavioral biases and non-expected utility behavior, including ambiguity effects (Camerer, 1987).

Despite the burgeoning theoretical literature and the promising explanatory potential of ambiguity preferences, evidence on the impact of ambiguity in experimental financial markets is very limited and rather mixed. Studies that combine the predominant design for ambiguous assets in individual decision making, the two-color Ellsberg urn, with predominant market designs in experimental financial trading, the double auction and the call market, are particularly scarce. Therefore, the main contribution of this paper is to test whether ambiguity effects arise and survive in financial trading environments, by providing a simple, direct and simultaneous comparison between an ambiguous asset, based on two-color Ellsberg urn, and a corresponding risky asset in double auctions and call markets.

A chronological description of our experiment best explains the treatments and their results. We started with a standard double auction market where subjects were able to simultaneously trade a risky and an ambiguous asset, but in two different markets on a split screen. To provide more room for ambiguity effects we stayed as close as possible to individual decision task settings: by separating the two markets with dedicated trading account for each market, we eliminated the possibility to arbitrage between risk and ambiguity; and by displaying both markets on the same screen, we increased the salience of ambiguity, as shown by Fox and Tversky (1995). Depending on the results of this treatment, the plan was to proceed into one of the following two directions: in case of ambiguity effects, we planned to test their robustness in a more integrated financial market with arbitrage. In case of no ambiguity effects, we planned to give them more room by administering a call market with less intra-period market feedback. The results from the double auction market experiment did not show any ambiguity effects on transaction prices, bids, asks, volume, volatility, or share distributions in traders' portfolios. Accordingly, we administered a call market treatment, while taking great care to keep all other aspects unchanged. Again, we did not observe systematic ambiguity effects on any of the above mentioned variables. We therefore degenerated the two sided call market setting into an individual auction against the computer by implementing a Becker–DeGroot–Marschak (BDM) mechanism (Becker et al., 1964). This

was meant to test whether our operationalization of ambiguity was sufficient to produce ambiguity effects in individual decision making. As expected, subjects' individual willingness to pay (WTP) for the risky asset was significantly greater than for the ambiguous asset. However, although these results supported our operationalization of ambiguity, alternative methods might have been even stronger. The latter could explain, why related studies discussed further below (e.g., Sarin and Weber, 1993) found some ambiguity effects in markets, while we did not. As a robustness check we therefore ran the BDM treatment again, but produced the ambiguous asset with a compound lottery, which was also used in Sarin and Weber (1993). The difference between the WTP for the risky and the ambiguous assets turned out to be smaller and statistically insignificant. Hence, it seems that our original operationalization of ambiguity has been sufficiently strong, supporting the notion that market forces washed out ambiguity effects in the double auction and call markets.

The paper complements a small number of experimental studies on ambiguous assets in markets. To the best of our knowledge, Camerer and Kunreuther (1989) are the first to study ambiguity effects in experimental markets. They administered open outcry double auction markets for insurance coverage against risky and ambiguous hazards that produced only losses. The markets were separated without opportunities for arbitrage and ambiguity was operationalized through compound lotteries (second-order probabilities), also referred to as 'weak ambiguity'. The authors conclude that "[t]he effects of ambiguity are rather minor and mixed" (p.287), with conflicting effects on the number of insurance contracts held at the end of trading and no effects on prices. By contrast, we used strong ambiguity and study its effects in the gain domain.

Bossaerts et al. (2010) used a double auction environment to experimentally investigate the simultaneous trading of assets with state-dependent dividends where state probabilities were either risky or ambiguous. They administered a three-color Ellsberg urn, where the payoffs from the risky and the ambiguous asset are *not* independent from each other, and they let subjects learn about the composition of the ambiguous urn by drawing balls without replacement. The authors acknowledge that this reduction in ambiguity on prices could be confounded with convergence to equilibrium, "because it is difficult—if not impossible—to assess when prices have 'settled down' during an experimental period." (p.1351) Moreover, their setup requires the computation of state price/probability ratios with a number of assumptions on priors and updating.<sup>3</sup>

<sup>3</sup>As there exists no theory for updating under ambiguity, the authors "follow the simplest approach and use uniform priors over the ambiguous states for the initial draw, updated by Bayes' Rule for subsequent draws." (p.1347) Although recent evidence points into the direction of Bayesian learning under ambiguity, it also suggests that subjects might over-adjust/under-adjust to contradictory/confirming signals. (Qiu and Weitzel, 2013)

Bossaerts et al. conclude that ambiguity aversion matters only partially: "The predictions for portfolio choices seem quite robust and well supported by the experimental data; the predictions for prices are less robust." (p.1355) By contrast, we excluded updating under ambiguity and ensured independent payoffs by contrasting a two-color Ellsberg urn with a separate risky urn.

Kocher and Trautmann (2013) designed two separate first-price sealed bid market environments for risky and ambiguous assets and allowed subjects to self-select into one of two, mutually exclusive markets. Hence, as in our study, arbitrage between risk and ambiguity was not possible. The ambiguous asset was operationalized with a two-color Ellsberg urn. Although most subjects chose to submit a bid in the risky market, average transaction prices for both types of assets were equal across markets. By contrast, we allowed all subjects, without prior self-selection, to trade both types of assets in double auction and call market environments. Moreover, risky and ambiguous assets were not traded in mutually exclusive environments, but simultaneously.

The study of Sarin and Weber (1993) is most related to our experimental setting: in 14 markets the authors explored several combinations of treatment effects, including sealed bid auctions vs. open outcry double auctions, independent vs. simultaneous trading of risky and ambiguous assets (with arbitrage), experienced executives vs. students as traders, smaller vs. larger number of trading periods, low vs. equal probability levels, and the framing of ambiguity as nature vs. expert judgments. In all treatments, compound lotteries were used to operationalize (weak) ambiguity. Sarin and Weber (1993) report inconclusive results when risky and ambiguous assets were traded independently. When traded simultaneously, market prices for ambiguous assets were significantly lower. The authors acknowledge that this is unexpected, as simultaneous auctions allow for arbitrage.<sup>4</sup> Note, however, that this result is based on only four markets (including both sealed bid auctions and double auction markets), which constitute the entire experimental evidence on the simultaneous trading of risky and ambiguous shares with independent outcomes. We complement these market experiments with a greater number of independent observations, strong ambiguity, no arbitrage in simultaneous trading, and an individual decision making BDM control.

As the designs of prior studies differ quite substantially, it is not surprising that results are mixed. For better comparability and interpretability this paper attempts to provide a simple and direct test with well-known design features. With regard to individual ambiguity effects we can clearly replicate prior studies.

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<sup>4</sup>"It seems that a more transparent comparison between the unambiguous and ambiguous assets leads to a greater differential in market prices (simultaneous versus independent) contrary to our expectation." (Sarin and Weber, 1993, p.612)

However, when exposing these effects to basic market mechanisms, ambiguity does not seem to play a role anymore.

The paper is organized as follows. Section 2 presents the experimental design, followed by a discussion of the results and robustness checks. Section 4 concludes.

## 2 Experimental Design

### 2.1 Overview

Table 1 provides an overview of the experiment. In total, 176 subjects participated in eight sessions. We ran four sessions with double auction markets (DA, twelve independent markets with eight subjects each), two sessions with call markets (CM, six independent markets with eight subjects each), and two sessions with BDM evaluations (WTP, individual observations of 32 subjects). Each experimental session consisted of twelve consecutive and identically designed periods. Subjects traded or evaluated risky assets (R) and ambiguous assets (A), which we both referred to as shares. The shares were traded in two markets, which we administered separately, but simultaneously on a split screen. To avoid any reference to risk or ambiguity we referred to the shares on the two sides of the split screen as blue and yellow shares. All monetary values were denominated in experimental currency units (ECU) with an exchange rate of 200 ECU per Euro.<sup>5</sup>

### 2.2 Risky and ambiguous asset

The risky and the ambiguous asset paid either a high or a low dividend (300 or 124 ECU), which were announced at the end of each period. We follow Abdellaoui et al. (2011) and determined the dividend payments by drawing a ball from an urn that contained eight balls with (up to) eight colors. If the drawn ball matched one of four winning colors, the corresponding asset paid the high dividend (and, otherwise, the low dividend). As the probability for drawing a winning color is 0.5 we replicate the underlying objective probabilities of a two-color Ellsberg urn.

The four winning colors were determined as follows. Upon arrival, and without any information about the experiment, each of the 24 subjects selected four colors by independently and privately marking them on a sheet with eight colors. Hence, each of the 24 sheets indicated four privately chosen winning colors. The 24 sheets were pinned on the lab wall, organized in two columns: one column with twelve sheets, one for each period, represented the winning colors

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<sup>5</sup>In the experiment we referred to one ECU as 'Taler'.

of the risky assets; the other column with twelve sheets, one for each period, represented the winning colors of the ambiguous assets.

For the risky asset, we publicly filled an urn with exactly eight different colors. After each period, a subject drew one ball from the risky urn (with replacement) and we compared the drawn color with the winning colors for that period to determine the dividend.

To operationalize ambiguity, twelve different urns (one for each period) were created as follows. For each session we invited four additional subjects, who stayed in the waiting room outside of the lab. After the other 24 subjects were seated and marked their winning colors, we asked the additional subjects to each fill three urns with exactly eight balls in any color combination of their liking. We provided eight transparent jars, each filled with 144 balls (small marbles) of one of the eight colors, and placed them on a table in the front of the lab.<sup>6</sup> We then gave each of the additional subjects three empty urns and concealed the table with blinds. Neither the experimenters nor the other subjects were able to see how the additional subjects filled the ambiguous urns (one by one) behind the blinds. None of the subjects had any further information about the experiment. After the additional subjects handed over their ambiguous urns, we placed all twelve urns (one for each period) in a random sequence on a separate table in full view of the other subjects. We then privately paid each of the additional subjects a fixed fee of six Euros and dismissed them.<sup>7</sup> After each period, a subject drew a ball from one of the twelve ambiguous urns to determine the dividend of the ambiguous asset. The ambiguous urn was then removed so that the dividend payment of each period was determined with a new ambiguous urn.

As a robustness check for our main operationalization of ambiguity, we also used a compound lottery to produce a weakly ambiguous urn (with second-order probabilities) in Session BDM2. In line with, e.g., Sarin and Weber (1993), Abdellaoui et al. (2011), and many others, we implemented (computerized) lotteries with uniform distributions to determine the composition of colors in the ambiguous urn. A random draw of this urn then determined the dividend of the ambiguous asset, as described above.

### 2.3 Double auction market

We used a standard computerized continuous open-book double auction market in which eight participants traded for twelve periods. Each trading period lasted

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<sup>6</sup>We chose 144 marbles so that it was in principle possible to fill all twelve urns with only one color.

<sup>7</sup>The blinds stayed around the table with the jars throughout the whole experiment so that nobody (also not the experimenters) could see the remaining balls. To make sure that there were eight balls in each ambiguous urn, we counted the number of marbles through the fabric of the urns (bags).



four minutes. Traders could both sell and buy shares with limit orders or direct trades. For limit orders a new bid/offer had to be higher/lower than outstanding bids/offers.

The screen was organized in such a way that it simultaneously displayed two independent markets on the left and right hand side (see Figure 1 in the appendix). In Session DA1 and DA2, traders simultaneously faced risky assets in one market and ambiguous assets in the other.<sup>8</sup> Hence, Session DA1 and DA2 directly contrasted the ambiguous with the risky asset, which makes the ambiguous alternative more salient and pronounces ambiguity effects, at least in individual decision making (Fox and Tversky, 1995). As a robustness check, we also ran sessions that mimic the setup of Sarin and Weber (1993), where assets are traded without a direct contrast: in Session DA3 all traders faced risky assets in both markets, while in Session DA4 all traders faced ambiguous assets in both markets on the split screen.

At the beginning of each trading period, subjects were endowed with one of two endowment profiles. In DA1 and DA2, one half of the traders received 10 risky shares and 4200 ECU cash in the risky market and 6 ambiguous shares and 5200 ECU cash in the ambiguous market; and the other half received 6 risky shares and 5200 ECU cash in the risky market and 10 ambiguous shares and 4200 ECU cash in the ambiguous market. In all endowments, 3000 ECU of the cash portion was provided as a loan and had to be returned at the end of each period.<sup>9</sup> The same endowment profiles applied to DA3/DA4, but with risky/ambiguous shares only. Arbitrage across markets was not possible and short sales were not allowed. As explained in the introduction, we wanted to give ambiguity effects ample room to survive.

## 2.4 Call market

The main motivation for the CM treatment was to reduce the within market feedback. We therefore took great care to only adjust the market design and to keep everything else (e.g., endowments, split screen allocation of markets, ambiguity operationalization) as close as possible to the DA treatment. In each market, traders submitted the maximum quantity of shares they are willing to buy/sell and the highest/lowest price they are willing to pay/accept (see Figure 2 in the appendix). After all subjects have entered their trades, the computer matched the buy and sell orders to determine a market price for each of the two CMs on the screen and then cleared each of the two markets separately.

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<sup>8</sup>We counter-balanced R and A on the left and right hand side of the split screen: four traders (randomly determined) always traded the risky asset on the left hand side and the ambiguous asset on the right hand side of the split screen, and the other four traders vice versa.

<sup>9</sup>No subject went bankrupt.

We used the same programming code and implementation as Cheung and Palan (2012).<sup>10</sup>

## 2.5 Individual Decision Making

In eliciting the individual WTP of subjects for the risky and the ambiguous asset we, again, used the split screen partitioning to stay close to the appearance of the other treatments (see Figure 3 in the appendix). We implemented the BDM mechanism as follows. Subjects submitted their willingness to pay for one share in each of the two markets in twelve subsequent periods. The computer drew a random price for each market. If the subject's stated willingness to pay exceeded the random price, the subject purchased the share at the random price; otherwise, the subject did not purchase the share. As the WTP only applied to one share, we had to adjust the dividend by a factor of ten (high/low dividend: 3000/1240 ECU) and the endowment to 3500 ECU (including a 500 ECU loan).

In the Session BDM1 we operationalized ambiguity as in all DA and CM sessions by inviting additional subjects who produced ambiguous urns (see Section 2.2 above). To ensure that this operationalization of ambiguity was strong enough, we repeated the treatment in a Session BDM2 with ambiguity based on a compound lottery (see above).

## 2.6 Final Payment

For the final payment, we employed the random incentive system (Starmer and Sudgen, 1991; Hey and Lee, 2005). At the end of the session one of the twelve periods was individually and randomly selected (with a twelve-sided dice). Within the payoff-relevant period we selected one of the two markets of the split screen (left or right) with a coin flip. Traders in the DA and CM sessions received the end-of-period net cash plus the dividends of their share holdings. Subjects in the BDM treatments were paid according to their WTP and the outcome of the BDM mechanism.

## 2.7 Procedure

All subjects were recruited from a broad student databases across several fields of study using ORSEE (Greiner, 2004). We only invited subjects, who have never participated in asset market experiments before.<sup>11</sup> All DA and CM sessions and BDM1 were run in November 2011, December 2011 and July 2012 at the University of Düsseldorf (Germany). BDM2 was run in November 2012

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<sup>10</sup>We thank the authors for providing their zTree code.

<sup>11</sup>To minimize the confounding effects and variance arising from gender compositions in experimental asset markets (as shown in Eckel and Fullbrunn (2012)), we only invited male students in the DA and BDM sessions. In the CM sessions, two markets also include female traders. As our results do not differ for female traders or markets, we do not report them separately.

at the University of Nijmegen (The Netherlands). The experiment was entirely computerized using zTree (Fischbacher, 2007). The duration per session was approximately 1.5 hours for which subjects received an average payment of 20.06 Euros.<sup>12</sup>

The sequence of events was as follows. Upon arrival, subjects marked the winning colors and the additional subjects filled the ambiguous urns and were then dismissed (except in BDM2). In the DA and CM sessions, market participants were then trained in using the computer interface. Step by step we explained how to buy and sell, and subjects had a trial round to get accustomed to the interface. After the trial round, we carefully explained the actual experiment, in particular, the determination of the dividend payments.<sup>13</sup> We then privately answered remaining questions and started the first of the twelve payoff-relevant periods. At the end of the session, subjects answered a short demographic questionnaire, were paid in cash and dismissed.

### 3 Results

In analyzing the data we search for ambiguity effects in three categories of market variables: market prices (incl. bids and asks), trading volume and volatility, as well as portfolio composition in subjects' end-of-period share holdings.<sup>14</sup> For robustness we also check whether our operationalization of ambiguity generates ambiguity effects on individual decision making and how strong these effects are in comparison to an alternative operationalization (compound lottery). All results are analyzed per period to identify possible learning effects. Unless mentioned otherwise we use a 95 percent confidence interval as standard level of statistical significance ( $p < 0.05$ ).

#### 3.1 Prices, bids, asks

We start with the analysis of prices in the DA, where the risky and the ambiguous assets are traded simultaneously (Sessions DA1 and DA2), by computing the difference between the median market prices for R and A for each period

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<sup>12</sup>BDM 2 was half an hour shorter than the other sessions (1 hour).

<sup>13</sup>All instructions and ztree codes are available from the authors upon request.

<sup>14</sup>This is in line with the theoretical literature, which suggests three areas where ambiguity effects may be particularly important. First, ambiguous assets may command a return premium and therefore trade at lower prices, as suggested by models that attempt to explain the equity premium (Epstein and Wang, 1994; Maenhout, 2004; Cao et al., Winter 2005; Leippold et al., 2008). Second, higher uncertainty about ambiguous assets may result in less trading (lower liquidity) and higher price volatility, as theoretical literature on portfolio inertia, excess volatility and amplification effects suggests (Epstein and Wang, 1994; Routledge and Zin, 2009; Guidolin and Rinaldi, 2010; Illeditsch, 2011). Third, some traders may overweight their portfolio with less ambiguous assets, as implied by models on the home bias and the familiarity bias (Uppal and Wang, 2003; Huang, 2007; Cao et al., 2011).

and market. The first part of Table 2 reports the corresponding results, including averages across markets. A positive difference indicates ambiguity aversion, because the market price of A is subtracted from R. The p-values refer to two-sided Wilcoxon signed rank tests of the Null that the differences are equal to zero. As Table 2 shows for DA, no statistically significant differences in prices exist with a single exception in Period 3. In a robustness check we also compare the prices of R in DA3 with A in DA4, but do not find any ambiguity effects.<sup>15</sup>

The results of the DA also apply to the CM. The second part of Table 2 reports the corresponding clearing price differences between R and A. Again, no statistically significant price differences can be detected.

Note that the differences for the DA are medians of a four minute trading period. Learning through market feedback could therefore be an explanation for the insignificant results in the DA, even in the very first trading period. However, if learning eliminates ambiguity effects, we would expect differences in prices in the early periods of the CM. In the CM setting, market feedback about prices is only available at the end of a period, which implies that, if ambiguity effects exist at the outset, they cannot be reduced by learning in the first period, but only across periods. Although the average difference in the very first period in the CM is relatively high (61.83), it is not statistically significant. This provides little support for learning as an explanation for missing price differentials.<sup>16</sup>

As market prices are formed by the marginal trader it is possible that ambiguity effects primarily manifest themselves in the order book, rather than transaction prices. For example, even if the transaction prices for R and A are equal, it is possible that the bids for R are all fairly close to the clearing price (indicating relatively homogenous risk preferences), while higher heterogeneity in ambiguity preferences creates an order book for A where bids deviate more strongly downwards. This notion is in line with Bossaerts et al. (2010), who report a high heterogeneity in traders' ambiguity preferences. To test possible ambiguity effects on order books we compute the median bid difference and the median ask difference between R and A in CM. As the results in Table 2 (third and fourth part) show, we find no statistically significant differences in asks and only one period with differences in bids, which are, however, negative. Again, as observed with transaction prices, we also find for bids and asks that the differences in the very first period are positive and relatively high (compared to subsequent periods), but not statistically significant.

<sup>15</sup>For brevity, we only report simultaneously traded assets in Table 2. Detailed results on DA3 and DA4 are available upon request.

<sup>16</sup>For robustness, we also analyzed the trading pattern in the first period of the DA on a minute-by-minute basis, but did not detect any ambiguity effects in the first or subsequent minutes.

Hence, overall, we can conclude that we do not find any ambiguity effects on market prices, bids, or asks. This applies to double auctions and to call markets.

### 3.2 Volatility and volume

To test ambiguity effects on price volatility we compute the differences between the standard deviation of prices of R and A in DA1 and DA2.<sup>17</sup> Table 3 reports the results per period and market ( $s_R - s_A$ ). Overall, we find no statistically significant differences between the volatility of R and A (with Period 3 as only exception).

Table 3 also reports the differences between the volume of R and A in simultaneous trading in DA and in CM ( $q_R - q_A$ ). A positive/negative number indicates that more risky/ambiguous shares have changed hand. In the DA markets we find only two out of twelve periods with statistically significant differences in trading volume: in Period 6 and Period 10 more ambiguous (not risky) assets were traded. In the CM treatments there is not a single period with significant differences in trading volume.

### 3.3 Portfolio composition

As the total amount of shares in the market is constant, we focus on differences in the distribution of shares in traders' portfolios. Specifically, if ambiguity leads to greater disagreement about the value of assets, some traders underweight/overweight ambiguous assets to a greater extent than risky assets.

We use two measures for possible differences in portfolio composition. First, we compute the difference of the standard deviation of end-of-period share holdings in R and A ( $dis_R - dis_A$ ). Negative values result from a more heterogeneous distribution of ambiguous shares in traders' portfolios, indicating higher disagreement about the value of A. Second, as an alternative measurement, we compute the difference of the share holdings of the two traders with the highest number of shares in their end-of-period inventory ( $h_R - h_A$ ). Again, negative values indicate more polarized holdings of the ambiguous assets.

Table 4 shows the results per period and market for simultaneously traded assets in DA and CM. We find not a single period in which the end-of-period distribution of R significantly differs from A.

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<sup>17</sup>Note that CM treatments do not provide continuous trading data for intra-period volatility.

### 3.4 Robustness checks: individual decision making and operationalization of ambiguity

Given that we hardly find any ambiguity effects in markets, it is important to test whether our operationalization of ambiguity is sufficient. In Session BDM1 we therefore elicited subjects' WTP with a BDM mechanism (as described in Section 2.5) and computed the individual bid differentials between assets R and A. Table 5 reports the average per period across all subjects. As expected, the average bids for A are lower in all periods ( $Bid_R - Bid_A > 0$ ), and in the majority of periods this difference is also statistically significant. Moreover, the average differential across all periods and markets in Session BDM1 is also statistically significant.

Although these results show that our operationalization works in individual decision making, it could still be possible that alternative operationalizations of ambiguity used in related studies are even stronger. The latter could explain why, for example, Sarin and Weber (1993) find ambiguity effects in markets, while we do not. To test this we implemented a compound lottery (used by, e.g., Sarin and Weber, 1993) in BDM2. Table 5 reports the average bid differentials between R and A across all subjects. Although the average bids for A are lower in most of the periods (with the exception of Period 12), only one of them is statistically significant.

Hence, overall, our primary operationalization of ambiguity by using additional subjects is not only sufficient to generate ambiguity effects in individual decision making, but also stronger than alternative methods used in related studies.

## 4 Conclusion

Sarin and Weber (1993) conclude 'that the market forces alone may not be sufficient to wash out the effects of ambiguity on decisions' (p. 616). Our results, however, suggest that market forces may indeed be sufficient to wash out the effects of ambiguity. This raises the question: which mechanism leads to the elimination of ambiguity effects in markets, in particular with regard to prices? An exhaustive answer goes beyond the scope of this study, but our results provide some indications which mechanisms are *less* likely to be at work.

Market feedback may be an intuitive explanation for the absence of ambiguity effects. The crucial difference between markets and situations of individual decision making is that, in markets, an individual's decision is influenced by decisions of other market participants. As subjects trade they may learn that the fundamental price of the ambiguous asset does not differ from the risky asset,

provided that there is a sufficient number of traders who attach the same value to both assets. Consequently, prices converge, as also found by Sarin and Weber (1993) for some of their independent market settings. Our results, however, do not suggest that learning plays a role, neither within nor across periods. If market feedback within a trading period would have been the crucial mechanism, we would have expected to see ambiguity effects in the CM, but not in the DA. Market feedback across periods would have eliminated ambiguity effects in later periods in the CM, but not in the first period. We do not find any ambiguity effects in the CM, not even in the first period.

Anticipation of (equal fundamental) asset prices in markets could explain missing ambiguity effects in the first period of the CM. If this is true, we would expect equal bids for risky and ambiguous assets in the individual WTP. Both the BDM mechanism and the CM are incentive compatible. If a subject anticipates a fundamentally equal price for risk and ambiguity, she should bid accordingly in both the CM and BDM treatment. We do find differences between the two treatments, however, with significant ambiguity effects in the latter, but not in the former.

Another feature of many asset markets is that multiple items can be traded while most individual decision making situations consider only one item. With multiple items the main effect of ambiguity may be hidden in other dimensions than prices, such as portfolio choices. Bossaerts et al. (2010) provide some support for this notion, but also acknowledge "a somewhat surprising state of affairs: much of asset pricing theory claims to make sharp predictions about prices but much less sharp predictions about portfolio choices." (p.1355) In line with the latter, we do not observe ambiguity effects on portfolios, and also not on other dimensions, such as trading volume, order books, or volatility.

Overall, our results call for caution in the analysis and interpretation of ambiguity effects in asset markets. This study suggests that the explanatory potential of ambiguity preferences in financial markets lies in settings that are less straightforward than our experiment.

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## Tables and Figures

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Table 1: Overview of sessions

Session	# Markets	# Subjects	ø Pay (Euros)	Ambiguity
DA 1	3	$3 \times 8 = 24$	21.58	strong
DA 2	3	$3 \times 8 = 24$	19.42	strong
DA 3	3	$3 \times 8 = 24$	22.04	strong
DA 4	3	$3 \times 8 = 24$	19.17	strong
CM 1	3	$3 \times 8 = 24$	19.96	strong
CM 2	3	$3 \times 8 = 24$	22.90	strong
BDM 1	N/A	20	16.55	strong
BDM 2	N/A	12	16.85	weak

Table 2: Differences between R and A: prices, bids, asks

The table reports differences between the risky asset (R) and of the ambiguous asset (A) in the double auction (DA) and call markets (CM) for each market where R and A are traded simultaneously (Sessions DA1, DA2, CM1, CM2) and as a period average.  $P_R - P_A$  is the difference of clearinghouse prices from CM and of median prices from DA.  $B_R - B_A$  is the median bid difference and  $A_R - A_A$  the median ask difference between R and A in CM. Bids and asks are only included when a subjects submitted quotes for both assets. The p-value refers to a Wilcoxon signed rank test of the null hypothesis that the difference is equal to zero (that the measure of interest does not differ between assets R and A).

	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12
DA: $P_R - P_A$												
M1	49	-3	10	20	-20	-5	-10	-16	0	5	-22	0
M2	-15	-8	17	-26	-10	1	-9	0	1	26	2	-10
M3	0	5	5	-1	0	9	-5	-2	-5	-5	-9	0
M4	60	17	12	-5	-16	13	0	2	2	0	-18	-45
M5	30	30	10	10	15	15	10	10	9	6	0	0
M6	0	-9	0	-3	-2	2	0	0	1	0	0	4
Average	20.50	5.50	9.00	-0.58	-5.33	5.83	-2.25	-1.00	1.33	5.25	-7.75	-8.42
p-value	0.196	0.600	0.035	0.753	0.206	0.116	0.449	0.829	0.291	0.234	0.196	0.425
CM: $P_R - P_A$												
M1	-89	-5	-20	5	7	-10	-2	0	7	-7	0	0
M2	60	-60	-16	-40	-25	-45	-25	0	-10	1	1	0
M3	100	-49	12	0	9	1	7	0	0	3	1	0
M4	0	0	0	-15	-5	0	0	0	0	0	15	35
M5	240	0	20	-100	70	-50	0	-5	-20	10	-15	0
M6	60	0	20	10	30	45	-10	70	0	20	30	-20
Average	61.83	-19.00	2.67	-23.33	14.33	-9.83	-5.00	10.83	-3.83	4.50	5.33	2.50
p-value	0.205	0.087	0.670	0.292	0.249	0.459	0.281	0.898	0.425	0.206	0.244	0.898
CM: $B_R - B_A$												
M1	0	0	0	-5	0	0	0	0	0	0	0	0
M2	-180	10	-50	-13	-25	0	-10	0	0	-15	0	0
M3	180	-75	25	1	0	0	0	3	5	-1	0	0
M4	0	0	0	0	-5	0	0	0	0	0	0	0
M5	75	0	0	-50	0	0	0	0	0	0	0	0
M6	0	0	0	0	0	0	0	0	0	0	0	0
Average	13	-11	-4	-11	-5	0	-2	0	1	-3	0	0
p-value	0.760	0.567	0.188	0.051	0.035	0.765	0.223	0.978	0.540	0.536	0.302	0.331
CM: $A_R - A_A$												
M1	0	0	0	0	0	0	0	0	0	0	0	0
M2	-10	20	-23	0	0	0	3	0	0	0	0	0
M3	85	0	40	0	0	0	0	5	0	0	0	0
M4	0	0	0	-35	0	0	0	0	3	0	0	0
M5	100	50	-75	0	0	0	0	0	0	0	0	0
M6	0	0	-15	0	0	0	0	0	0	0	0	0
Average	29	12	-12	-6	0	0	0	1	0	0	0	0
p-value	0.782	0.143	0.142	0.835	0.283	0.375	0.073	0.841	0.322	0.603	0.230	0.522

Table 3: Differences between R and A: volatility and volume

The table reports differences between the risky asset (R) and of the ambiguous asset (A) in the double auction (DA) and call markets (CM) for each market where R and A are traded simultaneously (Sessions DA1, DA2, CM1, CM2) and as a period average.  $s_R - s_A$  is the difference between the standard deviation of prices of R and A in DA.  $q_R - q_A$  refers to the difference between the volume of R and A in DA and in CM. The p-value refers to a Wilcoxon signed rank test of the null hypothesis that the difference is equal to zero (that the measure of interest does not differ between assets R and A).

	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12
DA: $s_R - s_A$												
M1	6	54	2	3	-12	7	1	-10	-36	-10	-15	12
M2	65	6	56	-1	12	-8	-13	14	-2	-10	16	-17
M3	2	-9	3	-4	-5	-18	1	0	1	3	1	0
M4	-37	11	8	-2	1	-56	64	1	-7	-3	-7	17
M5	45	0	2	-6	18	13	-1	0	38	-3	0	-4
M6	4	10	0	2	-5	-3	-1	15	0	1	-1	1
Average	14	12	12	-2	2	-11	9	4	-1	-4	-1	2
p-value	0.173	0.116	0.028	0.345	0.917	0.917	0.345	0.753	0.600	0.173	0.753	0.753
DA: $q_R - q_A$												
M1	13	9	34	15	-1	-10	1	-2	-24	-4	12	-5
M2	3	1	-4	-2	3	-12	-10	-2	-4	-10	13	8
M3	-15	9	-14	1	10	-11	2	0	12	-2	4	-5
M4	0	-3	-4	-10	7	-4	-5	8	-17	-4	5	-4
M5	-3	-6	6	0	-2	-2	16	-9	-6	-10	-4	17
M6	-4	2	-4	9	6	-6	-2	-2	12	-3	-4	15
Average	-1	2	2	2	4	-8	0	-1	-5	-6	4	4
p-value	0.597	0.462	0.916	0.673	0.116	0.028	0.834	0.286	0.462	0.027	0.168	0.344
CM: $q_R - q_A$												
M1	1	-3	1	0	2	-2	1	3	1	4	2	0
M2	2	-9	-18	-8	-8	2	-5	3	-7	-1	-3	3
M3	8	-4	1	0	2	-4	0	5	12	-5	3	4
M4	-2	-5	4	1	-7	-4	-2	-2	6	-7	4	-6
M5	2	10	9	-2	3	0	-1	-6	-10	3	0	-6
M6	4	1	9	-10	12	-4	4	10	-7	0	9	-5
Average	3	-2	1	-3	1	-2	-1	2	-1	-1	3	-2
p-value	0.112	0.463	0.343	0.196	0.753	0.109	0.597	0.344	0.753	0.527	0.169	0.291

Table 4: Differences between R and A: distribution of shares

The table reports differences between the risky asset (R) and of the ambiguous asset (A) in the double auction (DA) and call markets (CM) for each market where R and A are traded simultaneously (Sessions DA1, DA2, CM1, CM2) and as a period average.  $dis_R - dis_A$  is the difference of the standard deviation of end-of-period share holdings.  $h_R - h_A$  is the difference of the share holdings of the two traders with the highest number of shares in their end-of-period inventory. The p-value refers to a Wilcoxon signed rank test of the null hypothesis that the difference is equal to zero (that the measure of interest does not differ between assets R and A).

	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12
CM: $dis_R - dis_A$												
M1	-1.22	-1.14	0.79	1.14	-0.64	-1.23	0.05	0.61	1.72	0.65	0.95	2.12
M2	0.86	-4.55	-3.42	-1.13	-1.75	2.11	-1.49	0.80	-3.89	-0.09	-1.20	-0.03
M3	0.32	0.74	-0.86	-0.74	-0.27	0.48	-0.99	2.22	1.03	-1.08	0.24	-0.53
M4	-1.09	-2.33	-0.55	0.38	0.80	-1.59	-2.35	-1.92	0.30	-0.69	1.39	-1.06
M5	0.64	1.46	1.97	-2.15	2.36	-0.37	0.82	-3.28	-3.40	1.21	-1.26	-1.86
M6	2.16	-2.74	2.22	-3.12	3.54	-2.45	0.22	2.94	-1.87	0.71	3.00	-2.19
Average	0.28	-1.43	0.02	-0.94	0.67	-0.51	-0.62	0.23	-1.02	0.12	0.52	-0.59
p-value	0.753	0.173	0.917	0.249	0.463	0.463	0.345	0.753	0.345	0.753	0.463	0.249
DA: $dis_R - dis_A$												
M1	0.48	-1.96	0.40	-0.74	1.27	1.55	-2.92	-3.17	0.10	0.58	0.98	-0.03
M2	-0.88	-0.09	-0.19	1.95	1.86	0.87	1.19	-0.26	2.15	-1.09	-0.93	0.42
M3	-2.44	-1.10	-1.27	0.33	2.98	3.04	2.37	-0.38	0.22	0.33	-0.79	0.93
M4	0.89	0.75	-1.77	-3.15	1.81	1.47	-2.33	2.82	-3.78	0.40	1.75	-3.73
M5	0.00	4.06	1.36	-3.06	1.84	-1.48	-0.82	0.41	2.08	-0.40	0.03	0.93
M6	0.76	-0.42	-0.93	1.54	-1.53	-1.72	-0.56	-1.48	-0.70	-1.93	-0.09	0.88
Average	-0.20	0.21	-0.40	-0.52	1.37	0.62	-0.51	-0.34	0.02	-0.35	0.16	-0.10
p-value	0.932	0.866	0.352	0.499	0.063	0.499	0.499	0.499	0.612	0.446	0.612	0.498
CM: $h_R - h_A$												
M1	-2.44	-1.10	-1.27	0.33	2.98	3.04	2.37	-0.38	0.22	0.33	-0.79	0.93
M2	0.89	0.75	-1.77	-3.15	1.81	1.47	-2.33	2.82	-3.78	0.40	1.75	-3.73
M3	0.00	4.06	1.36	-3.06	1.84	-1.48	-0.82	0.41	2.08	-0.40	0.03	0.93
M4	0.76	-0.42	-0.93	1.54	-1.53	-1.72	-0.56	-1.48	-0.70	-1.93	-0.09	0.88
M5	-0.20	0.21	-0.40	-0.52	1.37	0.62	-0.51	-0.34	0.02	-0.35	0.16	-0.10
M6	0.93	0.87	0.35	0.50	0.06	0.50	0.50	0.61	0.45	0.61	0.50	0.50
Average	-0.01	0.73	-0.44	-0.73	1.09	0.40	-0.23	0.25	-0.26	-0.25	0.28	-0.10
p-value	0.673	0.463	0.345	0.463	0.116	0.753	0.463	0.600	0.917	0.834	0.463	0.462
DA: $h_R - h_A$												
M1	-0.03	-0.11	0.03	0.09	-0.08	-0.05	0.03	0.05	0.14	0.03	0.05	0.09
M2	0.00	-0.25	-0.22	-0.06	-0.13	0.09	-0.02	0.05	-0.25	0.05	-0.06	-0.05
M3	0.05	0.08	-0.05	-0.05	-0.05	0.03	-0.03	0.13	0.06	-0.03	0.00	-0.02
M4	-0.08	-0.09	-0.06	0.08	0.06	-0.06	-0.09	-0.09	0.05	-0.08	0.06	0.00
M5	0.00	0.03	0.11	-0.08	0.17	-0.02	0.03	-0.20	-0.25	0.05	-0.06	-0.16
M6	0.09	-0.11	0.09	-0.09	0.19	-0.11	0.00	0.14	-0.11	0.02	0.20	-0.08
Average	0.01	-0.08	-0.02	-0.02	0.03	-0.02	-0.01	0.01	-0.06	0.01	0.03	-0.03
p-value	0.829	0.115	0.917	0.752	0.600	0.463	0.670	0.753	0.462	0.670	0.670	0.292

Table 5: Differences between R and A: bids in BDM

The table reports the average of differences between bids (WTP) for the risky asset (R) and for the ambiguous asset (A). The first row shows the averages in Session BDM1 with our primary operationalization of ambiguity, and the third row reports the averages in Session BDM2 with ambiguity as a second-order probability (compound lottery). The p-value refers to a Wilcoxon signed rank test of the null hypothesis that the difference is equal to zero (that the average bid does not differ between assets R and A).

	Period 1	Period 2	Period 3	Period 4	Period 5	Period 6	Period 7	Period 8	Period 9	Period 10	Period 11	Period 12
BDM 1: $Bid_R - Bid_A$	200	65	168	36	279	65	240	68	164	31	123	162
p-value	0.017	0.287	0.025	0.476	0.001	0.169	0.003	0.337	0.005	0.383	0.004	0.015
BDM 2: $Bid_R - Bid_A$	114	115	10	1	117	83	107	142	202	152	156	-14
p-value	0.170	0.204	0.813	0.809	0.305	0.325	0.195	0.176	0.016	0.132	0.100	0.843

## Appendix

Figure 1: Screenshot of DA split screen

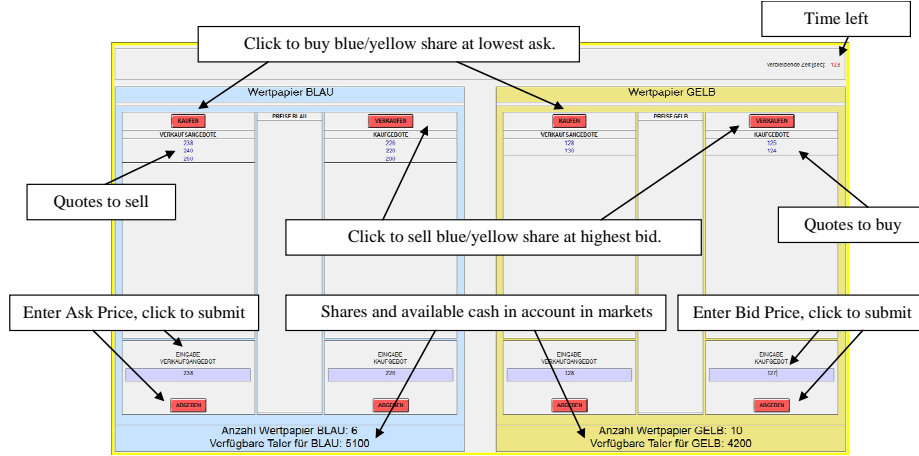


Figure 2: Screenshot of CM split screen

Runde: 1

**Blue Section (Left):**

- # of blue shares to buy: 3
- Highest price to buy: 150
- Anzahl Wertpapier BLAU: 6
- Verfügbare Taler für BLAU: 5100

**Yellow Section (Right):**

- # of yellow shares to sell: 10
- Lowest price to sell: 110
- Anzahl Wertpapier GELB: 10
- Verfügbare Taler für GELB: 4200

Shares and available cash in account in markets

OK

Figure 3: Screenshot of BDM split screen

**Yellow Section (Left):**

- Wertpapier GELB
- Maximum bid for yellow/blue share: 2800
- Verfügbare Taler für GELB: 3000

**Blue Section (Right):**

- Wertpapier BLAU
- Maximum bid for yellow/blue share: 1250
- Verfügbare Taler für BLAU: 3000

Available amount of Taler for yellow/blue share

OK

## Sample of instructions of DA1 and DA2

(not for publication)<sup>18</sup>

Welcome to the experiment. You participate in this experiment to earn money. Accordingly, you should try to maximize the payoff in this experiment. If you follow the instructions carefully, and make good decisions, you will receive a significant amount in cash. In this experiment you trade shares. All transactions are calculated in “Taler”. At the end of the experiment your total amount earned in Talers will be exchanged and paid to you at the following exchange rate: 200 Talers = 1 Euro.

### 1. Trading in the market

Trading of shares takes place on a market platform. Thus, you first have to get experienced with the trading platform. For the trading actions you need Talers and shares. You find your inventory in Talers and shares in the field below the trading platform.

If you want to sell a share, you can use the text box “ENTER SELLING PRICE”. In this text box you enter the price at which you are willing to sell the share. Afterwards click on the “SUBMIT” button below the text box. Please do this right now (You can type in any arbitrary price)! Once you have done this, you will recognize that there are 8 prices (one of each participant) recorded in the schedule named “QUOTES TO SELL” on the left-hand side. The lowest quote is listed in the first row and is emphasized. If you click the “BUY” button you will buy a share at the currently lowest selling price. If you first select an other price on the list by clicking on it, you will buy the share at the selected price. Please buy a share now by clicking on a price and then on the “BUY” button. Now, everybody should own the same amount of shares as in the beginning, because all of you have offered one share to sell and all of you have purchased one share.

If you want to buy a share, please use the text box “ENTER BUYING PRICE”. In this text box you enter the price at which you are willing to buy the share. Afterwards, click on the button “SUBMIT” below the text box. Please, do this right now (You can type in any arbitrary price)! Once you have done this, you will recognize that there are 8 prices (one of each participant) recorded in the schedule named “QUOTES TO BUY” on the right-hand side. The highest quote is listed in the first row and it is emphasized. If you click the “SELL” button you will sell a share at the currently highest buying price. If you first select an other price on the list by clicking on it, you will sell the share at the selected price. Please sell a share now by clicking on a price and then on the “SELL” button. Now, everybody should own the same amount of shares as in the beginning, because all of you have bought one share and all of you have sold one share.

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<sup>18</sup>Translated from German and formatted in a more compact way than in the actual hand-outs for the subjects. Note that participants saw the trading screen and interacted with it while the instructions were read out aloud.



Remember: If you buy a share, your funds decrease by the purchase price and your amount of shares increase. If you sell a share, your funds increase by the purchase price and your amount of shares decrease. The corresponding prices will be recorded in the list “PRICES”. The sequence of the prices will depend on the point in time when the prices are set.

At first there will be a 2-minutes trial period. The duration of the period will be displayed in the upper right corner of the screen. None of your actions in the trial period will affect your payoff. Thus, these actions will not impact your starting position in the experiment. The only goal is to get experienced with the trading platform. Make sure that you successfully submitted bids to buy and to sell. Furthermore ensure that you at least accepted one bid to buy and one bid to sell. Please feel free to ask questions during the trial period. Thereafter (in subsequent periods) you will always be given the possibility to ask questions. Keep in mind: The better you understand the trading platform, the more you can focus on other important aspects of the experiment.

## **2. Procedure in one period**

In each period you will trade two types of shares in two separate markets: share BLUE and share YELLOW (this is different from the trial period). The markets will be open for exactly 4 minutes. At the beginning of each period you will be given a separate endowment of shares and Talers. All the shares you own at the end of the period will pay you either 300 or 124 Talers each. This payout will be randomly determined (for more detailed information see Section 3). At the end of each period you will be able to see the computation of the period's payoff. In total there will be 12 periods.

## **3. Share payoff**

After each period the payoffs of the two shares will be determined. For this, a bag filled with marbles will be provided for each share.

Share BLUE: For share BLUE every bag will contain 8 marbles with 8 different colors (white, red, purple, blue, black, brown, bright green, dark green).

Share YELLOW: Each of the bags might contain 1 to 8 different colors (white, red, purple, blue, black, brown, bright green, dark green). There will be a different bag for each period, i.e., a total of 12 different bags. For each bag all color combinations are possible. Thus, it might happen that one bag contains exactly 8 marbles of the same color. However, another possibility is that there is one bag which contains exactly one marble of all 8 colors. All combinations between these two examples might also be possible.

As you have seen at the beginning of the experiment, the bags for share YELLOW have been composed by four randomly picked persons. Each of these persons received three empty bags and could pick eight marbles of up to eight colors. Thus, these persons could fill all of these bags with an arbitrary composition of the eight colors. Afterwards the bags will be closed, so that neither you nor the experimenters will know the composition of the marbles in the bags. To ensure that there are exactly

eight marbles in each of the bags, the experimenter will count the marbles through the fabric of the bags. You can also check this after the experiment yourself if you wish. The persons who have composed these bags will not take part in the experiment. In total there are 12 bags (three bags filled by each of the four persons) which will be randomly assigned to the 12 periods before we start with the trading.

After each period, each share's value will be determined by the following procedure: At the beginning of the experiment, you have marked four colors for the share BLUE and four colors for the share YELLOW. The selections of colors of the whole group (12 for share BLUE and 12 for share YELLOW) will be randomly assigned to the 12 periods. After each period, one participant will draw exactly one marble out of the 'BLUE bag' to for the BLUE share, and one participant will draw exactly one marble out of the 'YELLOW bag' (that was assigned to that period) for the YELLOW share. If the drawn marble's color equals one of the four marked colors, then the share's value is 300 Talers. Otherwise the value is 124 Talers. The value will be entered in the computer by the supervisor and is shown on your screen in the payoff computation.

#### 4. Endowment and payoff

At the beginning of each period you will get an endowment as shown in the table below. Four participants receive endowment type I and four participants endowment type II. These types will be randomly assigned by the computer at the beginning of in each period. You will have to refund 3000 Talers at the end of each period.

endowment type	Blue market		Yellow market	
	number of shares	available Talers	number of shares	available Talers
I	10	4200	6	5100
II	6	5100	10	4200

You can use the Talers in the BLUE market only for trading in the BLUE market, and the Talers in the YELLOW market only for trading in the YELLOW market. At the end of each period, your total Taler payoff is: available Talers at the beginning of a period + amount of shares  $\times$  share payoff (300 or 124) + Talers earned by selling shares - Talers spent on buying shares - refunding of 3000 Talers.

At the end of the experiment, only one period will be paid out in cash. This period will be determined by a throw of a 12-sided dice. Furthermore, only one market (BLUE or YELLOW) will be paid out. This market will be determined by a coin toss. Thus, there is exactly one period (out of all 12 possible periods) of exactly one market (of share BLUE or YELLOW) which will be paid out to you. Keep in mind that every decision for each share in each period might be essential for your actual payment! You receive an additional show-up fee of 4 Euros for your participation in the experiment. These 4 Euros will be added to your cash payment. The final payment in cash is determined as:  $\text{Payment (in Euro)} = \text{Taler payoff}/200 + 4 \text{ Euros (show-up fee)}$ .