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The role of the end time in experimental asset markets[☆]

Anita Kopányi-Peuker[a,1], Matthias Weber[b,*]

[a] Institute for Management Research, Radboud University Nijmegen, The Netherlands
[b] School of Finance, University of St. Gallen, and Swiss Finance Institute, Switzerland

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ABSTRACT

There are hundreds of scientific articles on experimental asset markets. Almost all of them use a short and definite horizon. This may be one of the starkest differences between experimental settings and real-world financial markets, which usually have indefinite and comparatively long horizons. We analyze the implications of different end time assumptions in an asset market experiment in which we vary the length of the horizon and whether the end time is definite or indefinite. We find very similar price dynamics with recurring bubbles in all treatments.

1. Introduction

Experiments have become an important tool when analyzing human interaction in financial markets.[2] The reason for this is the control that experiments offer. The usual problems that exist with observational data (such as omitted variable bias or reverse causality) can be controlled in a laboratory setting: the randomization of treatments in general allows researchers to make causal claims about treatment effects. In addition, it is possible to observe fundamental values, which is in general not possible outside the laboratory. While it may seem obvious to some that price bubbles can exist in actual financial markets (e.g., the Dutch Tulip Bubble, the South Sea Bubble, or the Dotcom Bubble), others may deny the existence of bubbles and the possibility to identify a bubble

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* Correspondence to: Swiss Institute of Banking and Finance, University of St. Gallen, Unterer Graben 21, 9000 St. Gallen, Switzerland.
E-mail addresses: anita.kopanyi-peuker@ru.nl (A. Kopányi-Peuker), matthias.weber@unisg.ch (M. Weber).

1 Mailing address: Nijmegen School of Management, Postbus 9108, 6500HK, Nijmegen, The Netherlands

2 There is by now a vast literature on experimental asset markets. Reviews of the literature can, for instance, be found in Bossaerts (2009), Palan (2013), or Powell and Shestakova (2016). Recent contributions include Cheung and Palan (2012), Kirchler et al. (2012), Sutter et al. (2012), Huber and Kirchler (2012), Cheung et al. (2014), Füllbrunn et al. (2014), Noussair et al. (2016), Bao et al. (2017), Holt et al. (2017), Bosch-Rosa et al. (2018), Hanaki et al. (2018), Weber et al. (2018), Charness and Neugebauer (2019), Ahrens et al. (2020), Huber et al. (2020), Tucker and Xu (2024), Weitzel et al. (2020), Corgnet et al. (2021), Hommes et al. (2021), Lambrecht et al. (2024), and Weber et al. (2024).

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while prices are rising or at their peak (rather than just classifying bubbles ex post). As fundamentals are unobservable, prices might be justified by expectations at any point in time, and the fact that prices decrease at some point do not prove that these expectations were incorrect ex ante. Indeed, Nobel Memorial Prize in Economic Sciences laureate Eugene Fama stated: "I don't even know what a bubble means. These words have become popular. I don't think they have any meaning."[3] In the laboratory, fundamental values can be observed and substantiated claims about price accuracy can be made in a straightforward manner.

Most of the experimental literature relies on definite horizons and a relatively low number of periods of about ten to twenty. This could be considered one of the greatest differences to actual financial markets. While a financial asset in the real world may not exist forever, such assets usually last for a long time and it is uncertain when they cease to exist (they thus have an indefinite ending). Whether the end time of an asset is definite or indefinite could have considerable implications for how people price assets. It is, for instance, straightforward to conduct backward induction when the end time is definite, especially with a short planning horizon. In the world outside the laboratory, we often think of equity markets when talking about bubbles and not about bond markets — the reason may be that bonds generally have a fixed end time (although risky bonds might default before this fixed end time). The observations from real financial markets lead us to this experiment analyzing which role the definiteness and the length of the time horizon play for the pricing of assets in the laboratory, which we view mainly as a methodological contribution.

Compared to the literature with definite horizons, there are only very few papers with indefinite horizons. As the experimental asset market literature is by now very large and diverse, it is not surprising that there are nevertheless already some papers that make use of an indefinite end time in the laboratory (e.g., Hirota and Sunder, 2007; Hens and Steude, 2009; Holmen et al., 2014; Asparouhova et al., 2016; Fenig et al., 2018; Crockett et al., 2019; Duffy et al., 2024). However, hardly any studies compare markets with an indefinite end to the same markets with a definite end (the focus in the literature usually lies on comparing different treatments with indefinite ends). Concerning the length of the horizon, there are some studies with more periods than the usual ten to twenty (e.g., Lahav, 2011; Hoshihata et al., 2017), but surprisingly there are basically none that vary the length of the horizon in a controlled way (an exception is Razen et al., 2017, who have markets with eight and 14 periods). To the best of our knowledge, there is no study varying the horizon length with indefinite end times (or with repeated markets to make the analysis of learning across repetitions possible).

To preview our results, we find no statistically significant effect of whether the end is definite or indefinite. This holds for the extent of overpricing, as well as for the degree of pricing improvements across rounds and for the speeding up of bubbles (that is, bubbles appearing earlier in a round in later rounds of the experiment). We similarly find no statistically significant effect of the length of the trading horizon. This again holds for the extent of overpricing, as well as for the degree of pricing improvements across rounds and for the speeding up of bubbles. In all treatments, we find recurring bubbles, only slight improvements of pricing across rounds, and bubbles that appear a bit earlier in later rounds of the experiment.

We consider our null results as confirmation for the findings from the literature on asset market experiments. Given that we find no qualitative differences in pricing behavior across the different treatments, it is unlikely that the results obtained in the literature, which are usually treatment comparisons, are artifacts of definite and relatively short time horizons.

This paper is organized as follows. Related literature is discussed in Section 2. We present the experimental design in Section 3. Section 4 contains the results, and Section 5 concludes.

2. Related literature

There are already several studies making use of an indefinite horizon. The most closely related studies are discussed below and all studies with an indefinite horizon that we are aware of are summarized in Table 1 (this table draws heavily on a similar table in Duffy et al., 2024).

One of the first studies using an indefinite horizon is Camerer and Weigelt (1993). In this study, eight to 12 participants form a market and trade a stochastically lived asset by an oral double auction. Assets pay cyclically three different dividend values creating an incentive to trade. At the end of each period, it is decided whether the asset exists for another period with a probability of 85%. Markets vary certain dimensions (e.g., whether the dividend structure is common knowledge) but not the termination of the market. They find that inexperienced participants do not learn to price assets close to the constant fundamental value.

The first study that uses a definite and indefinite ending in the same experiment is Hirota and Sunder (2007). However, they do not use the same type of terminal value in their different treatments. They use a terminal value determined by a random draw from a known probability distribution in their treatment with definite ending. In contrast, in their treatment with indefinite ending, they use a terminal value that is determined by predicted market prices, where the predictions are made by other participants in the laboratory. They do so, as their focus is on investigating the effects of investors' trading horizons, arguing that long-horizon investors trade based on the expected payoff of an asset at maturity, while short-horizon investors trade based on expected market prices of an asset in the short term. This paper therefore does not compare the effect of definite and indefinite ending on pricing, but uses the different endings to motivate different payoffs when the markets end (in order to investigate a different research question). In their treatment with indefinite ending, participants are told that trading would end after period 30 the latest (and likely before), but not the exact ending time or rule.[4]

[3] This quote is from an interview in 2010 with John Cassidy for The New Yorker: https://www.newyorker.com/news/john-cassidy/interview-with-eugene-fama.

[4] A similar implementation of an indefinite ending is used by Kopányi-Peuker and Weber (2021a), where participants are only told that markets end between periods 25 and 40.

Table 1
Experimental asset market experiments with indefinite horizons.

Study	Market setting	Continuation probability	Results	Variation def/indef
Camerer and Weigelt (1993)	Double auction, random termination	0.85	Substantial mispricing	No
Ball and Holt (1998)	Double auction, random asset reduction	5/6[a]	Bubble-crash pattern	No
Hirota and Sunder (2007)	Double auction, different trading horizon	Unknown[b]	Accurate pricing for known terminal value distribution, mispricing without	Yes[c]
Hens and Steude (2009)	Double auction, leverage effect	0.97	Underpricing	No
Kose (2015)	Double auction, bankruptcy risk	0.875	Moderate underpricing	Yes[c]
Holmen et al. (2014)	Double auction, different incentives	Varying[d]	Overpricing	No
Asparouhova et al. (2016)	Double auction, test of the Lucas model, random termination	5/6	Fundamentals depend on risk attitude, no clear bubble result	No
Fenig et al. (2018)	Call market in a production economy	0.965	Significant overpricing	No
Weber et al. (2018)	Call market, bonds with possible default	Endogenous[e]	Bubbles disappear with experience	No
Crockett et al. (2019)	Double auction, test of the Lucas model, random termination	5/6	Overpricing mainly with linear utility	No
Carbone et al. (2021)	Double auction, test of the Lucas model, random termination	5/6	No significant mispricing	No
Kopányi-Peuker and Weber (2021a)	Learning-to-forecast and call markets	Unknown[b]	Recurrent bubbles, unless low C/A ratio	No
Halim et al. (2022)	Double auction, test of the Lucas model, random termination	5/6	Overpricing in all treatments	No
Duffy et al. (2024)	Double auction, random termination	0.9	Underpricing on average	Yes
Weber et al. (2024)	Call market, bonds+CDS, default possible	Endogenous[e]	Accurate pricing for bonds, not for CDS	No
This paper	Call market with definite and indefinite ending	0.9 after fixed number	Recurrent bubbles, no treatment differences	Yes

[a] The continuation probability is applied for each individual asset independently with a fixed number of trading periods. Classroom experiment.
[b] Participants only know that the number of periods is below 30 (Hirota and Sunder, 2007) or between 25 and 40 (Kopányi-Peuker and Weber, 2021a).
[c] No clean treatment variations due to different terminal values (Hirota and Sunder, 2007) or due to the comparison data being take from another study, with different subject pool and instructions (Kose, 2015).
[d] Participants know that the market ends with equal probability between periods 8 and 15.
[e] The continuation probability depends on the price in an initial public offering.

Kose (2015) investigates pricing in markets with indefinitely lived assets. The experimental treatments run for this paper use an indefinite ending with different types of fundamental values (constant, decreasing, and increasing fundamental values). The design of the experiment allows a comparison with the data of the flat fundamental treatment of Kirchler et al. (2012), which has a definite ending.[5] Comparing the flat fundamental treatment of Kirchler et al. (2012) with the most comparable treatment of Kose (2015) shows similar behavior in both experiments. In general, prices in the treatments of Kose (2015) are relatively low, assets are mainly underpriced or priced close to the fundamental value, overpricing is rare. This may be related to the low cash-to-asset ratio (the cash-to-asset ratio is not identical across treatments due to the different fundamental value trajectories, but it is equal to one in their treatment with constant terminal value and of the same order of magnitude in the other treatments).

Duffy et al. (2024) also investigate the pricing of indefinitely lived assets. In their baseline treatment, participants trade an asset paying a fixed dividend in each period. The asset ceases to exist after each period with 10% probability. The asset has no terminal value. The authors implement the random termination via block random termination, so that they always observe at least ten trading decisions per market (with three repetitions of a market). In additional treatments, Duffy et al. (2024) provide a comparison of markets with definite and indefinite horizons (this is the only fully clean comparison of a definite horizon and an indefinite horizon in the literature that we are aware of). Their results show very similar pricing of the assets in the two additional treatments providing the clean comparison of end types. Assets in these treatments are priced close to the fundamental value (in the main treatment, assets are priced below the fundamental value, which suggests that the timing of the dividend payments may play a role). A key difference to our study is that they use a low cash-to-asset ratio of one (as the literature suggests that a high cash-to-asset ratio is a precondition for asset price bubbles to emerge, one might argue that the effect of the end time on bubble formation should be studied with a high cash-to-asset ratio). Another difference to our study is that Duffy et al. (2024) use a block random termination implementation of the indefinite ending, as compared to a "live" ending in our experiment. Furthermore, in the additional treatments, they use a two-stage setting, in which the trading happens in a first stage before the dividends are realized in a second stage.

The situation is similar when looking at the length of the trading horizon. Some studies already contain a long trading horizon, but hardly any vary the length of the trading horizon. In Table 2 we summarize all studies that we are aware of with more than 20 trading periods (per repetition of a market if markets are repeated) and all studies with a controlled variation of shorter versus longer horizons (even if the longer version contains fewer than 20 periods).

[5] The data from the flat fundamental value treatment of Kirchler et al. (2012) are referred to as "Treatment 0" in Kose (2015), which might cause confusion (because such a numbering of treatments suggests that the same subject pool, very similar instructions, etc. are used across the differently numbered treatments).

Table 2
Experimental asset market experiments with long horizons or a shorter/longer comparison.

Study	Market setting	Number of periods	Results	Variation shorter/longer
Lahav (2011)	Call market	200	Multiple bubbles and crashes, no common pattern among groups	No
Deck et al. (2014)	Double auction with overlapping generations	25[a]	M-shaped double bubbles	No
Hoshihata et al. (2017)	Call market and continuous double auction	100	Flat bubbles in some groups	No
Razen et al. (2017)	Double auction, constant FV, partly cash injection	8 vs. 14	Overpricing only with long horizon and cash inflow	Yes
Kopányi-Peuker and Weber (2021a)	Learning-to-forecast and call markets	25 to 40	Recurrent bubbles, unless C/A ratio is low	No
Anufriev et al. (2022)	Learning-to-forecast market, different prediction horizons	50[b]	Less trend extrapolation when prediction horizon is longer	No
Anufriev et al. (2024)	Learning-to-forecast market, long horizons	ca. 150	Relatively stable markets in the long run	No
This paper	Call market with short and long horizon (definite + indefinite)	ca. 15 vs. 30	Recurrent bubbles, no treatment differences	Yes

[a] Assets exist for 25 periods, but traders are only active for 10 or 15 periods.
[b] Treatments differ in the prediction horizon: the price in period t depends on the price prediction for period $t+1$, $t+2$, or $t+3$.

We have chosen the threshold of 20 periods based on the classical asset market settings with trade in the laboratory (using a continuous double auction or a call market). Note however, that there are also asset market experiments where participants do not directly trade with each other. In so-called learning-to-forecast experiments, subjects' only task is to submit a price forecast — trades of these assets are then computerized, with trading decisions based on subjects' price forecasts. Learning-to-forecast experiments are usually implemented with 50 periods.[6] As this type of experiment usually involves 50 periods, we only include learning-to-forecast experiments in Table 2 if they deviate from the this setup in certain aspects. None of the learning-to-forecast studies in the table explicitly vary the trading horizon. Anufriev et al. (2022) vary how many periods in advance participants need to forecast the price of the asset, but all markets last for 50 periods. Anufriev et al. (2024) ran long horizon forecasting markets (150 periods). They compare it with the shorter horizon of 50 periods by looking at the first 50 periods of the 150 periods. The main focus of their paper is not on the horizon, however, but time pressure during participants' decision making.

Lahav (2011) implements trade in the laboratory with a very long trading horizon. The author does not implement different treatments but focuses on call markets run for 200 periods in order to investigate what caused bubble-crash patterns in the earlier literature. In this paper, participants face an asset market with a declining fundamental value. Assets pay random dividends and there is no buyback price and no interest rate on cash. The data obtained in this study show no clear pattern that is common for all markets. Market prices track the fundamental value well in some markets, whereas multiple bubbles are observed in other markets.

Hoshihata et al. (2017) analyze and compare call markets and continuous double auctions in a setting with 100 periods. The authors implement a declining fundamental value, with an increasing cash-to-asset ratio (which is typical for experiments with declining fundamental values). The authors find very similar pricing dynamics in call markets and continuous double auctions. They observe several bubbles, including flat bubbles (prices staying considerably above the declining fundamental value for many periods).

Kopányi-Peuker and Weber (2021a) find repeating and relatively flat bubbles in call markets with 25 to 40 periods when the cash-to-asset ratio is high. When the cash-to-asset ratio is low, the assets are priced close to the fundamental value.

The only study explicitly varying the length of the trading horizon is the paper by Razen et al. (2017). Participants of that study trade an asset in a double auction. The asset does not pay any dividends, and cash does not earn any interest. The buy-out price of the asset in the end is random, leading to a constant fundamental value in the market. Treatments differ in two dimensions: first, whether markets have 8 or 14 periods; and second, whether extra cash is injected in the market or not (CASH vs. BASE). Their results show slight overpricing in all treatments, and no differences in the treatments with different length, as long as there is no extra cash injected in the economy (thus, in a setup similar to ours). However, when extra cash is present in the market, overpricing increases, and they observe a more pronounce bubble-crash pattern.

Summarizing the connection of our paper to the existing literature, we thus fill several gaps in the literature. In particular, we provide the first clean treatment comparison of indefinite and definite horizons in a setting with a high cash-to-asset ratio. We also provide the first clean treatment comparison of shorter and longer trading horizons beyond the 8-vs-14 period comparison by Razen et al. (2017). Furthermore, our study is the first to analyze the (expected) length of the trading horizon in markets with indefinite end.

[6] 50 periods are not only the standard in learning-to-forecast experiments with asset markets (e.g., Bao et al., 2017, 2020; Hommes et al., 2021) but also in such experiments with goods markets or macroeconomic settings (e.g., Hommes et al., 2007; Assenza et al., 2021; Hommes et al., 2019).

3. Experimental design

3.1. The call market

Our experiment relies on a call market setting. Six participants form one market (or group), and they can trade assets with each other. Each session consists of three independent market rounds, where each round consists of several trading periods (when a round ends is a treatment variable and discussed in Section 3.2). Each participant starts each round with the same initial endowment of three assets and 5500 points in their cash account. In each period, participants have the opportunity to trade assets by submitting marginal bids and asks simultaneously. Afterwards, the computer calculates the market price by constructing the demand and supply functions from the submitted bids and asks. The market price is determined by the intersection of these two functions. If the highest bid is lower than the lowest ask, there is no trade in the given period.[7] Furthermore, if there is excess demand or supply for the determined market price, it is randomly determined which bids or asks are executed of those bids or asks at the market price. If there is an interval of possible market prices (because of a vertical overlap of market demand and supply schedules), the realized market price is the midpoint of this interval. Participants are allowed to submit multiple bids and asks at the same time, but they face the following constraints while trading:

1. Short-selling is not allowed: participants cannot offer more assets to sell than they own, and similarly they cannot try to buy more assets than there are available on the market (18 minus their own asset holdings).
2. Borrowing money is not allowed: participants cannot submit bids that they cannot pay for with their current cash holdings.
3. No self-trade: the highest bid submitted by participants cannot be higher than their own lowest ask, that is, they are not allowed to trade with themselves.

If any of these constraints is violated, the software displays an error message, and participants have to reconsider their bids and asks.

After each trading period, thus after the market price is computed and trade is executed, dividend and interest earnings are paid out. Each asset pays a random dividend in each period: the dividend is either 0 or 10 points, both with equal probability, and the dividend is the same for all assets in a given period. The interest rate on cash is 4%, and applies to all cash participants hold at the end of the trading period. Both interest and dividend earnings are paid to a separate savings account. This savings account also pays interest, but participants cannot use money in this account to buy assets. This leads to a cash-to-asset ratio that is constant across all periods.

When a round ends, participants receive 125 points for each asset that they hold at this time. These 125 points correspond to the constant fundamental value of the asset (which can be calculated as the expected dividend divided by the interest rate). Participants are not explicitly told that 125 is the fundamental value of the asset; however, they do know that this is the buyout price, and they have full information on the interest and dividend payments, so that they can compute the fundamental value.

Pricing the asset at the fundamental value gives a cash-to-asset ratio of $5,500/(3*125) \approx 14.67$, which is relatively high compared to previous studies. We have chosen this high cash-to-asset ratio, because we believe that it is more representative of actual asset markets (with a lot of cash/wealth available in comparison to the value of any single asset). Furthermore, we are interested in how the different end times affect bubble formation and mispricing and therefore there needs to be space for bubbles to arise.

In each period participants have a limited amount of time to make their decisions. They have two minutes in the first ten periods of the first round and one minute in all other periods. If participants do not make a decision within this time, the computer automatically proceeds to the next period not submitting any bids or asks for that participant in the given period. Participants can also decide not to trade by submitting an empty schedule. Throughout the experiment participants can see the history of the current round: they receive graphical information about the realized market price and they can see their own trades and cash and asset balances in a table. For an example of a participant's decision screen, see Online Appendix A.

3.2. Treatments

The experimental design is a 2x2 factorial design. In one treatment dimension, we vary the length of a round, with a short horizon of about 15 periods and a long horizon of about 30 periods. In the second treatment dimension, we vary whether the rounds have a definite or indefinite ending. This leads to four treatments: *short-definite*, *short-indefinite*, *long-definite*, and *long-indefinite*.[8]

In all treatments, markets are as described above, the only difference arises in the number of periods per market round (and in whether these numbers are known beforehand). In the *definite* treatments all rounds have 15 (*short-definite*) or 30 (*long-definite*) periods per round, and the exact length is known to participants. In the *indefinite* treatments, we implement a minimum number of periods for each round (13 in *short-indefinite* and 28 in *long-indefinite*), and a continuation probability of 90% thereafter. We consider

[7] There is also no trade if there are no bids or no asks.

[8] Treatment *long-indefinite* is similar to the full information treatment of the call market in Kopányi-Peuker and Weber (2021a) but not identical. The differences are (except for the exact number of periods) that markets are terminated by discretion rather than relying on a continuation probability in Kopányi-Peuker and Weber (2021a) and that there is an upper bound of 1500 on bids and asks in that paper.

Table 3
Number of periods per round.

	Definite	Indefinite
Short	15, 15, 15	17, 16, 14
Long	30, 30, 30	32, 31, 29

this continuation probability high, as in each round participants play, in expectation, additional nine periods above the minimal number of periods.[9] Participants are aware of the minimum number of periods and the continuation probability.

The random numbers were drawn prior to the experiment and the same number of extra periods, determined with this continuation probability, was used in all sessions of the *indefinite* treatments.[10] This resulted in 4, 3, and 1 additional periods above the minimum in the three rounds, respectively (which is below the expected number of additional periods in each of the rounds; details on the procedure of the random draw can be found in Online Appendix B.1). There are thus 17, 16, and 14 periods in the three rounds of *short-indefinite* and 32, 31, and 29 periods in the three rounds of *long-indefinite*. Table 3 gives an overview of the experimental design, with the number of periods in each round.

One could bring forward the following reasoning why one would expect treatment differences. First, assume that bubbles arise in the markets (in line with the literature on experimental asset markets with high cash-to-asset ratios; e.g., Kopányi-Peuker and Weber, 2021a), sustained at least in parts by trend-following behavior. Second, assume that these bubbles deflate or burst due to imperfect backward induction when the end time comes near, for instance between three and ten periods before the (definite) end of the market (that is, at some point participants start to understand that the asset must soon be worth the buyout price; the point when they realize this may be different for different participants). If that is the case, one would expect higher average prices in the *long* treatments than in the comparable *short* treatments (because the number of periods with lower prices after the imperfect backward induction starts is higher in relation to the total number of periods in the *short* treatments). One would similarly expect higher average prices in the *indefinite* treatments than in the *definite* treatments, because backward induction is not straightforwardly possible with an indefinite end time. One could come up with similar arguments why learning across rounds should be more pronounced along these treatment dimensions.

However, it is certainly possible to come up with different arguments leading to these or opposing predictions, and we believe that testing the effects of the length and the definiteness of the ending is relevant beyond testing one particular theoretical mechanism in mind. Therefore, we remain largely agnostic about the exact psychological mechanisms at play and use two-sided hypotheses throughout the statistical analysis.

3.3. Procedures

The experiment was programmed in `PhP/MySql` and conducted at the CREED laboratory of the University of Amsterdam. The majority of participants were students of economics or business. The experiment was pre-registered at https://osf.io/2s9z6 (or rather "mid-registered", as the first sessions had already been run).[11]

There are 60 different groups of six participants each, 15 groups in each treatment. Thus, 360 participants participated in the experiment. *Short* sessions lasted on average around 110 minutes, with average earnings of about 23.50 euros. *Long* sessions lasted on average around 165 minutes, with average earnings of 34.50 euros.[12]

At the beginning of the experiment, participants read the instructions on paper at their own pace. Thereafter, they had to answer comprehension test questions on screen. They could not proceed to the first round of the experiment before all test questions were answered correctly. The experimental instructions and the comprehension test questions are reproduced in Online Appendix C. Note that the numbers used as examples in the instructions are more often above the fundamental value than below. However, the opposite is true in the comprehension test questions, so that no considerable anchoring bias should arise from the examples if instructions and comprehension test questions are considered jointly. Even if such a bias exists, there is no reason that they should be different in the different treatments. Therefore treatment effects are not expected to be distorted.

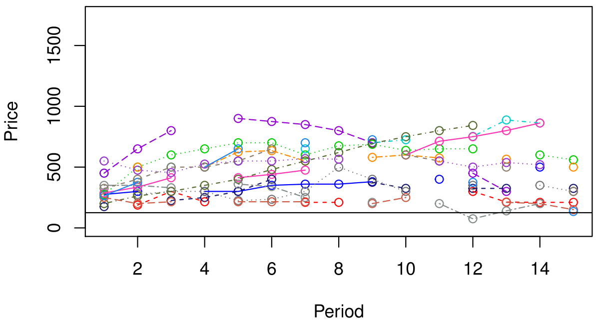
After the experiment, participants completed a short post-experimental questionnaire to gather demographic information (age, gender, field of study, nationality). Participants' earnings were determined based on their final cash holdings and asset holdings (bought back for 125 points per asset) at the end of a randomly chosen market round. They received 1 euro for each 800 points they earned. In addition, they received 10 euros for participation.

[9] As past draws of the continuation probability do not influence the continuation probability in the future, participants participate, in expectation, in further nine periods after any period that they are trading in during the random continuation phase. We believe that this continuation probability is sufficiently high to discover economically meaningful effects if they exist: it seems unlikely that increasing the continuation probability from 0% (definite end time) to 90% does not influence behavior, while increasing it even further would.

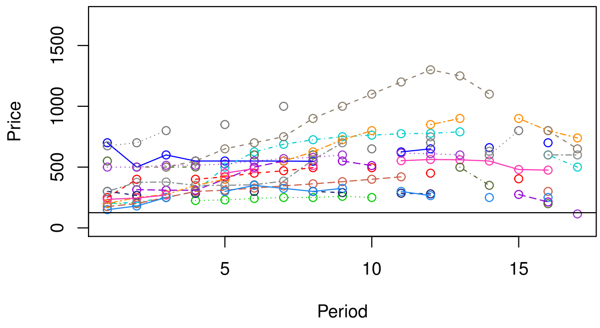
[10] Participants are not made aware of the fact that the random numbers were drawn prior to the experiment and obviously not of the outcome of the random draw (the latter would make the markets definite). From a participant's perspective, there is thus always a very high probability that the period they are trading in is not the final period of a round (100% until the minimum number of periods in a round is reached and 90% thereafter).

[11] Details are described in Online Appendix B.2. The analysis outlined in the pre-registration and carried out in this paper follows the analysis in the earlier working paper version with fewer observations (Kopányi-Peuker and Weber, 2021b).

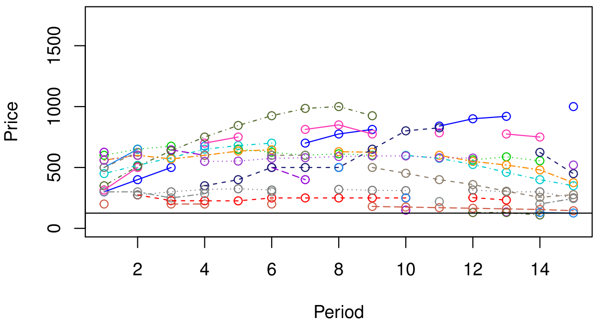
[12] We encountered minor problems during the data collection. These are documented in Online Appendix B.3.



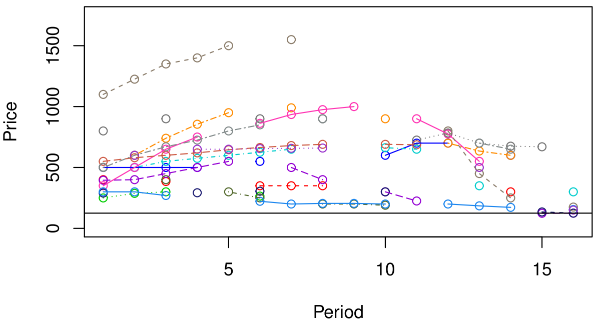
(a) *short-definite* Round 1



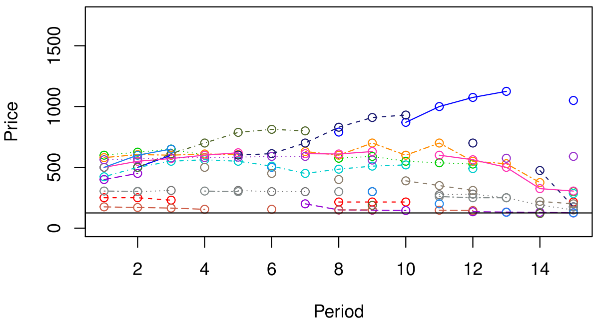
(b) *short-indefinite* Round 1



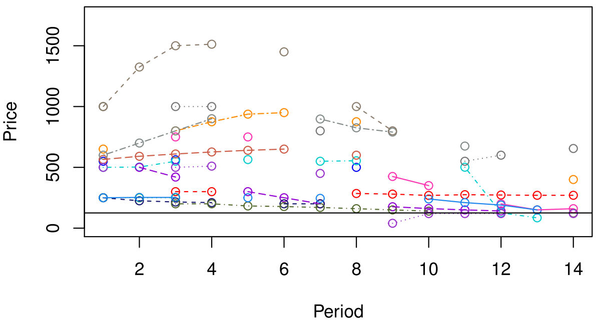
(c) *short-definite* Round 2



(d) *short-indefinite* Round 2



(e) *short-definite* Round 3



(f) *short-indefinite* Round 3

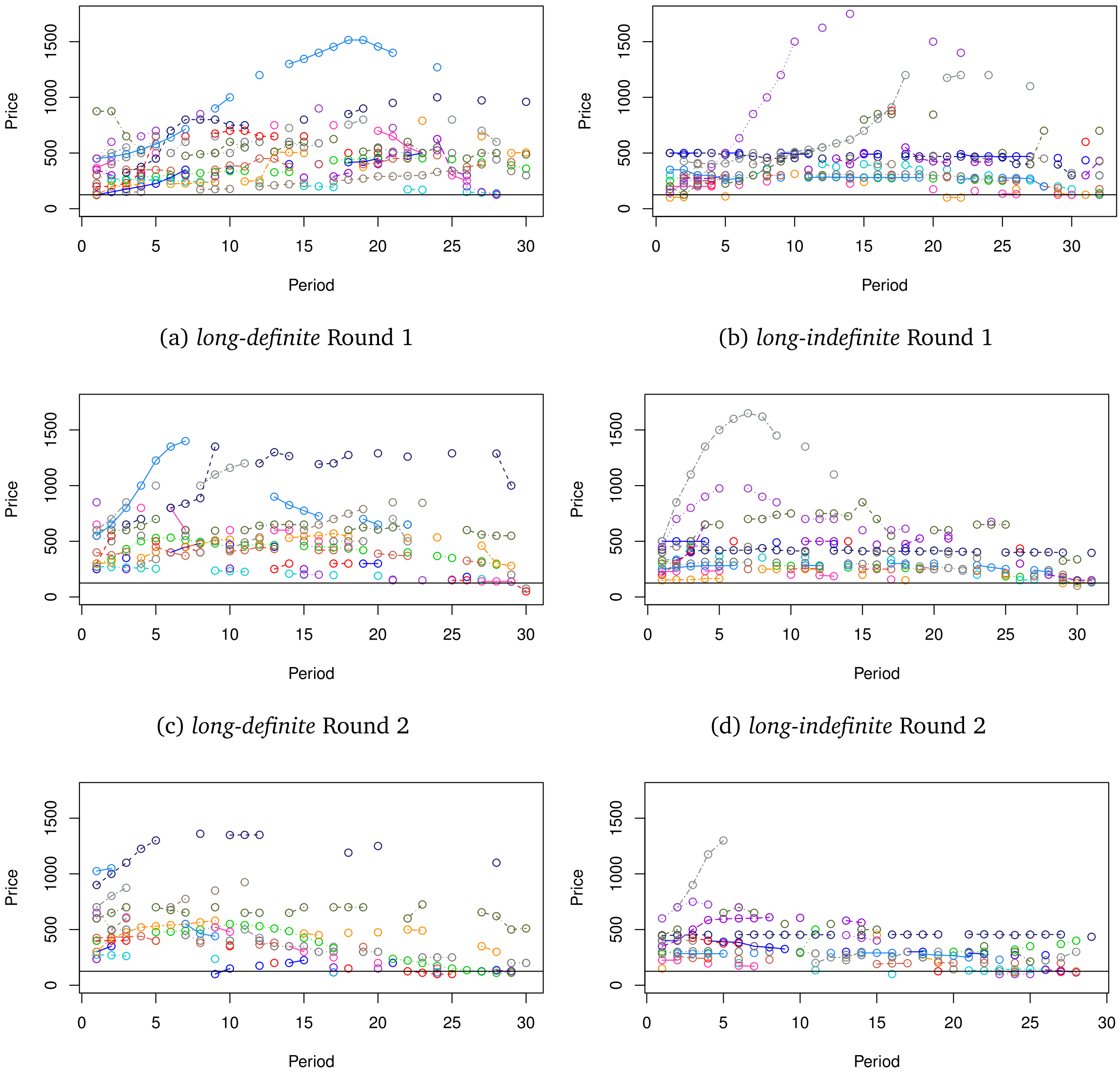
Fig. 1. Market prices in treatments *short-definite* and *short-indefinite*. This figure shows prices in the treatments *short-definite* and *short-indefinite* in all rounds. Each color and line type represents one group. The horizontal thin line shows the fundemantal value.

4. Results

Figs. 1 and 2 show market prices in all treatments, separately for each round. Each line represents one group (interruptions of the lines correspond to periods without trade). Fig. 3 shows the mean prices per group across all periods of a round. As the fundamental value in our markets is constant, and as almost all observed prices are above the fundamental value, the mean price is a good measure of both mispricing and overpricing (for other measures, see Stöckl et al., 2010).

Figs. 1 to 3 already reveal what will be confirmed in the statistical analyses below. Market prices are on average considerably above the fundamental value, and do not crash at the end of the rounds.[13] In all treatments, there are reoccurring bubbles in several

[13] A similar pattern, albeit with smaller magnitude, was also observed in the BASE14 treatment of Razen et al. (2017), with a constant C/A ratio of 3, constant fundamental value, and 14 periods, which is the most comparable treatment to our experiment.



(a) *long-definite* Round 1
(b) *long-indefinite* Round 1
(c) *long-definite* Round 2
(d) *long-indefinite* Round 2
(e) *long-definite* Round 3
(f) *long-indefinite* Round 3

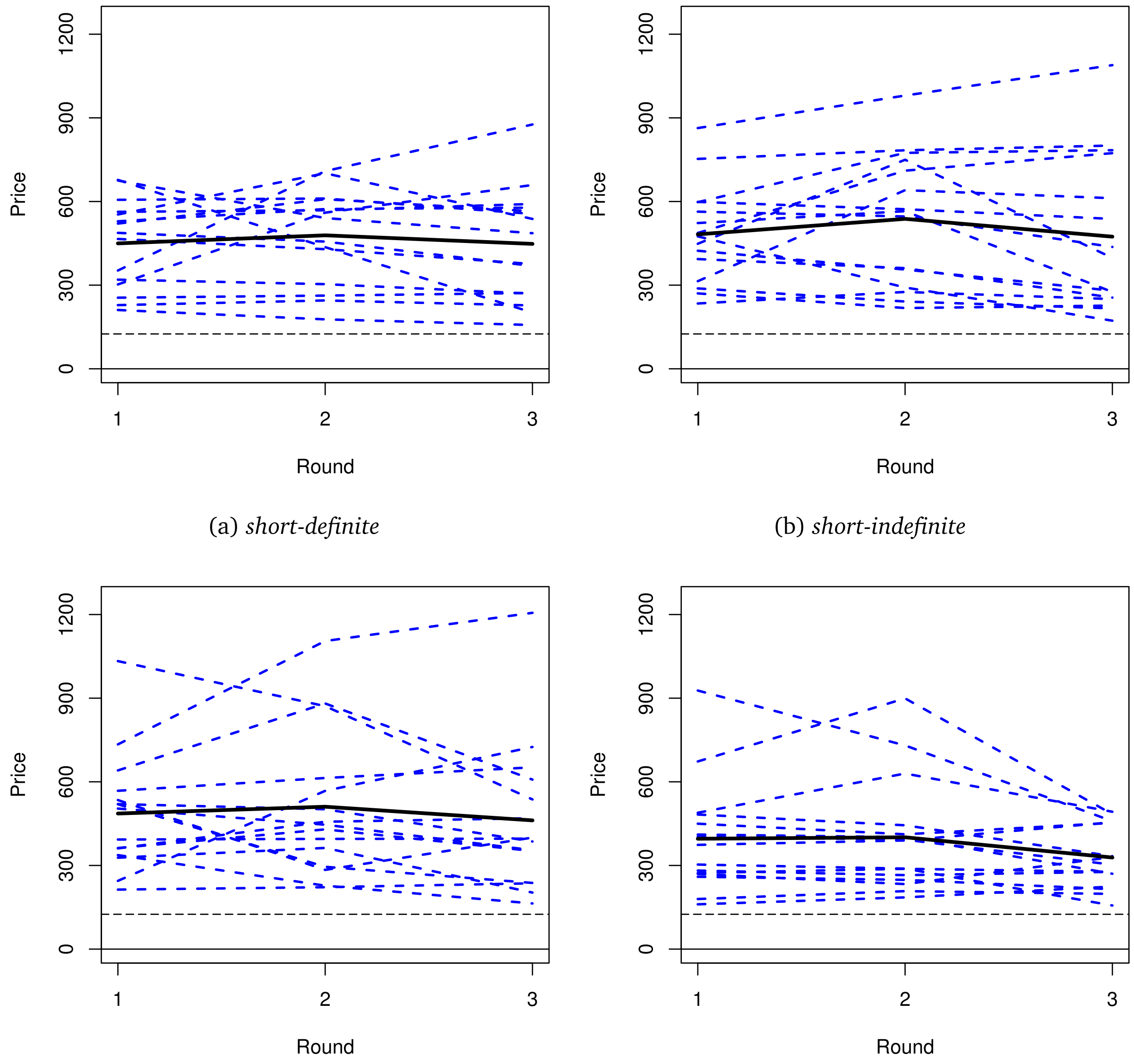
Fig. 2. Market prices in treatments *long-definite* and *long-indefinite*. This figure shows prices in the treatments *long-definite* and *long-indefinite* in all rounds. Each color and line type represents one group. The horizontal thin line shows the fundemantal value.

groups. We use the bubble definition from Kopányi-Peuker and Weber (2021a) that there is a bubble in a market if the mean price is at least twice the fundamental value.[14] With this definition, there are 13, 13, and 12 bubbles in rounds 1, 2, and 3, respectively, among the 15 groups in *short-definite*; among the 15 groups in *short-indefinite*, there are 14, 13, and 12 bubbles in the three rounds; in *long-definite*, there are 13, 13, and 11 bubbles (15 groups); in *long-indefinite*, there are 13, 11, and 11 bubbles (15 groups).[15] The number of bubbles does not substantially differ across treatments.[16]

[14] Note that this bubble definition is conservative in the sense that it relies on mean prices. The numbers would naturally be even higher if already the price in one period above twice the fundamental would be considered a bubble.

[15] Such large and recurring bubbles have similarly been found in markets with a relatively long and indefinite horizon in Kopányi-Peuker and Weber (2021a), who show that the large recurring bubbles are due to a high cash-to-asset ratio, which we also have in this paper.

[16] Razen et al. (2017) applied a different bubble measure, relying on differences from the average baseline market. Applying their method by considering the *short-definite* treatment per round as baseline, we find considerably less bubbles: 1 market in all three rounds in *long-indefinite*, 1 market in rounds 1 and 3 in



(a) *short-definite*

(b) *short-indefinite*

(c) *long-definite*

(d) *long-indefinite*

Fig. 3. Mean prices in all treatments and rounds. This figure shows the mean market prices across all periods of a round. Each blue dashed line corresponds to one group. The thick solid black lines represent the mean values of these lines per treatment. The horizontal thin black dashed line shows the fundamental value.

No systematic differences are visible between the four treatments. Patterns and even price levels look very similar independent of whether the horizon is *short*, *long*, *definite*, or *indefinite*. Pricing is not much more accurate in later rounds than in earlier rounds, and the level of learning that does exist is not different across the treatments. Similarly, while there is some mild speeding up of bubbles (bubbles forming earlier in a round in later rounds of the experiment), the treatment has no sizable impact on this speeding up of bubbles.

4.1. Overpricing

We provide linear regression results to see whether differences in overpricing observed in Figs. 1 to 3 are statistically significant and indeed systematic treatment differences. As stated in the pre-registration, we view the baseline regressions without interaction

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short-indefinite and 1 market in round 2 in *long-definite*. This definition compares maximum price, amplitude, and crash *across* treatments. If the deviations from the baseline treatment are large, this is considered a bubble. With this alternative definition the numbers of bubbles in our experiment change, but there are still no considerable differences across treatments.

Table 4
Treatment effects on mean prices.

	Round 1		Round 2		Round 3		Mean R1-R3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	480.5***	845.1*	520.9***	762.7	487.8***	570.0	496.4***	725.9
	(38.2)	(373.1)	(42.5)	(459.1)	(47.3)	(360.8)	(37.4)	(378.1)
Long	−24.9	−21.8	−52.3	−50.2	−65.8	−65.0	−47.7	−45.7
	(48.6)	(47.3)	(57.4)	(58.7)	(58.4)	(60.5)	(49.5)	(50.3)
Indefinite	−29.0	−51.9	−25.3	−41.2	−53.7	−60.3	−36.0	−51.1
	(48.6)	(50.9)	(57.4)	(62.8)	(58.4)	(67.2)	(49.5)	(54.8)
Av. age		−22.0		−14.5		−5.0		−13.9
		(16.9)		(20.0)		(14.1)		(16.2)
No. female		−8.9		−7.0		−3.2		−6.3
		(21.1)		(33.0)		(39.1)		(28.4)
No. econ		37.1		25.2		9.9		24.1
		(19.6)		(20.5)		(18.7)		(16.9)
R^2	0.01	0.13	0.02	0.06	0.04	0.04	0.02	0.07

This table reports regression results with mean market prices as dependent variable. Results are reported for the three rounds separately (round 1 in columns 1 and 2, round 2 in columns 3 and 4, round 3 in columns 5 and 6) and for the means across rounds (columns 7 and 8). Each observation corresponds to one group. Each regression is thus conducted with 60 observations. The variables *Long* and *Indefinite* are dummy variables that equal one if the treatment has a long or indefinite horizon, respectively. *Av. age* denotes the average age in a group. *No. female* and *No. econ* denote the number of females and students of economics/business, respectively. Heteroskedasticity-robust standard errors are provided in parentheses. ***, **, and * denote statistical significance at the 0.1%, 1%, and 5% level, respectively.

Table 5
Treatment effects on mean prices (with interaction term)

	Round 1		Round 2		Round 3		Mean R1-R3	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	449.8***	774.0	478.7***	645.6	448.0***	448.6	458.8***	622.8
	(41.2)	(389.0)	(43.2)	(476.2)	(52.0)	(366.1)	(38.7)	(391.5)
Long	36.5	23.5	32.0	24.5	13.7	12.4	27.4	20.1
	(68.0)	(62.4)	(79.7)	(77.5)	(85.9)	(85.9)	(68.4)	(66.1)
Indefinite	32.4	−6.1	59.0	34.3	25.9	18.0	39.1	15.4
	(61.5)	(64.6)	(74.8)	(76.5)	(89.0)	(90.4)	(68.2)	(69.4)
Long*Indefinite	−122.8	−91.0	−168.6	−149.9	−159.1	−155.3	−150.2	−132.1
	(96.7)	(93.4)	(113.6)	(111.7)	(116.0)	(113.3)	(97.9)	(95.0)
Av. age		−19.9		−11.1		−1.4		−10.8
		(17.5)		(20.8)		(14.6)		(16.9)
No. female		−7.1		−4.0		−0.1		−3.7
		(20.8)		(32.0)		(37.8)		(27.3)
No. econ		36.4		24.1		8.7		23.1
		(19.2)		(20.9)		(19.4)		(16.9)
R^2	0.04	0.14	0.05	0.08	0.07	0.07	0.06	0.10

This table reports regression results with mean market prices as dependent variable (similar to Table 4 but including an interaction term). Results are reported for the three rounds separately (round 1 in columns 1 and 2, round 2 in columns 3 and 4, round 3 in columns 5 and 6) and for the means across rounds (columns 7 and 8). Each observation corresponds to one group. Each regression is thus conducted with 60 observations. The variables *Long* and *Indefinite* are dummy variables that equal one if the treatment has a long or indefinite horizon, respectively. *Av. age* denotes the average age in a group. *No. female* and *No. econ* denote the number of females and students of economics/business, respectively. Heteroskedasticity-robust standard errors are provided in parentheses. ***, **, and * denote statistical significance at the 0.1%, 1%, and 5% level, respectively.

term or demographic variables as the main regressions, with the average price across rounds as the main outcome variable. The other regressions (which all yield very similar results) are considered robustness checks, and the regressions for the three rounds separately (with similar results as for the average across rounds) are considered of secondary importance. The dependent variable is the mean price per group, there is thus a total of 60 observations for all of the reported regressions. The groups do not interact with other groups in any way, so that the observations can be treated as statistically independent.

The results are shown in Tables 4 and 5. Table 4 provides coefficients of dummy variables for the long treatments (*Long*) and the indefinite treatments (*Indefinite*), without an interaction term. These are thus the baseline regressions. The coefficients are reported separately for each round and for the average across all three rounds. Half of the regressions do not include demographic covariates, the other half includes the demographic covariates age (average age in a group), gender (measured by the number of females in a group) and field of study (measured by the number of economics or business students in a group). All regressions include an intercept. Table 5 reports coefficients of the same regressions but including an interaction term of treatment variations *Long* and *Indefinite*.

Tables 4 and 5 show that whether the horizon is definite or indefinite does not affect the pricing of the assets. In the baseline regressions without an interaction term (Table 4), which allow us to decompose the effect simply into the two effects of *indefinite* and

Table 6
Treatment effects on mean prices R1 minus R3 (learning)

	(1)	(2)	(3)	(4)
Intercept	−7.3	1.8	275.1	325.4
	(48.3)	(57.7)	(262.3)	(261.3)
Long	40.9	22.8	43.1	11.1
	(52.5)	(87.1)	(52.9)	(85.2)
Indefinite	24.6	6.5	8.3	−24.1
	(52.5)	(74.9)	(58.8)	(80.0)
Long*Indefinite		36.2		64.3
		(105.8)		(104.6)
Av. age			−17.0	−18.5
			(11.3)	(11.2)
No. female			−5.8	−7.0
			(34.3)	(34.3)
No. econ			27.3	27.8
			(22.3)	(23.1)
R^2	0.01	0.02	0.07	0.08

This table reports regression results with the dependent variable mean prices in the first round minus mean prices in the third round. Each observation corresponds to one group. Each regression is thus conducted with 60 observations. The variables *Long* and *Indefinite* are dummy variables that equal one if the treatment has a long or indefinite horizon, respectively. *Av. age* denotes the average age in a group. *No. female* and *No. econ* denote the number of females and students of economics/business, respectively. Heteroskedasticity-robust standard errors are provided in parentheses. ***, **, and * denote statistical significance at the 0.1%, 1%, and 5% level, respectively.

long horizons, an *indefinite* horizon leads to slightly lower prices, without these differences being significantly different from zero.[17] The fact that backward induction is more difficult with indefinite horizons does thus not seem to be important for the pricing of the assets. One may also argue that backward induction is more difficult in the treatments with more periods. However, the difficulty of backward induction similarly does not lead to higher prices in the long treatments. Again, the baseline regressions, a *long* horizon has a slightly negative effect on prices, without these differences being significantly different from zero.

In the robustness checks with an interaction term (Table 5), all coefficients are similarly insignificant. The direct effects of *long* or *indefinite* are very small and positive in these regressions. The interaction of these two treatment dimensions has a somewhat larger negative insignificant coefficient, which is partially offset by the direct positive effects, yielding a small negative net effect.

Given that average prices are around 400 to 500, while the fundamental value of the asset is at only 125, the sizes of the treatment differences can be considered small (assuming that the estimates of our null result represent the true effect sizes). The results are robust to using maximal prices in a round as measure for pricing accuracy instead of mean prices. This can be seen in Tables D.1 and D.2 in the online appendix.

4.2. Learning and speeding up of bubbles

Not only it is interesting to examine potential differences about the extent of overpricing, but it is also interesting to examine the dynamics of prices. Does it depend on the particular trading horizon whether participants learn to price the assets well with experience? Does it depend on the treatment whether bubbles appear earlier in later rounds of the experiment? Figs. 1 to 3 suggest that prices do not become considerably more accurate across the rounds, with at best slight decreases in overpricing during the course of the experiment in all treatments. Similarly, the shapes of the bubbles look similar in all treatments and do not change much across the rounds. In particular there seem to be no treatment differences with respect to the speeding up of bubbles (that is, bubbles appearing in earlier periods of later rounds). Regression results confirm these impressions, as shown in Tables 6 and 7.

Table 6 provides coefficients for the treatment variables with the dependent variable 'mean prices R1 minus mean prices R3'. If participants learn to price the assets better over the rounds, mean prices should decrease across rounds (given that they are considerably above the fundamental value in the first round), yielding a positive dependent variable. Considering the mere means for illustration, there is only very limited learning. On average, pricing improves by 2 points in *short-definite*, by 8 points in *short-indefinite*, by 25 points in *long-definite*, and by 67 points in *long-indefinite*. The learning is not statistically significant (this can be seen when looking at the intercepts of the regressions, as reported in Table 6; note that whether learning can be observed or not is not a treatment difference and thus of secondary importance for us).[18] The regressions in Table 6 reveal that there are no significant treatment differences concerning the learning of asset prices, in the main regressions without interaction term or demographic control variables and in the robustness checks including such variables.

[17] This finding relates to a finding in a different setting: comparing hard-close auctions (with definite ending) and sudden-termination auctions (with indefinite ending), Füllbrunn and Sadrieh (2012) do not find efficiency differences between the two auction mechanisms.

[18] Several studies in the literature report considerable learning across rounds in different settings (e.g., Dufwenberg et al., 2005; Haruvy et al., 2007; Füllbrunn et al., 2020). However, these studies have in common that they have a relatively low cash-to-asset ratio. Very slow learning in call markets, as in this paper, has been observed in markets with a high cash-to-asset ratio in Kopányi-Peuker and Weber (2021a).

Table 7
Difference between mean prices computed in the first half of the periods as a fraction of the mean price computed on all periods, R3 minus R1 (speeding up of bubbles).

	(1)	(2)	(3)	(4)
Intercept	0.20***	0.15**	0.89	0.74
	(0.05)	(0.05)	(0.45)	(0.49)
Long	0.02	0.12	0.02	0.11
	(0.06)	(0.07)	(0.06)	(0.08)
Indefinite	0.00	0.10	0.03	0.12
	(0.06)	(0.10)	(0.06)	(0.10)
Long*Indefinite		−0.20		−0.18
		(0.13)		(0.13)
Av. age			−0.03	−0.02
			(0.02)	(0.02)
No. female			0.00	0.00
			(0.03)	(0.03)
No. econ			−0.02	−0.02
			(0.02)	(0.02)
R^2	0.00	0.04	0.05	0.08

This table reports regression results with the dependent variable 'difference between first and third round of the mean price in the first half of a round divided by the mean price in the entire round'. Each observation corresponds to one group. Each regression is thus conducted with 60 observations. The variables *Long* and *Indefinite* are dummy variables that equal one if the treatment has a long or indefinite horizon, respectively. *Av. age* denotes the average age in a group. *No. female* and *No. econ* denote the number of females and students of economics/business, respectively. Heteroskedasticity-robust standard errors are provided in parentheses. ***, **, and * denote statistical significance at the 0.1%, 1%, and 5% level, respectively.

To see whether bubbles speed up over time (that is, whether bubbles appear earlier in the last round of the experiment than in the first), we consider the following measure, as mentioned in the pre-registration. We calculate the fraction of the mean price in the first half of a round (if the number of periods is odd, we round up) and divide it by the mean price in the whole round (such a measure is also used in Kopányi-Peuker and Weber, 2021a). If bubbles appear earlier in later rounds, this measure should be higher in later rounds than in earlier rounds. This is indeed what we observe in all treatments.[19] This measure increases from 0.97 to 1.12 (*short-definite*), from 0.95 to 1.20 (*short-indefinite*), from 0.93 to 1.20 (*long-definite*), and from 0.98 to 1.16 (*long-indefinite*). This speeding up of bubbles is reflected in an intercept that is positive and significant in the baseline regression and positive and partially significant in the robustness checks (this can be seen in Table 7; whether there is a speeding up of bubbles or not is of secondary importance for this paper, as it is not a treatment difference). Table 7 shows that there are no significant treatment differences considering the speeding up of the bubbles.

5. Concluding remarks

Despite the fact that there are already hundreds of scientific articles on experimental asset markets, there is thus far hardly any literature analyzing the effect of the end time being definite. Similarly, there is hardly any literature analyzing the effect of the number of periods in a controlled manner. This is surprising, as the definite and short horizons usually employed in the laboratory may be considered a striking difference to actual financial markets. The current paper addresses this methodological gap in the literature.

We find that price dynamics are very similar across the different treatments, independent of whether the horizon is *short*, *long*, *definite*, or *indefinite*. With a relatively large number of observations, we do not find any statistically significant differences between treatments with respect to pricing accuracy, learning, or the speeding up of price bubbles. Bubbles recur in all treatments with only very small improvements in pricing accuracy across the repetitions of a market. Bubbles appear slightly earlier in later repetitions of the markets in all treatments, as generally observed in the literature. Overall, our findings are in line with the results of the few papers with closely related aims (Kose, 2015; Duffy et al., 2024; Razen et al., 2017).

These null results can be interpreted as good news for the large existing literature on experimental asset markets. Given that we observe such similar dynamics in the different treatments, it seems most likely that the findings from existing asset market experiments, in particular treatment differences, are not driven by the particular choice of the (usually short and definite) end time.

There are still several unexplored research topics related to the end time of experimental asset markets. Variations of cash-to-asset ratio, discount parameter, or production technologies are promising for future work. Similarly, investigations of experimental asset markets in which participants have different trading horizons could be fruitful.[20]

[19] Such a speeding up of bubbles has already been observed in a variety of different market settings (e.g., King et al., 1993; Dufwenberg et al., 2005; Haruvy et al., 2007; Kopányi-Peuker and Weber, 2021a).

[20] We thank two anonymous reviewers for suggesting these avenues for future research.

CRediT authorship contribution statement

Anita Kopányi-Peuker: Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing. **Matthias Weber:** Conceptualization, Data curation, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Resources, Software, Supervision, Validation, Visualization, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jcorpfin.2024.102647.

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