Market Bubbles and Crashes as an Expression of Tension between Social and Individual Rationality: Experiments*

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Preliminary Version: June 7, 2012

*The authors are grateful for comments from Minghei Cheng, Luis Rayo, Wendy Shin, and participants during seminars at the Australian National University, Melbourne University, Sydney University, University of Miami, University of New South Wales, University of Technology Sydney, University of Utah, and during presentations at the Experimental Finance 2010 conference in Gothenburg, Sweden, the 2011 Finance Down Under Conference in Melbourne, Australia, the Xiamen Conference on Experimental Economics and Finance 2010, and the 2011 Society for the Advancement of Economic Theory meetings in Faro, Portugal. This study was made possible because of financial support from the Swiss Finance Institute at the Ecole Polytechnique Fédérale Lausanne and the U.S. National Science Foundation (grants SES-0616431 and SES-1061824).

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Abstract

We investigate the claim that social rationality explains the emergence of one type of bubble in competitive asset markets that we shall refer to as "credit market bubble," and that individual rationality explains the subsequent crash. The bubble is defined as a situation where (i) the debt is priced above its intrinsic value and (ii) the debt is rolled over even though each creditor should cash in as it is commonly known that the debtor would never be able to repay the debt at face value. Building on evidence from behavioral game theory, we conjecture that credit market bubbles emerge whenever the debtor's payment ability, although never sufficient, grows over time. As such, bubbles are beneficial, even if they eventually lead to crashes which cause re-distribution of wealth away from those who ride the bubble too long. We argue that this captures the essence of many financial bubbles alleged to have been observed in the real world. Experimental data confirm the emergence of bubbles in this setting. The bubbles are robust – they re-emerge upon replication – and decay can be avoided by adding noise, e.g., through random introduction of (informed) newcomers. The presence of financial markets increases the overall beneficial effects of the bubbles. Prices always remain above levels predicted by conventional asset pricing theory. Nevertheless, they exhibit properties of (informational) efficiency. Among others, prices cannot be used to predict the length of the bubble, but correlate with the payoffs that the claims eventually generate.

I. Introduction

Periods of exuberantly increasing asset prices followed by sharp price declines (crashes) are said to have been part of competitive financial markets ever since their inception in late 15th century Antwerp (Schumpeter [1939]). Most accounts of alleged asset price bubbles focus on the detrimental effects of the eventual crash. For example, the housing and credit market bubble that burst in 2007-8 has been claimed to have caused an almost 5% drop in U.S. GDP between the second Quarter of 2008 and 2009, and about a 25% drop in wealth, much of it invested for insurance and retirement purposes. Still, there is no doubt that the economy benefited immensely from the funding available because of the build-up of the bubble. Indeed, it is possible to argue that everyone would have been worse off if the financial securities said to be the cause of the crash had never been allowed in the first place.

But such an argument is difficult to defend when dealing with bubbles that emerge in the field, because we do not have the data to prove the case. That is why experimental economists have long attempted to create bubbles in a controlled environment. One setting where asset prices (in the market for a single asset with stochastic dividend payments and a principal of zero) are often too high and eventually drop back to a rational level was pioneered in Smith et al. [1988]. Since Smith et al., there has been a large series of experimental studies addressing the effects of different design parameters on the magnitude of the pricing bubbles.

It is difficult to make sense of the prices in bubble experiments – it imply that subjects collectively make very large mistakes. It is therefore perhaps not surprising that the bubbles are not robust: the mis-pricing quickly disappears with experience; even if only one-third of the subjects have had prior experience, mispricing fails to re-emerge (Dufwenberg et al. [2005]; re-kindling the bubble requires specific changes in the parameters, see Hussam et al. [2008]).[1,2]

Several researchers have noted that the original Smith et al. asset payoff structure lacks a "real world" flavor; e.g., Noussair and Powell [2010]. The sinking fund feature is rarely seen in naturally occurring financial markets and possibly clashes with participants' idea of what a stock payoff should look like. When the expected payoff of the asset is changed to be constant across trading rounds, the bubbles all but disappear [Noussair et al., 2001, Kirchler et al., 2010].

The goal of this paper is to create a realistic setup that generates bubbles in the lab in a way that is (i) unambiguous and (ii) robust. The former requires a design with a *unique* corresponding asset pricing equilibrium. The latter goal implies a design that allows for the bubble to be replicated, possibly within the same cohort and with the same parameters.

Our design exploits what we think is the cause of (credit) bubbles in the real world, namely, all creditors know that there is never going to be enough cash to repay the full debt but they would get more if they collectively refinanced the debt and waited for final payment until later. When put like this, the underlying game between creditors has the flavor of a multi-person centipede game. Theory predicts that it is individually rational for each creditor to refuse to ever roll over the debt, and the resulting equilibrium price of the debt instruments would equal the immediate debt repayment value. A bubble

[1] The bubble is robust in other dimensions, though: it survives manipulations of the first time it is tried with a new cohort, such as the addition of futures markets as in Noussair and Tucker [2006], the disallowance of speculative trades as in Lei et al. [2001], etc.

[2] Theoretical arguments can be found, however, to explain prices. Specifically, it requires risk neutrality to claim that a steady decline is the only viable equilibrium price path. With risk aversion, many price paths are consistent with equilibrium. This is because markets in the bubble experiments are usually incomplete. For example, if dividends can take on one of two values, and dividends are paid N times, then there are 2^N possible final outcomes, and depending on individual preferences and beliefs, agents may need to trade continuously to generate their preferred final wealth; the intermediate trading could create price paths like the ones observed in experiments. Price paths are only bounded by the maximum and minimum future cumulative dividends [Bossaerts, 2009].

emerges if prices are higher. This could happen because creditors refinance the debt and hence, collect more on average. But re-financing is not in one's self interest.

Experimentation with the centipede game has generated robust deviations from the Nash equilibrium predictions; see McKelvey and Palfrey [1992] for a 2-person, and Murphy et al. [2004], Schotter and Yorulmazer [2009] for a multi-person version of the game.[3] Importantly, the resulting allocations Pareto-improve over the equilibrium allocation. As such, players are evidently willing to forgo best-responding with the risk of being defected upon, as long as there is a good chance that everyone will be better off.

While one cannot exclude that choices partly reflect (random) mistakes, economists have generally interpreted the deviations from individually rational behavior as evidence of other-regarding preferences and social norms, and have incorporated those in their models under the genre of "behavioral game theory." Explicit estimation of the relative contributions of social concerns vs. decision errors exist, not for the centipede game, but for another game in the same category, namely, the public goods provision game [Goeree et al., 2002, Anderson et al., 1998]. Here, we demonstrate that Pareto improvements do provide significant explanatory power for why debt is rolled over even if the debtor will never be able to fully repay.

We refrain from explaining the deviations in terms of preferences or norms (which would imply that individuals always commit to the same level of altruism or inequity aversion) and instead refer to the willingness of an agent to forgo individual rationality because of potential Pareto improvements as *social rationality.* Social rationality is about exploiting Pareto improvements only when there is a chance of reciprocity. Social rationality will not emerge when reciprocity is unlikely to emerge.

[3]Schotter and Yorulmazer [2009], implements the game in a bank run setup; they find that "while the theory predicts no late withdrawals, we see that 50%, 67% and 58% of the subjects withdraw in late periods in the Simultaneous, Low and High-Information Sequential treatments, respectively." The late withdrawals are robust to replication of the game. This behavior is consistent with that observed in the centipede game; see Palfrey and McKelvey (1992).

Our focus will be on the potential effect that the introduction of financial markets would have on debt roll over in the presence of a bankrupt debtor. We conjecture that financial markets would enhance social rationality by providing opportunities for agents with less trust in social rationality to sell their claims to agents who believe in social rationality. This necessarily means that prices should robustly deviate from standard equilibrium predictions. That is, a bubble must emerge. As time evolves, the tension between social and individual rationality grows and eventually enough creditors cash in, causing prices to crash.

Our conjecture is not trivial. One can easily counter-argue that financial markets bring outcomes closer in line with individual rationality. First, there is ample evidence that financial markets improve valuation, and hence, choices. This has been observed in situations where (i) information would otherwise fail to aggregate [Forsythe et al., 1984], (ii) cognitive limitations would affect choices [Laibson and Yariv, 2007, Asparouhova et al., 2010, Oliven and Rietz, 2004], or (iii) traders would be confused [Porter and Smith, 1995]. Second, in the context of debt of a bankrupt debtor, one can envisage that prices reveal how long other creditors are willing to wait until cashing in their claims, so that best-response becomes easier to determine, leading to a rapid unraveling of the credit bubble.

There is a relationship between our credit market bubbles and bank runs [Diamond and Dybvig, 1983, Schotter and Yorulmazer, 2009]. In bank runs, however, the only way to redeem (cash in) a claim is to clear it with the issuing institution. In our setting, agents also have the option to sell their claim in a secondary market. In addition, bank runs are an equilibrium phenomenon, while our credit market bubbles are not.

More importantly, bank runs emerge when the claim (deposit) holders unexpectedly need money, before the bank's investment successfully pays off. In contrast, in our credit market setting, neither the opportunity cost for claim holders (from investing elsewhere) nor the solvency of the (bankrupt) debtor change over time.

It is also important to note that our credit markets are neither a prediction market for, nor a derivative market to, a game [Berg et al., 2008, Wolfers and Zitzewitz, 2004]. Instead, our markets are integrated into a game – the players in the market and those concurrently playing in the game are the same. As a result, prices form endogenously. Our approach is closer to Crawford and Broseta [1998], Kogan et al. [2010], where the focus is on the effect of auctions on equilibrium selection; we instead study the effect of markets on non-equilibrium behavior, and the feedback from non-equilibrium behavior in the underlying credit game on asset pricing. Endogeneity of prices also distinguishes our setting from that of clock games (Abreu and Brunnermeier [2003], Moinas and Pouget [2010]), where the emphasis is on individual incentives to ride the bubble when asset prices are excessively high.

Our experiments are a microcosm of the Eurozone sovereign debt crisis. The willingness of private creditors to voluntarily reduce the principal on their Greek bondholders is, we think, a expression of social rationality. They are willing to do so even when others (e.g., the European Central Bank) are unwilling to join the debt reduction, as long as free-riding is sufficiently limited so that it does not eliminate the potential for higher future payments to exceed the payoffs from immediate default.

In our experiments, we unambiguously observed price bubbles. Significantly, relative to (control) experiments without markets, social welfare (average effective payments per claim) increased. As such, financial markets appeared to enhance social rationality rather than eliminate it. Offsetting the enhanced social welfare was an increase in payoff volatility: since it was hard to predict when the bubble bursted, some subjects lost money (relative to the payoff of immediately cashing in) because they waited too long to sell or cash in.

Prices were always above intrinsic values (current redemption values of all the claims), a hallmark of price bubbles. Prices were mostly even above the effective value, defined as the actual amount that we ended up paying per unit of remaining claims (this amounts reflects actual roll-over decisions). But prices did exhibit features that one would associate with an (informationally) efficient market. While the initial price level predicted the minimum duration of the pool, it could not predict its eventual duration. Initial prices were also correlated with effective value. This means that higher initial prices were an indicator of higher potential payoffs from future cash-ins. As such, asset prices reflected future reward opportunities.

While bubbles re-emerged, we observed decay when replicating with the same parameters and subjects. To determine whether it was easy to avoid decay, we organized a sequence of eleven online replications, whereby subjects self-selected into the replications from a common pool (an introductory class in finance at Caltech), and where the entire pool had access to prices and trades as well as credit bubble durations from past replications. Now subjects were not always the same anymore across replications, but all were informed about the past. The bubbles replicated robustly in this more noisy environment, without any sign of decay. Consequently, noise thus appears to be important for social rationality to survive in the presence of (individually rational) attempts to profit from it. Ours therefore is another example of how noise promotes the well functioning of financial markets (Black [1986]).

The remainder of the paper is organized as follows. In the next section we introduce the general methodology. Section III formalizes the setup. Specifics of the experiments are discussed in Section IV. Results are reported in Section V. In Section VI, we elaborate on decay in the duration of the credit bubble. Section VII offers further discussion and Section VIII concludes.

II. Methodology: General

All experiments were variations of the following setting. Subjects were allocated a fixed and known number of securities. The securities (called "tickets") were *claims* *to a growing pool of money.* Subjects could trade the securities in an electronic, continuous limit order market for several rounds in a row (we used Flex-E-Markets; see http://www.flexemarkets.com). The number of possible trading rounds was limited to nine and subjects knew this beforehand. At the end of a trading round, subjects could decide whether *to roll over* their holdings to a subsequent trading round, or *cash-in* part or all of their claims. Securities that were not rolled over were paid out of the pool of money. Significantly, cash-in requests were private, so that subjects never knew how many tickets had been submitted for cash-in.

In each round t, securities carried a known *face value* $F(t)$ that increased over time. Once a trading round concluded, if the number of securities submitted for cash-in was lower or equal than the amount of money in the pool divided by the face value, all submitted securities paid to their holders the face value. Otherwise, the pool was "liquidated" and all outstanding securities (not only the ones submitted for cash-in) were paid pro-rata.[4] Face values were set so that there was never enough money in the pool to pay for all outstanding securities. We shall refer to the pro-rata value of outstanding securities given the amount of money in the pool in round t as the *intrinsic value* $I(t)$. If all remaining securities were in the hands of a single subject, cash-in was forced (roll-over was no longer permitted).

The parameters (number of tickets, the initial size and growth of the pool, and the evolution of the face values) were jointly determined so that it was always in one's interest to cash-in (to not roll over one's claims) the round before all remaining subjects were expected to cash-in. This required, among others, $F(t) > I(t+1)$, for all t. As a result, all Nash equilibria outcomes have the pool being liquidated after the first round. Nevertheless, the parameters were also set to ensure that everyone is always better off if all decided to roll over their claims to the subsequent round. As a result, the Nash

[4]In the early treatments only the submitted securities would be paid pro rata. The theoretical implications of both setups are identical. However, the setup where all outstanding securities are paid is the one more commonly observed in practice.

equilibria are not Pareto optimal; Pareto improving payouts can be obtained only if one refrains from best-responding to other agents' strategies, i.e., to not act in self-interest.

Table I illustrates this with an example. (We ran the experiments with parameter values like the ones used to create this table.) Table I shows that there were substantial incentives to cash in before the last players. For example, if two players roll over the securities until $t = 4$ (third row), cashing in at $t = 3$ generates \$1.80 while rolling over till $t = 4$ leads to a payoff of only \$0.26, even if four other players plan to cash-in at $t = 3$ as well. So, if one observes roll-over of securities with this parameter choice, one cannot readily attribute this to lack of incentives. When the group of claim holders that cash in last is large, incentives are lower. For example, when all ten other players roll over until $t = 3$ (second row of Table I), then cashing in at $t = 2$ leads to a payoff of 1.50, while rolling over to $t = 4$ generates only a marginally lower 1.32. As such, defection is less profitable, and recurrence of the bubble is more likely.

The availability of a competitive market in which claims can be traded does not change the conclusion that one should exercise early. At first, one might conjecture that the addition of the market would lower incentives to cash in early. For example, if the price P is higher than the current face value, an agent who would have planned to cash in immediately absent markets should sell her claims rather than cashing in. As a result, cash-ins are postponed. However, such prices are not rational in the sense of Radner [1972]: they do not correctly anticipate the value of the traded security in the next round.[5]

[5]Take the first cash-in example. Six players cash-in in round 3 and four cash-in in round 4. Given this action profile of the other players, the 11'th player's best-response is to cash-in in round 3. Now, in the presence of markets, this player would find it beneficial to sell her claim in round 3 instead of cashing in only if the price is better than the cash-in value (which equals the face value $F(3) = 1.80$). The only players willing to buy from him at a price different from $F(3)$ are the four players planning to cash in after round 4. But if these players have rational expectations, they are not willing to pay more than the intrinsic value in round 4 of the claim, $I(4)$, which equals 0.82. Since $I(4) < F(3)$, cashing in rather than selling remains the best response.

Generally, if the last cash-in takes place in round $t+1$, then the rational price $P(t)$ that players who plan to cash in at $t+1$ are willing to offer must be the round- $t+1$ intrinsic value $I(t+1)$. By construction, however, $F(t) > I(t+1)$, so $F(t) > P(t)$ and the best response to other players' plans continues to be cashing in rather than selling. Therefore, incentives are unaltered by the addition of the market as long as at least two players hold securities.

This result would not necessarily obtain, however, if one player could buy all the securities and, to maximize payoff, roll them over until the last round, to collect the pro-rata value based on a maximally grown pool of money. We eliminated this possibility by forcing cash-in when only a single holder remained. That is, roll-over was allowed only when there were at least two securities holders. From the moment there are at least two players, competition forces early cash-in.

III. Formal Theoretical Setup

We study a T-period economy with N risk-neutral creditors. In period 1 each creditor has an endowment of D securities that are claims to a pool of money (the pool can be thought of as the wealth of a fictitious lender) with a face value of $F(1)$ each and C units of cash. The face value of the asset grows deterministically at rate r each period, i.e., in period t the face value is $F(t) = F(1)(1+r)^{t-1}$. Agents choose when to cash in their claims against the money pool, which in period 1 holds $P(1)$ dollars, where $P(1) < F(1) \times N \times D$. After each period, the amount left in the money pool grows at a rate of $r^* < r$. The decision "to not cash in" claims is equivalent to "rolling over" the debt issued against the pool.

Let s_t denote the number of claims submitted for cash-in in period t ($t = 1, \ldots T$). Let $S(t) = (s_1, s_2, s_3, \ldots s_t)$ denote history of cash-ins till t; to reduce notation, we set $S = S(T)$. Let $P(t)$ denote the cash remaining in the pool in period t before claim submissions have been decided on. For $t > 1$, $P(t) = \max\{0, (P(t-1) - s_{t-1}F(t-1))(1+r^*)\}$. Let $\pi_t(S)$ denote the payoff to each submitted claim in period t. Thus, for $t > 1$

$$\pi_t(S) = \begin{cases} F(t) & \text{if } s_t F(t) < P(t) \\ \frac{P(t)}{\sum_{\tau \geq t} s_\tau} & \text{otherwise} \end{cases}$$

The above represents an N-person game where each player n has a strategy $s^n = (s_1^n, s_2^n, s_3^n, \ldots, s_T^n)$. Given $s = (s^1, s^2, \ldots, s^N)$, the payoff of strategy s^n is $\sum_{t=1}^{T} s_t^n \pi_t(S)$, where $S = \sum_{n=1}^{N} s^n$. The game is parameterized by $(T, N, D, F(1), P(1), r^*, r)$. We set $T = 9$, $N = 20$, $D = 6$, $F = 1.25$, $P(1) = 124.8$, $r^* = 0.1$, $r = 0.2$. The experiments used similar parameters. There were differences in number of subjects, number of claims per subject, etc., but we always adjusted the parameters such that the main theoretical predictions continued to obtain.

Proposition 1. *For the chosen parameters the unique equilibrium outcome is one where the pool is liquidated at $t = 1$.*

In our experiments (with the exception of the control sessions), participants had the opportunity to trade tickets among themselves before requesting cash-in. Let p_t denote the price of the claims in period t. We derive pricing predictions under the following rational expectations equilibrium notion.

Definition 1. (Radner-Nash Equilibrium) *Given $(T, N, D, F(1), P(1), r^*, r)$ an equilibrium in the credit market consists of a price vector $p = (p_1, p_2, \ldots, p_T)$, a net trade decision vector for each agent n, $\Delta D^n = (\Delta D_1^n, \Delta D_2^n, \ldots, \Delta D_T^n)$, and a strategy $s^n = (s_1^n, s_2^n, s_3^n, \ldots, s_T^n)$, both of which are feasible, i.e,*

(i) $p_t \Delta D_t^n \leq C - \sum_{\tau=1}^{t-1} p_\tau \Delta D_\tau + \sum_{\tau=1}^{t-1} s_\tau^n \pi_\tau(S)$, *and*

(ii) $s_t^n \leq D + \sum_{\tau=1}^{t} \Delta D_\tau - \sum_{\tau=1}^{t-1} s_{t-1}^n$ *where we set* $s_0^n = \Delta D_0^n = 0$ *for all* n,

such that the following hold:

- *Given* p *and* $\sum s^{-n}$ *each agent* n *chooses* ΔD_n *and* $s^n = (s_1^n, s_2^n, s_3^n, \ldots, s_T^n)$ *to maximize* $C - \sum_{\tau=1}^{T} p_\tau \Delta D_\tau + \sum_{\tau=1}^{T} s_\tau^n \pi_\tau$ *subject to the feasibility conditions (i) and (ii).*
- *The price* p_t *clears the claims market in period* t*, i.e.* $\sum_n \Delta D_t^n(p) = 0$.

The arguments in the previous section lead to the following claim.

Proposition 2. *In all Radner-Nash equilibria of the credit market, the pool is liquidated at* $t = 1$ *and* $p_1 = \pi_1 = I(1) = \frac{P(1)}{ND}$.

IV. Details About The Experimental Sessions

We report here on eleven experimental sessions. Seven were run at Caltech and are referred to as CIT1 to CIT7. Four sessions consisted of three replications, two consisted of two replications, and one session had four replications. Another session was ran at the Ecole Polytechnique Fédérale Lausanne, EPFL1, but had only one replication. Finally, three sessions with three replications each were run at the University of Utah, UU1, UU2, and UU3.[6] Parameter values differed slightly across experimental settings, as displayed in Table II, but we always made sure that traditional theory implied no roll-over of securities past the first round.

Shortsales were allowed and exploited in all but the five early experiments (CIT1 to CIT4 and EPFL1). When selling short, subjects were exposed to the risk of being chosen to pay the face value (or liquidation value) of a holder who submitted his/her ticket for cash in. With the exception of the first five sessions, the rest of the sessions had a trading round following the announcement of the liquidation of the pool but before the securities paid off. Thus, the participants had a choice to either buy or sell securities,

[6] All experiments were approved by the relevant Institute Review Boards for the protection of human subjects in academic research.

with the knowledge that the pool would be liquidated and all outstanding securities would pay the intrinsic value.

We will also report on five sessions where subjects could not trade their claims in a market. In all other respects, these sessions were the same as those with trading opportunities. They were ran as a control with which to determine the impact of financial markets on social rationality. See Table III for details.

In all experiments subjects were paid based on performance. For example, in all but one of the Caltech sessions subjects started with 6 claim securities, $6 of cash and a pool of money that started at value of $1.04 per claim and grew at a rate of 10% per round. The money that subjects made during the experimental session (without any conversion from experimental dollars to US dollars) was theirs to keep. Including the sign up reward, subjects made approximately $35 (or the equivalent in Swiss francs), and the range of payoffs was from about $10 to $55.

The duration of the bubbles could have been biased downward by a hardware limitation: the sessions took place in laboratories where computer mouse clicking would reveal submission of claims for cash-in after the trading round. In three of the Caltech sessions (CIT2, CIT3 and CIT4), loud background clicking noise was used to mask individual subjects clicking during cash-in request submission. Likewise, the limited total duration of the experiments (two hours) may have led to premature termination of the last replication (subjects might have been eager to leave). In CIT4, we tried to avoid such end-of-experiment effects by allotting a fixed amount of time (20 minutes) to each replication, independent of the actual duration of the replication. In the rest of the experiments, we merged trading and submissions for cash-in so that background clicking noise was no longer needed; the clicking generated by regular trading was sufficient to mask cash-in submissions.

All experimental procedures were explained in detailed instructions. A set of instructions is reproduced in the Appendix. Each session started with the experimenter reading the instructions out loud. The participants were allowed to interrupt the reading of the instructions if they had any questions. Following the instruction period, there was a practice round. The practice round replicated the first couple of rounds of the actual experiment except for the size of the money pool. The practice round helped participants familiarize themselves with the software (Flex-E-Markets) and the rules of the game before their actions counted towards the take-home pay.

V. Results

First, we always observed credit bubbles. As such, behavior never accorded with traditional theory. The pool was never liquidated after round 1, neither in the sessions with markets nor in the control sessions (without markets).

Second, financial markets did not shorten the bubble. Table IV lists the number of rounds until liquidation ("bubble duration") for each replication in each experimental session. In the first replication, bubbles lasted on average over five rounds, but decay was observed subsequently, down to two rounds in the fourth replication. In the control sessions (where subjects could not trade their claims), the number of rounds until liquidation was equally large, and the decay upon replication no less.

Figure 1 displays percentages of claims cashed in after each round in some sessions with markets. Subjects always cashed in uniformly across rounds (before termination of the money pool) in the first replication. With no exception, the incidence of cash-in after the first and the last active rounds from the first replication was reduced in the second replication; submissions became largely concentrated in a single round. The fact that often no claims were cashed in at the end of the first round suggests that cash-in decisions were deliberate and systematic, as opposed to the result of random mistakes.

Third, the presence of financial markets did substantially increase social welfare (measured as the average payout realized on cash-ins per claim unit). As Table V shows, without markets (control sessions), cash-in delays in replication 1 increased the average payout by 29% over that under Nash equilibrium. This increase is reduced to 8% by replication 3. With markets, however, average payout in the first replication increased by 36% per claim unit, and this was reduced only to 20% in the third replication.

Fourth, the cost of this increased social welfare was the risk of ending with a claim that is worth little or nothing because everyone else cashed in earlier and no money was left in the pool. As a result, the disparity of final wealth across subjects was indeed large. Figure 2 documents this. In the Radner-Nash equilibrium, everyone would make the same. Because of social rationality, subjects make more on average than in the Radner-Nash equilibrium, as pointed out before. But this is at the cost of substantial cross-sectional uncertainty.

Fifth, the claims were significantly overpriced compared to the Radner-Nash equilibrium. That is, we observed price bubbles.

To understand the nature of the bubbles, we benchmark transaction prices against: (i) The face value of the claim for the round in which the transaction took place ($F(t)$); (ii) The effective value of the claim, $E(t)$, defined as the total payments effectively made in the round of the transaction and beyond, divided by the total number of claims outstanding in that round, and (iii) the intrinsic value $I(t)$ of the claim, equal to the amount of cash in the money pool divided by the total number of claims outstanding in that round.

A bubble is defined with respect to the Radner-Nash equilibrium, and therefore a measurement of its magnitude in a given round t is the difference between the average transaction price in that round and the intrinsic value of the claim, $P(t) - I(t)$. Nevertheless, it could be argued that this price differential is not all irrational. According to the Efficient Markets Hypothesis, prices should reflect future payouts, which means that they should equal the effective value of the claim $E(t)$. Social rationality causes the effective value to be higher than the intrinsic value $I(t)$. Consequently, our bubble measure has two components, the "socially rational" part equal to $E(t) - I(t)$, and the remainder $P(t) - E(t)$. To the extent that the latter is positive, pricing is truly irrational.

The evolution of transaction prices and corresponding face, intrinsic and effective values in selected sessions is depicted in Figure 3, and Table VI provides full detail for all sessions. As the graphs demonstrate, prices always started at or above face values, which themselves were always above the intrinsic values. Hence, we robustly generated asset price bubbles, i.e., $P(t) - I(t) > 0$. With the exception of EPFL1, prices started out even above the effective value (total money effectively paid out during the experiment divided by number of claims outstanding). That is, the bubble generally included an irrational component, $P(t) - E(t) > 0$.

Sixth, pricing did satisfy essential requirements for informational efficiency. Indeed, there was a tendency for the difference between the trade prices and the effective values to diminish upon replication, so prices gradually started to reflect the true social value (total effective payout per remaining claim given actual cash-in policy). That is, the price bubble eventually reflected mostly the socially rational component $(E(t) - I(t))$.

To formalize the evidence on informational efficiency of prices, we first determined whether prices could be used to predict bubble duration. In an efficient market, this should be impossible. Table VII reports results from regressing bubble duration (measured as number of rounds until the pool was liquidated) onto the average trade price in round 1. To avoid confounding factors, we also included, as regressors, the number of participants in the session, and the replication number. The round-1 price does *not* predict bubble duration, supporting the hypothesis of an (informationally) efficient market. Notice that the duration of the bubble depended positively on the number of participants, and negatively on the replication number. While the latter merely reflects the decay we already referred to, the cause of the former is not immediately obvious.

Next, we determined to what extent trading prices reflected future payments on the claim, by regressing the effective value ont the round-1 average transaction price. Again, we avoided confounding factors by including the number of participants and replication number as regressors. Table VII shows that the round-1 trading prices predict effective value, consistent with the efficient markets hypothesis. The replication number has a significant negative impact on effective value, while effective value also correlates with the number of participants. The latter is rather surprising, because the effective value measures social gain. The finding means that the social gain increases with the number of participants, *ceteris paribus*. To put this differently: social rationality works better in larger markets; individual rationality has less of an impact as the number of participants increases.

In Table VII, we also report the results of the corresponding regressions when we added a dummy for sessions where we allowed for shortsales. (Additionally, we included a cohort dummy, to differentiate the sessions ran with Caltech subjects and those ran with University of Utah subjects, but this dummy was not significant.) While shortsales had no effect on bubble duration, it marginally reduced the effective value, and hence, the social gain.

Shortsales did have a significant effect on individual earnings. Figure 2 reports the individual subject earnings per session (bars above horizontal line) and compares this with maximum shortsell positions (bars below horizontal line). It is immediate that subjects with high earnings tend also to be the ones who took short positions.

In the first replication of a session, prices exhibited efficiency in yet another respect: a simple strategy generated a favorable reward-to-risk trade-off (Sharpe ratio) of 0.67, and this strategy was to purchase claims at the average price in the first round and to sell right after the face value had increased beyond the original purchase price. See Table VIII for a detailed report. A positive risk-reward trade-off attracts risk-tolerant purchasing of claims in early periods, and hence, helps further social rationality because cash-ins will be postponed. In subsequent replications, however, the strategy became unprofitable.

VI. Avoiding Decay

We observed unambiguous decay in the duration of the the credit bubbles. We attribute this to the strict stationarity of the replications: they were consecutive in time, and involved the same subjects and the same parameters. To test this, we subsequently changed the experimental design slightly, to determine whether these minimal changes could at least slow, if not eliminate, the decay. Essentially, we allowed subjects to change from one replication to another, while still ensuring that they had experience with credit bubbles or, at a minimum, had access to data from past replications. Also, unlike in the previous experiments, where subjects showed up in the laboratory with minimal idea about the type of experiment they would be involved in, subjects now knew the nature of the experiment, and could opt to stay out.

The experiment was organized as part of an introductory finance class at Caltech. All students were free to sign up for the replications and received fixed class credit for participation; in addition, they were paid for performance exactly as before, without any impact on their class grade. To simplify participation, the sessions were organized online, and a special web site was set up to facilitate communication and logistics (see http://www.hss.caltech.edu/~pbs/CMexp/). Students in the class came from a variety of backgrounds (graduate, undergraduate, covering diverse majors, from physics to biology and business economics and management); about 80 students were eligible to sign up for the experiments. All had experience with the trading interface, Flex-E-Markets, having participated in other experimental sessions either in-class or outside class.

We refer to the experiments as "online replications" because participants were not asked to show up in a physical laboratory; instead, they logged on to the experiment web site (see above for URL) in time for a session to start, from wherever they had internet access. Relative to the in-lab experiments discussed before, we changed one additional feature: we started the pool "in the money," which meant that the initial face value of the credit certificates was below their intrinsic value. From round 2 on, the situation was as before. We introduced this complication in order to confirm that cash-in decisions were deliberate and systematic, and not mostly driven by mistakes. We expected to (and did) observe less (or no) cash-in in round 1, because the face value was below the intrinsic value only starting from round 2.[7]

In the online replications, we had far less control over the situation than in the case of the in-laboratory experiments. We scheduled fifteen replications, but eventually had to cancel three because only a few subjects showed up online in time. Additionally, we had to discard the data from one replication, because we incorrectly accounted for subjects who had signed up but did not show up. Communication during the experiment took place through a chat window integrated in the market exchange platform. The lack of control worked both ways: while subjects knew the size of the pool backing the credit, the number of securities outstanding initially as well as the number of players, they could not have full knowledge about who else was participating because they could not even see them. Subjects used online pseudonyms during the entire experiment.

From the first to the last replication, the durations of the bubbles, in rounds, were: 5, 5, 4, 6, 8, 8, 6, 6, 8, 6, 7. As such, there is no evidence of decay. Cash-in histories, plotted in Figure 4, showed little or no cash-in requests in round 1 (as expected), while requests in round 2 gradually declined over time, though not uniformly.

Table IX provides information of the evolution of transaction prices ("Trade"), face values ("Face"), intrinsic values ("INT"), as well as effective remaining values ("EFF"). A number of features replicate the in-laboratory experimental sessions.

[7] In the online experiments, at most 2% of the tickets were cashed in after the first round in all 11 sessions; while when we started the pool out of the money in the earlier experiments, 5% or more were cashed in after the first round in 8 out of 11 sessions (first replication).

1. The average transaction price in round 1 again provides an indication of the minimum (but not actual) duration of the pool. In all sessions, the pool ran out of money only after the face value increased to a level above the average trade price in round 1.

2. Substantial social welfare was created: the ratio of the initial effective value over the initial intrinsic value ranges from 1.37 (1.96/1.43,[8] Nov 16 11 5:30pm session) to a low of 1.08 (1.55/1.43, Nov 1 11 5pm session). A ratio of 1.37 indicates that participants as a group earned 37% more than they would have in the Radner-Nash equilibrium.

3. There was no trend in trade prices in early rounds. However, when the pool survived beyond round 4, prices tended to increase.

4. While falling often dramatically relative to prior trading rounds, prices after anouncement of liquidation invariably are too high compared to the liquidation value (intrinsic value of last round).

5. Trade prices in round 1 are still too high compared to effective values, but the gap is reduced; in the first session (Nov 1 11 4pm session), the average trade price (1.71) is pretty much equal to the effective value (1.72).

While initial trade prices are still too high relative to effective values ($P(t) - E(t) > 0$), the strategy of buying at average prices in round 1 and selling at average prices in the round where the face value increases above the original purchase price generates a good risk-return trade-off: its Sharpe ratio equals 0.89. See Table X. When cashing in rather than selling in the marketplace, the return is reduced significantly, and the Sharpe ratio is a mere 0.02. Both strategies exhibit negatively skewed returns. The profitability of this investment strategy would reinforce the credit market bubble. Speculators would

[8] The round-two intrinsic value is used as benchmark, because nobody should cash in after round 1, in which the face value is below the intrinsic value of round 2 (this intrinsic value does assume that nobody actually cashes in after round 1, which did not always obtain.

buy in round 1 and sell in a later round, thus postponing cash-in of the tickets they bought, potentially delaying liquidation of the pool.

Overall, the online class experiments show that with less control, bubbles continue to emerge upon replication. It thus takes minimal noise in the system for social welfare to continue to be enhanced. The finding is reminiscent of information aggregation experiments, which produced mixed results [Plott and Sunder, 1988] because without noise they run into the curse of no-trade theorems [Milgrom and Stokey, 1982]. A disciplined way to introduce noise is to provide incentives to trade because of risk or ambiguity concerns. In information aggregation experiments [Bossaerts et al., 2011], this has led to the disappearance of anomalies such as "information mirages" [Camerer and Weigelt, 1991]. In future experiments, we plan to use trading for risk re-allocation purposes as a vehicle to ensure survival of social rationality, and hence, social welfare generated by credit market bubbles.

VII. Discussion

Naturally, the question is why we see bubbles and why we can replicate them. Human "foolishness" cannot be the sole explanation as subjects overall ended up making more than the equilibrium payoff. This behavior is similar to what is observed in the trust game (Berg et al. [1995]), where both investor and trustee obtain higher payoffs than predicted by Nash equilibrium. What is common to the trust and the centipede game is the availability of actions that, if collectively taken, make everyone better off. Only, these actions are subject to defection: the best response to everybody else choosing them is not to choose them oneself (which explains why they cannot be part of a Nash equilibrium).

Barring mistakes, the observed behavior in such games has been attributed to non-selfish attitudes (altruism, reciprocity, fairness, trust, etc.). Non-selfish behavior, which we refer to here as "social rationality," can be explained as the result of social norms (Camerer and Fehr [2004], Fehr and Fischbacher [2004]), and consistent with this, its emergence has been shown to depend critically on the cohort. In an influential cross-cultural study of behavior in ultimatum, public goods, and dictator games, Henrich et al. [2005] have shown that, for example, that "the higher the degree of market integration and the higher the payoffs to cooperation in everyday life, the greater the level of prosociality expressed in experimental games."

Social norms effectively ensure better outcomes (Pareto-improvements) for the group as a whole in situations where self-interested behavior (best-responding to other players' actions) would be detrimental. Social norms do not merely prescribe specific actions in specific games, but are a general code of conduct whereby individuals engage in non-selfish behavior despite the risk of being confronted with defection. Social norms ensure beneficial social allocations even among strategically sophisticated players (Camerer [1997]).

Adherence to social norms requires well-calibrated beliefs about what these norms are. For instance, in the trust game (Berg et al. [1995]), the entrusting investor makes a decent return in expectation even though he is occasionally defected upon by the trustee. It has been demonstrated that mis-calibrated beliefs about other players' likely actions in the trust game are positively associated with general socially maladapted behavior, such as borderline personality disorder (King-Casas et al. [2008]).

Social rationality has become an integral part of a new line of game theory known as behavioral game theory (Camerer [2003]). Behavioral game theory aims at explaining actual human behavior (and also non-human primate behavior, work in progress) based on different computational principles (e.g., the ability to influence value in repeated games as in Camerer et al. [2002] and Hampton et al. [2008], or the formation of belief hierarchies in one-shot games as in Camerer et al. [2004] and Coricelli and Nagel [2009]), in addition to social rationality. While traditional game theory provides a useful benchmark, behavioral game theory is rapidly replacing it to explain actual social interaction. Ironically, as King-Casas et al. [2008] indicates, traditional game theory continues to apply when players suffer from mental disorders.

Social rationality supports Pareto-superior allocations, when such exist, in strategic interaction situations. Here, we conjectured that this principle also applies to competitive financial markets. Our conjecture is based on prior published experimental evidence from one of us, who studied allocations in situations where the competitive equilibrium is not Pareto optimal. Specifically, Asparouhova [2006] studied prices and allocations in markets for bank loans under asymmetric information. Markets failed to settle on the competitive equilibrium when the equilibrium was not Pareto-optimal. Pareto-improving loan contracts continued to be offered despite the constant threat of cherry-picking by competitors (who offered new contracts with the sole purpose of attracting only the low-risk customers). With small exceptions, lenders refrained from cherry-picking, and (low-risk) borrowers did not wait for the lenders to offer the cherry-picking contracts. Nonetheless, the loan markets never completely settled on the Pareto-optimal allocations when these were not the competitive outcome. Instead, markets cycled between the competitive and the Pareto-optimal outcomes.

In Asparouhova [2006], the effects were significant, but not big. The resulting allocations did not deviate enough from competitive predictions and an outsider who did not know the underlying parameters (the actual distribution of risks in the economy) would mistakenly conclude that the competitive equilibrium obtained under the estimated parameters. Here, we studied a competitive market situation where Pareto improvements over the competitive equilibrium did generate big effects on prices and allocations.

Consistent with the emergence of the Pareto-improving, non-competitive outcomes, prices in our experiments were above competitive levels, but (necessarily) dropped sharply before the end of the game. Importantly, the presence of financial markets facilitated the emergence of social rationality. While the resulting allocations were beneficial on average, we did observe huge cross-sectional disparities in final wealth. *Ex ante*, many (and especially the investment savvy participants) would prefer to have competitive financial markets where one could trade claims, even if this may create huge wealth inequalities. Of course, an even better outcome would be to forbid early cash in, or to restrict access to claims to a group of creditors who are willing to "hold their breath" and not cash in early. Alternatively, one participant could just buy the entire supply of tickets and hold on to them until the last round. We explicitly ruled out that possibility (the experimental instructions specified that if one participant managed to buy all remaining tickets, the pool would be liquidated immediately). It would be interesting to study pricing in future experiments where we allow participants to become the sole holder of all outstanding tickets.

Our findings have implications for debt markets. Our allowing participants to cash in claims is homologous to a situation where claims are all short term and need to be re-financed every period. To cash in a claim corresponds to a refusal to re-finance. Our findings suggest that re-financing risk can be detrimental. Individual rationality may cause debt issues not to be re-financed when social rationality dictates that everyone would be better off to roll over debt and collect rewards in the future. The risk is real when debt holders cannot somehow be disciplined to serve the common good. In such situation, it is best for debtors to issue long-term claims, i.e., securities with maturities optimally matched to income streams.

Because overpricing re-emerges upon replication, our design allows one to study asset price bubbles experimentally. We think that our setting is more intuitive than the traditional one, pioneered in Smith et al. [1988]. Moreover, rational pricing in our setting is unequivocal. We would also claim that our new setting captures the essence of real-life bubbles. First, its emergence is beneficial for the group (the total payout to the subjects is higher than in the absence of bubbles). Second, the bubble eventually will burst, which leads to a huge re-distribution of wealth. Third, bubbles robustly re-emerge upon replication when there is noise, and hence, appear to be a natural phenomenon.

As mentioned before, our setting has something in common with the theory of bank runs (Diamond and Dybvig [1983]). Still, there are fundamental differences. In bank run models, the face value of the claims is such that everyone can be paid provided most players roll over claims until the end. There is a shortfall only if most cash in early. In our case, the pool never holds enough money to pay all claim holders. Here, the mere observation that anyone rolls over claims is inconsistent with Nash equilibrium, while roll- over of claims in the theory of bank runs *is* consistent with Nash equilibrium. Also, bank runs are detrimental to social welfare, while bubbles in our setting are beneficial.

Experiments on bank runs are relevant for us in one important respect: they allow us to argue that one alternative cause of non-Nash behavior cannot be the entire story, namely, random mistakes (relative to Nash behavior). Ignoring markets, in our setting, mistaken choices can be only of one kind: to roll over claims. In contrast, in bank run settings with multiple intermediate occasions to roll over, mistakes can be of two kinds: to roll over claims too long, or to stop rolling over too early. In principle, one would expect these two types of mistakes to occur equally often. At the same time, one of these mistakes, to roll over claims too long, is beneficial for the group as a whole (it is in our setting too), because it increases the pool of money from which certificates of deposit are paid. So, if social norms explain subjects' behavior, we would expect to see more "mistakes" of this kind. Schotter and Yorulmazer [2009] have shown that this is indeed the case, casting doubt on theories that rely entirely on random mistakes to explain non-Nash behavior (such as cognitive hierarchy modeling; see, e.g., Kawagoe and Takizawa [2010], for an application to the centipede game). Explicit estimation of the relative contributions of social rationality and random mistakes have been computed for another game, namely, the public goods contribution game Goeree et al. [2002], Anderson et al. [1998].

To be sure of the importance of social rationality in driving non-Nash behavior, we ran two additional sessions, where we randomly changed the growth of the money pool across replications, from 10% per round (as in the original experiments), to zero percent and -10%. We ran these experiments without the additional complication of markets. That is, subjects could not trade their claims. In each session, two replications were run per growth level. If social rationality (i.e., concern for Pareto improvement) plays a role in subject behavior, we would expect the duration of the pool (number of periods until liquidation) to be significantly higher when the pool grows than when it does not or when the pool shrinks. When the pool does not grow, there are no Pareto improvements from waiting; when the pool is reduced, remaining claimholders are worse off when delaying cash-in. Likewise, we expect significantly fewers cash-in requests in the first round when the pool grows. If behavior is entirely generated by random mistakes, there should be no treatment effects from changing the growth of the pool.

Table XI displays the results from regressing pool duration and (percentage) number of cash-in requests in the first period on pool growth (+1 if the pool grows by 10%, 0 if the pool remains stationary between periods, and -1 if the pool shrinks by 10%). Secular decay is corrected for by means of replication dummies. Growth increases pool duration ($p = 0.01$) and decreases period-1 cash-in requests ($p < 0.01$), confirming our hypothesis that social rationality contributes significantly to non-Nash behavior.[9]

Like bank runs, rational bubbles [Blanchard, 1979, Blanchard and Watson, 1982, Tirole, 1985] are also equilibrium phenomena while price bubbles in our credit market context are not. Rational bubbles can emerge in infinite-horizon overlapping generations models. There, Walras' law no longer holds, and as a result, allocations in competitive equilibrium may no longer be Pareto optimal. Indeed, equilibria with bubbles generally improve on a bubble-less equilibrium [Tirole, 1985]. As such, rational bubbles, like our credit market bubbles, are beneficial for the group as a whole.

[9]Further details about the experimental sessions with changing pool growth, such as instructions, can be obtained upon request.

Once a credit market bubble is started, agents need to assess what others think (to correctly anticipate pricing) and what others will do (to correctly roll over securities). To correctly anticipate beliefs and actions of others is also at the core of beauty contests (Nagel [1995]). In a beauty contest, players have to guess a number that is closest to a fraction (< 1) of the mean, median or maximum guess of the other players. There too, the traditional game-theoretic outcome (namely, to guess zero) does not obtain, at least not in early rounds. Unlike in our setting, however, there is no benefit to the group as a whole from playing a non-Nash strategy (namely, to guess any number larger than zero). Behavior during early rounds of a beauty contest can be explained in terms of limited strategic sophistication (i.e., truncated belief hierarchies; Camerer et al. [2002], Coricelli and Nagel [2009]). Upon repetition, one expects mistakes to decrease, and indeed convergence to Nash equilibrium is observed in the 1/2 median guess settings [Duffy and Nagel, 1997] after 10 replications.

Because non-Nash behavior is socially beneficial in our setting, we could expect social rationality to prevent convergence to equilibrium. In our tightly controlled in-lab experiments, we nevertheless see decay towards Nash equilibrium. In the more loosely controlled, and hence, noisy online experiments, however, there is no decay. The latter corroborates an important point first raised by the late Fisher Black. Specifically, Black [1986] wrote that "Noise makes trading in financial markets possible, and thus allows us to observe prices for financial assets. Noise causes markets to be somewhat inefficient, but often prevents us from taking advantage of inefficiencies." Our online experiment extends these ideas: there, we introduced a sufficient amount of noise for individual rationality to dominate, allowing for social rationality to stay upon replication even if it is in conflict with individual rationality. The in-laboratory experiments, in contrast, are too transparent (i.e., lack the noise) and hence, social rationality decays with replication.

But in our online experiment, we may have introduced noise in a way that is not sufficiently disciplined. A better way would be to introduce random liquidity shocks, or additional uncertainty that incentivizes market participants to trade for other reasons than just speculating when the pool runs out of money. We have used this approach successfully to eliminate anomalous pricing ("information mirages") in prediction markets [Bossaerts et al., 2011]. Future experiments on credit market bubbles should borrow design ideas from these successful experiments on information aggregation.

VIII. Conclusion

Alleged overpricing and subsequent sudden drops in prices have been documented for many financial markets and many periods even pre-dating the industrial revolution. With few exceptions they have an enormous effect on the real economy, yet little is understood about those bubbles. Field studies have been controversial because of lack of information about true fundamental values. For a variety of reasons, past attempts to study bubbles experimentally have likewise not been influential. Here, we propose a novel experimental design that, according to our experimental results, proves to be realistic, robust and unequivocal. Our design provides a canonical setting in which to study the emergence and bursting of the financial bubbles that have introduced so much uncertainty and disruption in modern, market-based economies. The study of those bubbles will, however, require new theory because the pricing cannot be explained with the theory we have now. Just as the anomalies in the underlying centipede game led to behavioral game theory, our experimental findings underscore the need for behavioral asset pricing theory. That is, we expect that our findings will lead to a new asset pricing theory that will build on the central tenets of behavioral game theory.

Notice that this is distinct from behavioral finance, which studies asset pricing when agents are boundedly rational. Behavioral finance is based on the heuristics and biases approach to individual decision making pioneered in Tversky and Kahneman [1974]; in behavioral asset pricing, agents are computationally and strategically sophisticated, but understand the value of social norms in enhancing the social welfare of everyone. That is, they are socially rational. Bubbles emerge because of social rationality. Subsequent crashes occur when individual rationality conflicts with social rationality and wins out.

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Appendix: Instruction Set

INSTRUCTIONS

Summary

This experiment concerns a market in tickets (claims) issued against a pool of money. Across a number of rounds, you will be able to trade the tickets with others at prices determined in the marketplace. Each round, you will also be able to submit the tickets to us, the experimenters, to be cashed in for a known amount called the face value. This face value grows over time (across rounds). The pool of money against which the tickets are issued is used to pay those who want to cash in. This pool also grows over time, but at a slower rate than the face value. If there is insufficient money in the pool to honor the requested cash-ins at face value, the pool is put in liquidation and all outstanding tickets will be paid pro rata.

The experiment will last at most 9 rounds. Your earnings for the experiment depend entirely on the cash you hold at the end of the last round. Changes in cash are determined by your trading choices (you want to buy low and sell high) and cash-in decisions (you want to maximize income from ticket submissions).

Details Of The Setup

In round 1, you and the other participants hold a total of N tickets (N will be announced at the beginning of the experiment). Each ticket has an initial face value of $1.25. In Round 2 the face value goes up by 20% and becomes $1.50. The face value continues to grow at the rate of 20% (subject to rounding to the nearest cent) in each subsequent round. The face values in all rounds are listed in red in the second column of the table below.

Round	Ticket Face Value (USD)	Value (USD) If Liquidation*
1	1.25	1.04
2	1.50	1.15
3	1.80	1.26
4	2.16	1.39
5	2.59	1.53
6	3.11	1.68
7	3.73	1.85
8	4.48	2.03
9	5.37	2.23

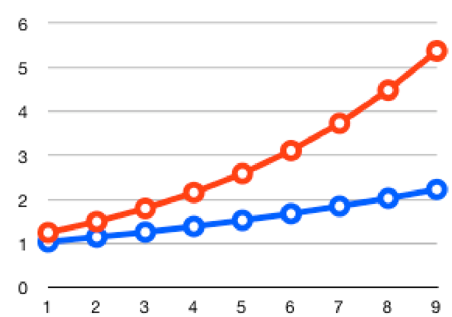
*Assuming no tickets were submitted for cash-in earlier

Each ticket is a claim to a pool of money. The pool of money starts with $X in Round 1 of the experiment (X will be announced at the beginning of the experiment). Given the total number of tickets (N), there is insufficient money in the pool to pay everyone the face value even if *all* tickets are submitted for cash-in. For instance, at the end of round 1, there is $X in the pool, so the amount of money per tickets, X/N, is insufficient to cover the face value, $1.25. But the pool of money grows over time, so that there is more money available per ticket if everyone waits to submit tickets. The pool grows *after* paying tickets submitted for cash-in.

When not enough money is available to honor the requested cash-ins, the pool is put into liquidation, and the remaining money is divided among all outstanding tickets. If, say, $45 is left in the pool when it runs out of money, and, say, 30 tickets remain outstanding, each ticket fetches $45/30=$1.50. This liquidation payment will necessarily be less than the face value even if nobody cashed in earlier. The third column of the Table above shows, in blue, how much each ticket would be paid if no tickets were submitted for cash-in earlier and the pool is liquidated after rounds 1,2,3,….,9, respectively.

Importantly, you will *not* know how many tickets were submitted in any given round (except your own, of course). As such, you will never be sure whether the pool will be put into liquidation until we announce so.

The figure below is a graphical depiction of the Table above; the red line indicates how the face value increases per round, and the blue line depicts how the liquidation value climbs.



In addition to tickets, you will begin the experiment with some cash. Use this cash to purchase more tickets if you wish. You can increase your cash either by selling tickets in the open market or by submitting tickets for cash-in. At the end of the experiment, <u>all the cash you have is yours to keep</u>.

Trading And Requesting Cash-Ins

In each round, you will be able to trade the tickets among yourselves in a market called **Public**. You can buy tickets and thus increase the number of tickets you held in the beginning of the round, or sell tickets, and thus decrease your ticket allotment. Public is a "public" market: everyone can see all orders that have been submitted, and trade always takes place at the best possible price.

In **Public**, you are allowed to short sell tickets. When you short sell, you keep the purchase price; however, if the buyer decides to submit your ticket for cash-in, we will make YOU pay for the face value or liquidation payment (whichever applies to tickets that were not acquired in a short sale). That is, we will take the payment out of YOUR cash, not the money pool. If you do not have enough cash, we will take away all your cash as penalty, and approach another shortseller to cover the liability. Notice that this means that you (as shortseller) may end up paying for someone else's shortsales!

In addition, you will be able to submit for cash-in *some*, *all*, or *none* of your tickets by submitting sell orders in a market called **Cash-In**. This is the market you should use to request cash-in of tickets against the pool of money. Unlike Public, in this market you need to identify the person you want to sell to. Thus, you should only submit sell orders to us, the experimenters; we will be logged in as the participant with name “50.” The Cash-In market is a private market, which means that nobody else will be able to see your order. Your request would be “valid” as long as you submit a sell order at a price equal to or below the face value.

Do not submit any other orders in Cash-In. If you submit an order to buy, or attempt to submit an order to sell to someone else besides us (“50”), you may forfeit your earning for the session.

While markets are open for trading, nothing happens to your sell orders in the Cash-In market (which also means that you may cancel them at any time before markets are paused). When markets are paused at the end of the round, the following will happen to your valid sell orders in the market Cash-In.

1. If the money pool is sufficient to cover the face values for all submitted sell orders, we will buy each submitted ticket for the face value for that round (as listed in the Table above).
2. If there is not enough money in the pool to honor all sell orders at the face value, the pool will be put in liquidation. This means that *all* remaining tickets (not only

those submitted through sell orders in Cash-In) will receive the liquidation payment.

Liquidation proceeds as follows. First, we announce publicly that there is not enough money left in the pool. We then allow for one more trading round before we actually liquidate the pool, so that you still have the opportunity to trade tickets among each other before we pay the tickets. While Cash-In will remain open during that extra trading round, you do not need to submit orders in that market. At the end of the extra round, we pay the liquidation payment to *all* outstanding tickets.

The maximum number of rounds is 9. If at the end of any round before round 9, only one participant is still holding tickets, the experiment terminates automatically, and the remaining tickets are paid either the face value or the liquidation value, whichever is smaller.

Markets are open for 3 minutes in rounds 1 to 3, and for 2:30 minutes in all remaining rounds. There will be three sessions. Including instructions, practice, inevitable pauses between sessions and rounds, the experiment lasts about two hours.

Good luck!

Figures

Tables

Table I

For each listed cash-in pattern and a round t, the table displays the intrinsic value $I(t+1)$, the face value $F(t)$, and the actual payoff from rolling over a claim until t and then cashing. Assumptions: 4 rounds; 11 players with endowment of 1 claim each, money pool of \$12, growing at 10% per round; face values $F(t) = \$1.25 \times 1.2^{t-1}$. First column describes strategies of others (number of players cashing in each of the four rounds); second column displays round where it is optimal to cash-in (always the round before the last remaining players cash-in).

Admissible Cash-In Strategy Of Players 1 to 10	Optimal Cash-In Round for Player 11 t	$I(t+1)$ ($t+1$ = the round when last cash-ins are made)	$F(t)$	Actual Payoff From Cash-In At t
(0, 0, 6, 4)	3	0.82	1.80	1.80
(0, 0, 10, 0)	2	1.32	1.50	1.50
(0, 4, 4, 2)	3	0.26	1.80	1.58
(4, 0, 4, 2)	3	0.47	1.80	1.69

Table II

Experimental Sessions (With Markets): In all sessions the initial face value of the claims was \$1.25 and grew by 20% each round. The money in the pool in all sessions grew by 10% each round. Every subject was initially endowed with 6 claims and 6 USD (or Swiss Francs).

	Experiment	Date	Number of Replications	Number of Subjects	Initial Money in the Pool
1	CIT1	May 19 2010	2	21	132
2	CIT2	August 11 2010	2	14	87.5
3	CIT3	August 12 2010	3	18	112.5
4	CIT4	August 20 2010	3	13	81.25
5	CIT5	January 12 2011	3	10	62.40
6	CIT6	January 13 2011	4	14	87.36
7	CIT7	January 14 2011	3	15	93.60
8	EPFL1	May 21 2010	1	16	106
9	UU1	February 1 2011	3	16	99.84
10	UU2	February 2 2011	3	19/13/13	118.56/81.12
11	UU3	February 4 2011	3	10	62.40

Table III

Experimental Sessions (No Markets): In all sessions the initial face value of the claims was $1.25 and grew by 20% each round. The money in the pool in all sessions grew by 10% each round. Every subject was initially endowed with 6 claims.

	Experiment	Date	Number of Replications	Number of Subjects	Initial Money in the Pool
1	NTCIT1	January 12 2011	3	8	49.92
2	NTCIT2	January 12 2011	3	12	74.88
3	NTCIT3	January 13 2011	5	12	74.88
4	NTCIT4	January 13 2011	4	5	31.20
5	NTCIT5	January 14 2011	3	12	74.88

Table IV

Bubble duration. Number of rounds before pool had insufficient money to pay submitted cash-in requests. Sessions without markets ("No Markets") as well as sessions with markets ("Markets").

No Markets	Replication 1	Replication 2	Replication 3	Replication 4	Replication 5
NTCIT1	6	4	3		
NTCIT2	5	3	2		
NTCIT3	6	4	3	2	2
NTCIT4	5	3	2	8	
NTCIT5	5	4	3		
Average	5	4	3	5	2

Markets	Replication 1	Replication 2	Replication 3	Replication 4	Replication 5
CIT1	6	5			
CIT2	5	3			
CIT3	5	4	3		
CIT4	6	4	3		
CIT5	5	3	3		
CIT6	5	4	3	2	
CIT7	5	4	4		
EPFL	7				
UU1	6	5	4		
UU2	6	4	3		
UU3	4	3	3		
Average	5.45	3.9	3.25	2	

Table V

Social welfare created over Nash or Radner-Nash equilibrium. Measured as the percentage increase in per-claim effective payout over round-1 intrinsic value. Sessions without markets ("No Markets") as well as sessions with markets ("Markets").

No Markets	Replication 1	Replication 2	Replication 3	Replication 4	Replication 5
NTCIT1	0.27	0.20	0.11		
NTCIT2	0.26	0.17	0.06		
NTCIT3	0.40	0.25	0.10	0.03	0.01
NTCIT4	0.23	0.16	0.04	0.09	
NTCIT5	0.27	0.17	0.08		
Average	0.29	0.19	0.08	0.06	0.01

Markets	Replication 1	Replication 2	Replication 3	Replication 4	Replication 5
CIT1	0.28	0.33			
CIT2	0.24	0.26			
CIT3	0.25	0.25	0.17		
CIT4	0.36	0.31	0.21		
CIT5	0.38	0.24	0.15		
CIT6	0.41	0.28	0.2	0.12	
CIT7	0.38	0.3	0.21		
EPFL	0.53				
UU1	0.4	0.39	0.32		
UU2	0.44	0.33	0.19		
UU3	0.24	0.18	0.15		
Average	0.36	0.29	0.20	0.12	

Table VI
Prices, effective and intrinsic values, sessions with markets. See text for definition of effective and intrinsic values.

			Replication 1			**Replication 2**			**Replication 3**			**Replication 4**		
Exp	Round	Face Value	Average Price	Effective Price	Intrinsic Value	Average Price	Effective Price	Intrinsic Value	Average Price	Effective Price	Intrinsic Value	Average Price	Effective Price	Intrinsic Value
CIT1	1	1.25	1.72	1.28	1.00	1.87	1.33	1.00						
	2	1.50	1.74	1.28	1.07	1.94	1.33	1.10						
	3	1.80	1.96	1.23	1.08	1.89	1.33	1.19						
	4	2.16	2.24	1.10	1.00	2.04	1.26	1.22						
	5	2.59	2.65	0.84	0.78	no trade	0.73	0.73						
	6	3.11	3.16	0.20	0.14									
CIT2	1	1.25	1.45	1.24	1.04	1.66	1.26	1.04						
	2	1.50	1.43	1.24	1.13	1.67	1.26	1.15						
	3	1.80	1.75	1.09	1.00	1.51	1.25	1.25						
	4	2.16	2.20	0.85	0.81									
	5	2.59	2.58	0.55	0.55									
CIT3	1	1.25	1.71	1.25	1.04	1.75	1.25	1.04	1.65	1.17	1.04			
	2	1.50	1.75	1.24	1.09	1.77	1.25	1.14	1.36	1.16	1.12			
	3	1.80	1.71	1.22	1.15	1.73	1.22	1.22	1.68	0.86	0.86			
	4	2.16	2.21	0.91	0.90	2.02	0.09	0.09						
	5	2.59	2.69	0.25	0.25									
CIT4	1	1.25	2.05	1.36	1.04	1.94	1.31	1.04	1.84	1.21	1.04			
	2	1.50	1.94	1.37	1.12	1.82	1.31	1.15	1.40	1.21	1.15			
	3	1.80	1.96	1.36	1.21	1.70	1.31	1.26	1.34	1.08	1.08			
	4	2.16	2.15	1.34	1.30	2.04	0.98	0.98						
	5	2.59	2.12	0.80	0.80									
	6	3.11	2.47	0.08	0.08									
CIT5	1	1.25	2.08	1.38	1.04	1.83	1.24	1.04	1.44	1.15	1.04			
	2	1.50	2.02	1.38	1.14	1.67	1.24	1.14	1.49	1.15	1.14			
	3	1.80	2.00	1.38	1.26	1.25	1.19	1.19	1.07	0.18	0.18			
	4	2.16	2.01	1.35	1.34									
	5	2.59	2.31	0.28	0.28									
CIT6	1	1.25	2.04	1.41	1.04	1.83	1.28	1.04	1.66	1.20	1.04	1.46	1.12	1.04
	2	1.50	1.98	1.42	1.13	1.72	1.28	1.13	1.56	1.20	1.12	1.32	1.08	1.08
	3	1.80	2.05	1.42	1.24	1.71	1.28	1.25	1.54	1.11	1.11			
	4	2.16	2.15	1.41	1.35	1.66	0.82	0.82						
	5	2.59	2.57	1.04	1.04									
CIT7	1	1.25	1.75	1.38	1.04	1.76	1.30	1.04	1.66	1.21	1.04			
	2	1.50	1.73	1.38	1.14	1.78	1.30	1.14	1.45	1.21	1.14			
	3	1.80	1.77	1.38	1.26	1.85	1.30	1.25	1.45	1.02	1.01			
	4	2.16	1.77	1.21	1.14	1.88	0.96	0.96	1.75	0.27	0.27			
	5	2.59	2.41	0.97	0.97									
	7	1.25	1.94	1.53	1.10									
EPFL	2	1.50	1.71	1.56	1.20									
	3	1.80	1.99	1.56	1.27									
	4	2.16	2.27	1.54	1.34									
	5	2.59	2.64	1.48	1.38									
	6	3.11	3.00	1.27	1.27									
	7	3.73	3.73	0.06	0.06									
UU1	1	1.25	1.94	1.40	1.04	1.91	1.39	1.04	1.88	1.32	1.04			
	2	1.50	2.01	1.41	1.13	1.95	1.39	1.13	1.91	1.32	1.14			
	3	1.80	1.95	1.40	1.20	1.96	1.39	1.24	1.90	1.32	1.25			
	4	2.16	1.95	1.37	1.28	2.11	1.38	1.34	1.53	1.12	1.12			
	5	2.59	1.95	0.95	0.88	1.99	0.75	0.75						
	6	3.11	1.95	0.76	0.76									
UU2	1	1.25	1.72	1.44	1.04	1.83	1.33	1.04	1.56	1.19	1.04			
	2	1.50	1.77	1.44	1.14	1.74	1.34	1.14	1.39	1.19	1.14			
	3	1.80	1.77	1.44	1.24	1.67	1.34	1.25	1.39	0.96	0.96			
	4	2.16	1.99	1.41	1.32	1.48	1.22	1.22						
	5	2.59	2.37	1.09	1.06									
	6	3.11	2.82	0.50	0.50									
UU3	1	1.25	1.67	1.24	1.04	1.66	1.18	1.04	1.18	1.15	1.04			
	2	1.50	1.52	1.23	1.13	1.54	1.16	1.09	1.40	1.13	1.11			
	3	1.80	1.75	1.16	1.13	1.74	1.02	1.02	1.45	0.52	0.52			
	4	2.16	2.07	0.62	0.62									

Table VII
Bubble duration and effective value regressions (t statistics in parentheses)

Dep. Var.		Av. Round 1 Price	Replication Number	Participants Number	Short Sell Dummy	Utah Dummy
Duration	Coef.	1.0023	-0.8953	0.1212		
	t-stat	(1.4579)	(-6.0982)	(3.2442)		
	Coef.	1.0969	-0.8780	0.1190	-0.1033	0.2288
	t-stat	(1.5270)	(-5.6821)	(2.7779)	(-0.3303)	(0.7722)
Effective Value	Coef.	0.2327	-0.0439	0.0058		
	t-stat	(4.4388)	(-3.9233)	(2.0371)		
	Coef.	0.2363	-0.0460	0.0086	0.0345	0.0204
	t-stat	(4.8498)	(-4.3874)	(2.9538)	(1.6263)	(1.0133)

Table VIII
Return from buying one ticket at average trade prices in round 1 and selling at average trade prices in the round when the face value increased above the purchase price.

Experiment	Replication 1	Replication 2	Replication 3	Replication 4
CIT1	0.14	0.09		
CIT2	-0.02	-0.09		
CIT3	0.00	-0.01	0.02	
CIT4	0.05	0.05	-0.25	
CIT5	-0.04	-0.35	0.04	
CIT6	0.05	-0.09	-0.07	-0.09
CIT7	0.01	0.05	-0.12	
EPFL	0.17			
UU1	0.01	0.10	-0.19	
UU2	0.03	-0.19	-0.11	
UU3	0.05	0.05	0.18	
Sharpe Ratio	0.67	-0.27	-0.43	NA
Skewness	1.13	-1.24	0.59	

Table IX
Experimental sessions where participants self-selected from of an introductory finance class pool: Transaction prices ("Trade"), face values ("Face"), intrinsic values ("INT"), as well as effective remaining values ("EFF"), per round. Numbers in parantheses are average transaction prices for trading round following announcement of liquidation.

Date	Variable	Round								
Time		1	2	3	4	5	6	7	8	9
(All)	Face	1.25	1.5	1.8	2.16	2.59	3.11	3.73	4.48	5.37
Nov 1 11	Trade	1.71	1.80	1.82	1.92	2.03	(1.93)			
4pm	INT	1.30	1.43	1.57	1.66	1.43				
	EFF	1.72	1.72	1.75	1.74	1.43				
Nov 1 11	Trade	1.91	N.A.	1.78	N.A.	1.87	(1.93)			
4:30pm	INT	1.30	1.43	1.57	1.65	0.32				
	EFF	1.68	1.68	1.69	1.65	0.32				
Nov 1 11	Trade	1.88	1.93	1.92	2.15	(2.42)				
5pm	INT	1.30	1.44	1.55	1.36					
	EFF	1.55	1.58	1.61	1.36					
Nov 2 11	Trade	1.90	1.95	2.09	2.08	2.08	2.76	(2.02)		
4pm	INT	1.30	1.43	1.57	1.70	1.06	1.17			
	EFF	1.77	1.77	1.78	1.78	1.17	1.17			
Nov 2 11	Trade	2.28	2.13	2.12	2.44	2.55	2.95	NA	NA	(2.77)
4:30pm	INT	1.30	1.43	1.57	1.71	1.75	1.35	0.81	0.53	
	EFF	1.93	1.93	1.93	1.94	1.88	1.44	0.85	0.53	
Nov 2 11	Trade	2.35	2.18	1.94	2.05	2.48	3.05	3.35	3.85	(2.77)
5pm	INT	1.30	1.43	1.57	1.67	1.60	1.38	0.91	0.69	
	EFF	1.84	1.85	1.87	1.89	1.77	1.48	0.97	0.69	
Nov 16 11	Trade	2.04	1.90	2.01	2.29	2.92	NA	(1.95)		
4pm	INT	1.13	1.43	1.57	1.70	1.83	1.53			
	EFF	1.92	1.92	1.92	1.93	1.92	1.53			
Nov 16 11	Trade	2.05	2.05	2.16	2.24	2.51	3.15			
4:45pm	INT	1.30	1.43	1.57	1.73	1.89	1.27			
	EFF	1.93	1.95	1.95	1.95	1.95	1.27			
Nov 16 11	Trade	2.15	2.20	2.30	2.47	2.65	2.96	3.62	4.41	(3.30)
5:30pm	INT	1.30	1.46	1.59	1.67	1.72	1.84	1.22	1.22	
	EFF	1.96	1.98	2.03	2.05	2.01	1.98	1.32	1.22	
Nov 26 11	Trade	2.57	2.62	2.51	2.50	2.44	2.56	(2.12)		
10:00am	INT	1.30	1.43	1.57	1.71	1.88	1.19			
	EFF	1.91	1.91	1.93	1.93	1.93	1.19			
Nov 26 11	Trade	2.50	2.51	2.48	2.32	2.39	2.89	3.24	(2.00)	
11:00am	INT	1.30	1.43	1.57	1.71	1.36	0.82	0.90		
	EFF	1.82	1.82	1.82	1.82	1.46	0.90	0.90		

Table X

Experimental sessions where participants self-selected from of an introductory finance class pool: Return from buying one ticket at average trade prices in round 1 and either cashing in the round when the face value increased above the purchase price ("Cash-In") or selling at average trade prices in the round when the face value increased above the purchase price ("Selling In Market"). Sharpe ratio of investment strategy and skewness of the distribution of returns is shown in the last two rows.

Date-Time	Return (in percentage)	
	Cash-In	Selling In Market
Nov 1 11 4pm	5.06	6.37
Nov 1 11 4:30pm	12.98	-2.35
Nov 1 11 5pm	-67.32	14.06
Nov 2 11 4pm	13.49	9.28
Nov 2 11 4:30pm	13.80	12.04
Nov 2 11 5pm	10.12	5.29
Nov 16 11 4pm	5.87	11.90
Nov 16 11 4:45pm	5.14	9.03
Nov 16 11 5:30pm	0.31	14.78
Nov 26 11 10:00am	0.78	-5.06
Nov 26 11 11:00am	3.68	-4.19
Sharpe Ratio	0.02	0.89
Skewness	-3.05	-0.65

Table XI

Results for two experimental sessions where pool growth changed randomly across replications between positive (+10%), zero, and negative (-10%) growth. Shown are estimates of regressions of (i) pool duration (number of periods until liquidation) and (ii) percentage claims submitted for cash-in in period 1, onto a growth dummy (+1 if pool grew by 10%, 0 if no growth, -1 if pool shrank by 10% across periods), as well as replication dummies to control for secular changes in pool duration and period-1 cash-in requests. t statistics in parentheses.

Ind Var	Pool Growth	Replication Dummies 1	2	3	4	5	Intercept (6th Rep)	R^2	Number Obs
Duration	0.750	2.25	1.00	0.88	0.63	0	1.88	0.96	12
	(3.87)	(5.20)	(2.58)	(1.81)	(1.57)	(0)	(6.46)		
Period-1	-0.27	-0.38	-0.46	-0.24	-0.26	-0.04	0.69	0.95	12
Cash-Ins	(-4.81)	(-3.05)	(-4.06)	(-1.68)	(-2.22)	(-0.38)	(8.22)		