That’s how we roll: an experiment on rollover risk

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Abstract

There is consensus that the recent financial crisis revolved around a crash of the short-term credit market. Yet there is no agreement around the necessary policies to prevent another credit freeze. In this experiment we test the effects that contract length has on the market-wide supply of short-term credit. Our main result is that, while credit markets with shorter maturities are less prone to freezes, the optimal policy should be state-dependent, favoring long contracts when the economy is in good shape, and allowing for short-term contracts when the economy is in a recession. We also report runs on firms with strong fundamentals, something that cannot be observed in the canonical static models of financial panics. Finally, we show that our experimental design produces rich learning dynamics, with a text-book bubble and crash pattern in the short-term credit market.

JEL Codes: C92, C91, G01, GO2, G21, G21

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1 Introduction

While the literature agrees on placing a run on short-term credit at the center of the recent financial crisis (Brunnermeier (2009), Krishnamurthy (2010)), there is much less consensus on how to prevent another panic. Brunnermeier et al. (2009) suggest that extending the maturity of short-term credits might help stabilize the market by making it less volatile. We use experimental tools to test the effects that this policy suggestion has on the market for short-term credit. To be more precise, we use experimental tools to investigate the effects of different maturity lengths on a market for Asset Backed Commercial Paper (ABCP), which is a specific type of short-term credit in which, if the issuing firm does not fulfill its promises, the holder of the ABCP can seize the posted collateral. Our results show that while on average markets with shorter credit maturities have a lower probability of freezing, a more detailed analysis of our data indicates that the optimal policy should be state-dependent, favoring long contracts when the economy is in good shape, and allowing for short ones during a recession. Our data also shows a significant number of firms with strong fundamentals being “locked out” of the credit market, as well as rich experimental dynamics with a consistent credit bubble and crash across sessions.

1.1 Why run an experiment on ABCP?

ABCP has been pointed as the necessary transmitter of the housing bubble into the financial system (Brunnermeier (2009)), so, while short-term credit is not a problem per se, ABCP played a central role in the financial meltdown and credit freeze of 2007. The argument is that ABCP (usually supported by structured subprime mortgages) took over the more “traditional” credit market in the years before the crisis, and by virtue of being cheap and unregulated, it exposed the market to a credit bubble, and to “excessive mismatch in asset-liability maturities”. In Figure 1 (borrowed from Brunnermeier (2009)) we see how the market for ABCP almost doubles in size from 2005 to 2007 (the final years of the housing bubble), to crash and drop from an outstanding $12,000 billion to $750 billion in just six months in 2007. Yet, what is most interesting about this graph is not the increase in ABCP, but rather how unsecured instruments were only slightly affected at the peak of the 2007 crisis. This suggests that the problem revolved more around a change in the perceived value of collaterals than around the use of short-term credit. In fact, even previous to this crash of the market, a whole literature had already developed around the idea of regulating collaterals (e.g., Geanakoplos (2009, 1996)).

In a more specific analysis, Shin (2008) looks at the particular case of Northern Rock, and explains the shift in paradigm that modern financial markets, have brought to our understanding of “bank runs”. As he puts it, while we all remember the lines forming at the doors of Northern Rock, the real storm had
occurred weeks before, when non-depository creditors (mostly of ABCP) decided not to roll over their credits to the bank\footnote{As Shin (2008) puts it “The depositor run, although dramatic, was an event in the aftermath of the liquidity crisis at Northern Rock”}. The important question, according to the author is thus “not so much why banks depositors are so prone to running, but instead why the plentiful short-term funding (…) suddenly dried up”.

Finally, it looks like the Federal Reserve agrees with the diagnosis, and has pointed at credit markets as the main reason for the recent crisis (Bernanke (2009a), (2009b), (2008)), making it modify its policy to a “credit-easing strategy rather than a quantitative-easing approach” (Bernanke (2009b)). In fact, in a 2008 speech\footnote{Given at the Federal Reserve Bank of Atlanta Financial Markets Conference on May 13 2008.}, Bernanke expressed this shift in the way the Federal Reserve would approach financial panics:
“Bagehot defined a financial crisis largely in terms of a banking panic—that is, a situation in which depositors rapidly and simultaneously attempt to withdraw funds from their bank accounts. In the 19th century, such panics were a lethal threat for banks that were financing long-term loans with demand deposits that could be called at any time. In modern financial systems, the combination of effective banking supervision and deposit insurance has substantially reduced the threat of retail deposit runs. Nonetheless, recent events demonstrate that liquidity risks are always present for institutions—banks and nonbanks alike—that finance illiquid assets with short-term liabilities.” (Emphasis added)

2 Our experiment in the context of the experimental literature

No experimental literature exists on the topic we are covering, so we use as references two strands of experimental research which are relatively close to our experimental design. The first one corresponds to continuous-time experiments, the second to “timing experiments”, with a special emphasis on the experimental bank-runs literature. Continuous time experiments started years ago, with Friedman and Cheung (2009) and Morgan and Brunnermeier (2010) (whose working papers appeared around 2003/04), but it has not been until recently that this experimental technique has taken off with Oprea et al. (2009) and Anderson et al. (2010) looking into strategic investment decisions, Oprea et al. (2011) studying the evolutionary equilibrium of the hawk and dove game, Friedman and Oprea (2012) experimenting with the effects of response delay in a repeated prisoners dilemma game, and Rabanal (2012) looking at mortgage default timing. While none of these papers directly address any of the questions of our paper, they are a good reference for the methodological design of our experiment.

The other relevant strand of literature is on experimental bank runs. To our knowledge, the first paper on this topic is Madies (2006), which is based on the theoretical model of Diamond and Dybvig (1983; DD). The results show that deposit insurance cannot avoid bank runs, and that the more experienced subjects are, the more often runs are observed. Garrat and Keister (2009), also test the DD setup but turn it into a repeated game by giving subjects the opportunity to exit several times per round. Schotter and Yorulmazer (2009) will also adopt this technique. Both papers find that not only more experienced subjects are more prone to runs, but that the more opportunities to run within each round, the more likely runs are. Surprisingly, Garrat and Keister (2009) report having to exogenously force some subjects to exit, else no panics would occur. More recently Arifovic et al. (2011) look at how bank runs can be understood as a pure coordination problem. Finally, (Klos and Strater (2012)) approach bank runs from a Global Games perspective.
In summary, while there exists some experimental literature studying banking panics, most of it is based on models of “classic” bank runs, and none addresses the intricacies of modern financial markets, and the freezing of short-credit markets.

3 Theoretical Benchmark

Our experiment is inspired on the continuous time model by He and Xiong (2012; HX). In it, a firm finances its long-term investment by issuing short-term credit to a continuum of creditors, which without loss of generality we can assume to be of $1. The value of the firm\footnote{We will assume that the firm’s only investment is on the long-term asset. Therefore, the value of long-term asset is the total value of the firm.} follows a geometric Brownian motion and is perfectly observable by all agents. The Brownian motion can be written as:

$$\frac{dy_t}{y_t} = \mu dt + \sigma dZ_t$$ (1)

Where $y_t$ is the value of the firm, $\mu$ is the drift, $\sigma$ the volatility, and $Z_t$ the standard Brownian motion.

Each creditor’s debt matures with the arrival of an independent Poisson shock of intensity $\kappa > 0$, creating a uniform distribution of the maturities\footnote{Most firms spread out their maturities to avoid having large liquidity needs on any one specific date.}, with all contracts having an expected duration of $1/\kappa$ at any point in time. This random maturities system is a simplifying assumption akin to Calvo pricing (Calvo (1983)), and avoids agents having to keep track of all other maturities when making the rollover decision, while still capturing all of the first order effects of other maturing contracts.

If within the time interval $[t, t+dt]$ enough creditors decide not to rollover their credit, then the firm draws from its cash reserves\footnote{He and Xiong (2012) describe $\vartheta$ as unreliable credit lines that the firm may tap, which is why the extra time is a function of the contract length. We believe that describing $\vartheta$ as cash reserves is more intuitive for our experimental purposes.}($\vartheta$) and survives, on average, an extra $1/\vartheta \kappa$. Once the firm runs out of reserves it goes bankrupt and liquidates its assets at a discount value $\alpha < 1$, so the value of the asset is $\alpha F(y_t)$, where $F(y_t)$ is the present discounted value of the firm.

As payoffs, agents receive a stream of interests $r$ until $\tau = \min(t_m, t_b, t_d)$ which is the earliest of three possible events. The first event ($t_m$) is the maturing of the long-term investment of the firm, in which case the agent gets back $\min(1, y_{tm})$ and the firm ceases to exist. That is, the firm pays back the full principal of the credit if it can, or whatever it can pay back (but never more than the original $1$ credit). The second possibility ($t_b$) is a bankruptcy of the firm, in which case the creditor gets back $\min[1, \alpha F(y_t)]$. Finally, the short-term credit can mature ($t_d$), at which point the creditor will decide to rollover his credit if the...
continuation value $V(y_{td}; y^*)$ is higher than getting his credit back ($1$), where $y_{td}$ is the value of the firm at the maturity point $t_d$, and $y^*$ is the stopping threshold of other agents. The continuation value is thus written as:

$$V(y_{td}; y^*) = \mathbb{E}_t \left\{ \int_{t}^{\tau} e^{(-\rho(s-t))} r ds + e^{(-\rho(\tau-t))} \left[ \min(1, y_t) 1_{\tau=t_m} + \right. \right.$$ 

$$\left. \min(1, \alpha F(y_t)) 1_{\tau=t_m} + \max_{\text{rollover or run}} \left( 1, V(y_{td}; y^*) \right) 1_{\tau=t_d} \right\}$$

(2)

In equation (2) $\rho$ is the discount value of the agent, and $1_{\{.\}}$ is an indicator function which takes value $1$ whenever the statement in brackets is true, zero otherwise.

By evaluating the change in value of the continuation value (2) over a small time interval $[t, t+dt]$ the authors can write the Hamilton-Jacobi-Bellman:

$$\rho V(y_t; y^*) = \mu y_t V_y + \frac{\sigma^2}{2} y_t^2 V_{yy} + r + \phi \left[ \min(1, y_t) - V(y_t; y^*) \right] +$$

$$\kappa \delta 1_{\{y_t < y^*\}} \left[ \min(1, \alpha F(y_t)) - V(y_t; y^*) \right] + \delta \max_{\text{rollover or run}} \{ 0, 1 - V(y_t; y^*) \}$$

(3)

The left hand side represents the required return to the creditor, the first two terms in the right hand-side evaluate the fluctuation in the value of the firm. The equation also contains the continuation values of each of the three outcomes (long-term maturity, bankruptcy, short-term maturity) weighted by the probability of each one.

Finally, from equation (3) the authors show that agents will rollover the credit if and only if $V(y_t; y^*) > 1$, that is, if the continuation value is greater than getting back the principal of the credit and “walking away”. This result takes them to a unique symmetric equilibrium which is determined by the condition $V(y^*; y^*) = 1$, where no subject rolls over the short-term credit for a firm whose value is below $y^*$, and always does so for values above $y^*$.

What should be understood from this model is that unlike global games, subjects do not get a noisy signal, but a precise one. The strategic uncertainty comes from the asynchronous structure of the maturities, and the frequent change in value of the firm. It is precisely from these two key elements that agents can coordinate on a unique equilibrium, and this is why we can have results that would never happen in classic static models.
4 Experimental Implementation

4.1 Basic Design

Our experiment considers groups of 4 subjects where each member of the group provides a $1 short-term credit to a firm which has made a time varying long-term investment. Each group is composed of 4 subjects (which is a number close to the 5 individuals in the groups of Garrat and Keister (2009)), and the composition of the groups remains invariable during all of the 60 rounds that a session lasts. All subjects are informed of the size of their group and of its unchanged composition during the session.

The time unit of our experiment are ticks. Following Anderson et al. (2009), each tick will be 1/5 of a second (i.e., 200 milliseconds). Each of the 60 rounds has a random end which is governed by a Poisson process, and has an expected length of 150 ticks (30 seconds), at which point the long-term investment matures and the firm ceases to exist. In HX, the value of the long-term investment \( y_t \) follows a geometric Brownian motion. Given the discrete nature of computer internal clocks, we will need to discretize this Brownian motion, and to do so we will use the procedure described in Anderson et al. (2009).

In each of the 60 rounds we will ask subjects to make one and only one decision, namely, whether or not to continue rolling over their credit to the firm. If at any time 2 subjects decide not to rollover their credit, then the firm will continue to run for a fixed \( \vartheta \) of ticks before it goes bankrupt and has to liquidate its assets at a fire-sale value. This “extra time” \( \vartheta \) is a linear function of the duration of short-term contracts and can be interpreted as the cash reserves of the firm\(^6\). The decision to choose 2 out of 4 subjects as the threshold for bankruptcy is again inspired by Garrat and Keister (2009), where the bank goes bankrupt if 3 out of 5 subjects decide to run.

The payoffs for each round will depend on the value of the firm at time \( t \), \( y_t \), and the decisions made by each subject in the group. To be precise, each round’s individual payoffs will accrue from two different sources:

1. Flow payoff: For each tick that a subject keeps his investment in the firm she receives $0.004 (i.e., $0.6 for every 30 seconds invested).

2. End of round status: Depending on the decisions of the particular subject and the decisions of the other members of the group, the round could end in three different ways.

   (a) Exit: if a subject exits the project at time \( t_e \), then she gets back her initial investment of $1, independently of the value \( y(t_e) \) of the project at that point.

\(^6\)This parameter comes directly from HX. In a future experiment we want to test the effects of changing \( \vartheta \).
(b) **Bankruptcy**: if at time $t_b$ two subjects have stopped rolling over their credit, then the firm will run on its cash reserves for $\theta$ ticks, until finally going bankrupt at $t_{b+\theta}$, and being forced to sell its assets in the secondary market at a value $\alpha(F(y_{t_{b+\theta}}))$, where $F(.)$ is the present discounted value of the firm and $0 < \alpha < 1$. At this point the firm will pay all subjects still invested $Min[1, \alpha F(y_{t_{b+\theta}})]$ and then will cease to exist.

(c) **Natural Ending**: If the firm reaches its random “natural” ending $t_n$ without going bankrupt, then all subjects still invested in the firm get $Min[1, y_{t_n}]$.

Subjects can keep track of both the firm’s value (green jagged line in Figure 2), and of the fire-sale value (golden jagged line in Figure 2) in the graphical interface on their screen. Other useful information appearing on the screen are the values at which subjects in the group decided to stop rolling over their credit in the previous 15 rounds (upper right box in Figure 2), the $\$1$ threshold under which payoffs would be <$\$1$ (horizontal red line in Figure 2), and the moment they had exited, if they had decided to do so (vertical green line in Figure 2).

### 4.2 Credit rollover and credit maturities

The maturities system of this experiment is one of its unique aspects when compared to the experimental bank run literature, since in our game subjects decisions are not simultaneous. This creates several
problems. The first is how to keep the game flowing (especially in the short maturity treatment) when subjects decide whether to rollover or not at every maturity. Our solution consists in having the credits rolled over by default, unless a subject decides otherwise.

To stop this automatic rollover, a subject will have to “connect” three numbered buttons on the screen by hovering over them in a precise sequence. We borrow this idea from Brunnermeier and Morgan (2010), where the purpose of the hovering mechanism is to avoid subjects making inferences from any clicks they can hear coming from other terminals. Our version is a slight variation over their mechanism: Not only do subjects have to hover, but they have to do it following a certain gradient, thus making it difficult to accidentally stop rolling the credit by inadvertently hovering over the designated area.

Subjects can decide to stop rolling over their credit at any time during the experiment. If they decide not to roll over, this decision will be implemented at the next maturity point. In fact, the second problem we confronted with our staggered maturity system was how to avoid turning the maturity points into focal points. The reason is that we are interested in observing at what value of the collateral agents decide to stop rolling over their credit, and we do not want this decision to be contaminated by the distance to the next maturity point. To avoid this confounding effect, we hide the maturity points from our subjects.

To hide the maturity points, we fix the length of credits to be $\delta$ ticks and have the computer randomly assign in each round $j$, and for each subject $i$, a random starting point $t_{ij}$ within the first $\delta$ ticks. From this initial (individual) point, maturities will happen every $\delta$ ticks. So, for example, for subject $i$ in round
his first maturity point will be at $t_{1ij} \in [0, \delta]$, his second maturity point $t_{2ij}$ at $t_{2ij} = t_{1ij} + \delta$, the third maturity $t_{3ij}$ at $t_{3ij} = t_{2ij} + \delta$, etc. (see Figure 3). As a result, at every point in time the expected maturity of every subject is $\delta/2$ ticks away, avoiding a focal point problem.

Finally, we borrow the idea of the “pseudo-strategy method” from Anderson et al. (2010) and let all rounds play until their random ending, without providing any information to subjects of what other members of their group are doing. Once the round ends, all subjects get a screen shot describing all the events in the round, including other subjects (and own) requests to exit (green vertical lines in Figure 4), other subjects (and own) actual exit (orange vertical line in Figure 4), as well as round length and final payoffs, and a bankruptcy point (if there was one) shown as a red vertical line.

The implementation of this “pseudo-strategy” method will allow us to collect more information at every round, and avoid the problem of censored values that would occur if the rounds did not run until their random natural ending. For a review of the strategy method, see Brandts and Charness (2011).

4.3 Parameters and Hypotheses

As mentioned above, the goal of this experiment is to test the effects that maturity lengths have on the market for short-term credit. We implement two treatments:
### Table 1: Parameter Values

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Long Contract</th>
<th>Short Contract</th>
<th>Comment</th>
</tr>
</thead>
<tbody>
<tr>
<td>( \delta )</td>
<td>40 ticks</td>
<td>10 ticks</td>
<td>Contract Length</td>
</tr>
<tr>
<td>( \theta )</td>
<td>15 ticks</td>
<td>3 ticks</td>
<td>Cash reserves</td>
</tr>
<tr>
<td>( \mu )</td>
<td>0.0024</td>
<td>0.0024</td>
<td>Drift of the Geometric Brownian Motion</td>
</tr>
<tr>
<td>( r )</td>
<td>$0.004\text{ per tick}$</td>
<td>$0.004\text{ per tick}$</td>
<td>Per-tick flow payoff</td>
</tr>
<tr>
<td>( \sigma^2 )</td>
<td>1.1</td>
<td>1.1</td>
<td>Volatility</td>
</tr>
</tbody>
</table>

- **Long treatment**: Each contract is 8 seconds long (i.e., \( \delta = 40 \) ticks), and cash reserves last for 15 extra ticks after 2 subjects exit the market.

- **Short treatment**: Each contract is 2 seconds long (i.e., \( \delta = 10 \) ticks), and cash reserves last for 3 extra ticks after 2 subjects exit the market.

Plugging these parameters into the He and Xiong (2012) model, it predicts that the optimal stopping threshold in the Short treatment will be higher than in the Long treatment. Therefore, we make the following prediction:

- **Prediction 1**: Subjects will stop rolling over their credit at higher values of the firm in the Short treatment than in the Long treatment.

Our second prediction is that we will see subjects stopping their rollover at values of the firm where, in case of a fire-sale, creditors would get back all of their investment (i.e., \( \text{Min}[1, \alpha F(y(t_{(b+d)}))] \geq 1 \)). This prediction also comes from He and Xiong (2012).

- **Prediction 2**: Credit freezes will happen even at values where a fire-sale would pay back the whole investment to all creditors.

## 5 Experimental Results

All session were run at the LEEPS lab of the University of California Santa Cruz, and all subjects were undergraduates from this institution. In total 72 subjects participated in the experiment, spread into 7 different sessions, and none played the game twice. In each session we had either 12 or 8 subjects for a total of 4,320 decisions (60 rounds × 72 subjects).\(^7\) From these observations we will ignore all stopping

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\(^7\)Each session begins with some practice rounds whose results are not used in the analysis.
decisions for a value above $4, a value for which it is impossible to lose money in the Short treatment, and for which the probability of losing money in the Long treatment is <0.1%. In total we end up with 4115 observations. In Figure 5 we plot the CDF for both treatments along with the theoretical stopping threshold from He and Xiong (2012) (vertical dotted lines) where a horizontal orange line indicates the median value of the distribution. As we can see in Figure 5, the observed stopping values in the Long treatment are much lower than those in the Short treatment (Kolmogorov-Smirnov $p$-value = 0.000), which is in line with Prediction 1.

![Figure 5: CDF plots for both treatments](image)

But Figure 5 is a CDF of the observed stopping values across all rounds in all sessions. To have a better description of the evolution of the stopping values during a session, we break the experiment into quarters (15 periods each quarter). As it is apparent from Figure 6 the stopping values across the two treatments continue to be are widely separated in the fourth quarter (Kolmogorov-Smirnov corrected $p$-value = 0.008).

Also apparent from Figure 6 is that the CDF’s of the stopping values for both treatments are significantly shifted to the right. If we compare the CDF’s for the last quarter to the rest of the session, we get significant differences (Kolmogorov-Smirnov $p$-value = 0.029 in the Short treatment, and $p$-value = 0.001 in the Long treatment). It appears that as the sessions evolve, subjects try to undercut each other by stopping at higher values of the firm.

To check the effects of the quarters on the stopping values we run a simple linear regression, which
Our dependent variable is the stopping value (Stopvalue). “1.short” denotes a dummy variable for short maturity treatments and the other variables are quarter dummies.

While the results of Table 2 confirm Prediction 1, namely, that Short contracts have significantly higher stopping values than Long ones, the surprising result is that the second and third quarters dummies are significant and negative. This means that for those quarters, the stopping values actually went down below the first quarter levels, while the dummy for the fourth quarter does not seem to be significantly different from the first quarter in the complete model (Model 2). A Kolmogorov-Smirnov comparing first and fourth quarter in the Short treatment confirms this $p$-value = 0.34, and $p$-value = 0.25 in the Long treatment.

When we plot the kernel density estimates for each quarter in both treatments (Figure 7) to have a visual idea of the distribution of the stopping values for both treatments, we see the surprising pattern of a textbook boom and crash of the credit market. In both treatments (although clearer in the Long treatment) the first quarter is similar to a left skewed plateau, as stopping decisions are spread out across the whole strategy space with somewhat more incidence on the left tail. This suggests that there is a sort of “tâtonnement” learning process during which subjects presumably try to get better acquainted with both, the mechanics of the game and the strategy of other members of the group. It is only by the second quarter
Table 2: Regression of stopping values and treatment

<table>
<thead>
<tr>
<th></th>
<th>(1) Stopvalue</th>
<th>(2) Stopvalue</th>
</tr>
</thead>
<tbody>
<tr>
<td>[1em] 1.short</td>
<td>0.318** (0.0756)</td>
<td>0.932*** (0.109)</td>
</tr>
<tr>
<td>2.quarter</td>
<td>-0.253*** (0.00388)</td>
<td>-0.205*** (0.0564)</td>
</tr>
<tr>
<td>3.quarter</td>
<td>-0.202*** (0.00814)</td>
<td>-0.126** (0.0587)</td>
</tr>
<tr>
<td>4.quarter</td>
<td>0.000974 (0.00491)</td>
<td>-0.0457 (0.0633)</td>
</tr>
<tr>
<td>_cons</td>
<td>2.062*** (0.0527)</td>
<td>1.766*** (0.0825)</td>
</tr>
</tbody>
</table>

N 1214 1214
adj. $R^2$ 0.045 0.182
Group dummies No Yes

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

when subjects seem to realize that more money can be made from stopping at lower values, with the result that they start taking higher risks.

These risks are clearly seen in the lower half of Figure 7 where we plot the density estimates of the fire-sale values at the subjects stopping points. Looking at Quarter 2, the estimations show that a large number of subjects are stopping at values where the fire-sale value of the firm is well below the $1 break-even threshold. It is no surprise that this excessive risk-taking in the second quarter ends up in bankruptcies and, therefore, in losses for the subjects still invested, sparking, by the third quarter, a “full blown” panic that settles at stopping values above $2 in Quarter 4. (Appendix A has kernel densities broken into sixths of the session for a more precise description of the timing).

Table 3: Mean and SD of observed stopping values for each quarter by treatment

<table>
<thead>
<tr>
<th>Quarter</th>
<th>Mean Long</th>
<th>Mean Short</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1.92±0.898</td>
<td>2.26±0.901</td>
</tr>
<tr>
<td>2</td>
<td>1.35±0.887</td>
<td>2.11±0.924</td>
</tr>
<tr>
<td>3</td>
<td>1.48±0.839</td>
<td>2.23±0.893</td>
</tr>
<tr>
<td>4</td>
<td>2.02±0.774</td>
<td>2.35±0.851</td>
</tr>
</tbody>
</table>
In Table 3 we report the mean stopping values for each quarter and treatment. As we can see, this table confirms the bubble and crash story, and suggests why we didn’t see a significant difference across the first and fourth quarter (Table 2): The rebound effect leaves the final stopping threshold very close to the starting average.\footnote{A Mann-Whitney test confirms that there is no significant difference between the first and last quarter, yet second and fourth are significantly different in both treatments.}

Given the parabolic evolution of the stopping values, we call these dynamics the “boomerang effect”. Therefore, we can state:

**Result 1**: The dynamics of stopping values do not move in one direction, but rather have a “boomerang effect”, resulting in a bubble followed by the crash of the credit market.

While beyond of the scope of this paper, the analysis of these rich dynamics could be a starting point for future research in market dynamics, especially in the detection of bubbles and financial panics. Yet, what is clear from both the kernel densities and in the empirical CDF’s is that a significant number of subjects are stopping their credit at values that are above the fire-sale break-even point $\alpha F(y_t) = 1$. Therefore:
Result 2: Overall, 60% of the decisions to stop rolling over the credit are made even when firms have strong fundamentals, and can pay back the entire investment to all subjects even in the case of a fire-sale (i.e., Frantic runs occur).

That frantic runs are common is particularly interesting when recalling that the unexpected freeze of securitized credit was one of the main reasons why the 2007 crisis was so damaging. Because frantic runs were so unexpected, firms had no contingency plan for a sudden freeze of the short-term credit market, making many firms unable to meet their liquidity needs.

The estimates above are useful as a first approximation to the qualitative results of our experiment on the short-term credit market. But, so far, we have not used all the data gathered in the experiment, since we have ignored the information contained in the decisions not to stop rolling over the credit. By not taking into account these non-stopping decisions our mean estimates are necessarily biased upwards.

We can overcome this bias by using the product limit estimator (Kaplan Meier (1958)), which corrects the bias in the estimations and, most importantly, allows the estimation of the survival curve for each treatment and lets us study the estimated stopping behavior of all the subjects across the whole range of firm values.

5.1 Hazard Rates and the Product Limit Estimator:

For any kind of policy analysis (and especially when dealing with financial panics) the state of the economy is a crucial factor. In our experiment we can take the value of collateral as a proxy for the state of the economy, so that the lower the value of the firm, the worse the economy is and vice-versa. To assess the behavior of our subjects for any state of the economy we will turn to estimating the hazard functions governing our experiment’s dynamics, and to do this we will need to use the product limit estimator.

The product limit (PL) estimator is a non-parametric Maximum Likelihood Estimator of the distribution, which is adapted to dealing with censored data as in our case. As mentioned, the previous analysis ignored the decisions not to run. The PL estimator takes these non-stopping decisions as “censored” observations in the sense that we do not observe a subject stop rolling over his credit, because the value of the firm never got to his stopping threshold. For example, for a given threshold of subject i in round j, \( t_{ij} \), we only observe his stopping decision if the value of the firm for that round \( y_j \) reaches his specific threshold (i.e. \( \text{Min}[y_j] \leq t_{ij} \)). If this condition is never met, then we are left with a censored observation.

But while it is usually straightforward to use the PL estimator, in our dataset we have left-censored data, which is not standard. To deal with it, we need to “flip” the data and work with their mirror image. To
flip the data, we have to find a constant, $S$, large enough such that $S > \max_{i,j}$ for all subjects $i$ and rounds $j$. As explained with some detail in Appendix C, we decided to subtract all values from 4.

In Figure 9 we present the hazard and cumulative hazard estimates for the flipped data of each treatment. The hazard function can be understood as the “probability[10]” that a subject that has not stopped rolling her credit will do so within an infinitesimally small range of firm values. To be more precise, define the instantaneous hazard rate as a measure of the probability that a subject will decide to stop rolling over the credit within the (limiting) interval $\Delta y$, given that he has not yet stopped rolling over his credit:

$$h(y) = \lim_{\Delta y \to 0} \frac{e[y, y + \Delta y] / N(y)}{\Delta y} \quad (4)$$

Where $e[y, y + \Delta y]$ is the number of observed rollover stops in the interval $[y, y + \Delta y]$, and $N(y)$ is the number of subjects at risk for value $y$. It is clear that if we do not take into account the censored observations, then $N(y)$ will be higher, bringing down the real hazard rate for the value of the firm $y$, and consequently biasing our results.

The Nelson-Aalen cumulative hazard rate is a non-parametric estimation of the cumulative hazard. Notice how the cumulative hazard can go above the value of 1, which means that for that value of the firm, if it occurred, a subject would have stopped rolling the credit more than once.

In Figure 9 we present both the estimated hazard function and the Nelson-Aalen estimates for the cumulative hazard of both treatments. What is striking in these graphs is how the hazard of the Long treatment start below that of the Short treatment, but ends up way above it, after both hazard functions cross. This is extremely important as it shows that the hazards are not proportional, and thus there is an interaction between the treatment hazard ratios and the state of the economy. What we see is that when the economy is in good shape (the value of the firm if high), subjects are more willing to rollover the credit under long contracts. Yet, when the value of the collateral is close to “breaking the buck”, then the hazard rate of the Long market explodes, overtaking that of the Short market. This suggests that the optimal approach to short-term credit regulation should be a dynamic policy that changes with the state of the economy.

A Fleming-Harrington[11] test with all the weight put on the left tail ($p$-value = 0.045) confirms that the differences across treatments are significant[12]. Therefore, even when the censored information is included in our analysis we see a highly significant difference between both treatments. Furthermore we see that

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[10] It actually is the ratio between the probability density function of the event (running), and the survivor function.

[11] Precisely because the hazard estimates cross, our dataset violates the proportional hazards assumption, and the Fleming-Harrington test is the most appropriate.

[12] A Fleming-Harrington test putting equal weight in both tails, and another test with all weight on the right tail will also show highly significant differences ($p$-value = 0.000 in both cases).
the hazards are not proportional, but rather that their ratio is state-dependent. This fits with the bimodal shape of the densities that we saw in Figure 7, where the Short treatment had a peak of stopping decisions for high values of the firm.

A Tobit model estimation confirms the Fleming-Harrington tests by showing a highly significant treatment dummy (Short), and still showing the “boomerang effect” across quarters (2.Quarter, 3.Quarter, 4.Quarter), as we can see in Table 4.

Unfortunately, to have precise quantitative estimates of the PL estimator we need to work with a subset of our data. This is due to the heavy censoring which will bias the PL in the direction of the censored data points (Moeschberger and Klein (1985, 2003), Miller (1983)). Moreover, if there are censored points beyond the last precise observation (i.e., the last observed stopping value), then the bias will be even stronger (Moeschberger and Klein (1985)). With almost 70% of our data censored, and with some of these censored values to the right of the highest observed stopping value, we will need to find a rule to reduce the censoring in our sample.

To avoid the bias described in (Moeschberger and Klein (1985)) we drop those observations that are

\[\text{Remember that we have flipped the data.}\]
Table 4: Tobit estimation of the stopping values

<table>
<thead>
<tr>
<th></th>
<th>Stopvalue</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
</tr>
<tr>
<td>Short</td>
<td>0.726***</td>
</tr>
<tr>
<td></td>
<td>(0.0209)</td>
</tr>
<tr>
<td>2.Quarter</td>
<td>-0.197**</td>
</tr>
<tr>
<td></td>
<td>(0.0832)</td>
</tr>
<tr>
<td>3.Quarter</td>
<td>-0.178*</td>
</tr>
<tr>
<td></td>
<td>(0.105)</td>
</tr>
<tr>
<td>4.Quarter</td>
<td>-0.248*</td>
</tr>
<tr>
<td></td>
<td>(0.134)</td>
</tr>
<tr>
<td>cons</td>
<td>0.0670</td>
</tr>
<tr>
<td></td>
<td>(0.108)</td>
</tr>
<tr>
<td>N</td>
<td>3997</td>
</tr>
<tr>
<td>adj. $R^2$</td>
<td></td>
</tr>
<tr>
<td>Group dummies</td>
<td>Yes</td>
</tr>
</tbody>
</table>

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

censored beyond the latest observed stopping value (in total 38 observations). Then, following Anderson et al. (2009), we eliminate all observations in those sessions where the minimum value of the firm was not low enough. In our case this means all observations for rounds where the minimum value was (strictly) greater than $0.9$ (in total 3020 observations). Finally we also need to drop those subjects who run only 2 or fewer times in the whole session (155 observations). So in total we are left with a subsample of 902 observations, where 448 are stopping decisions and 454 are censored observations (almost a 50% ratio of censored data).

Running the PL estimate on this new subsample we still have a significant effect of the treatment variable (Fleming-Harrington with all the weight on the left tail has $p$-value = 0.011), and the hazard curves continue to cross in the same way as with the full sample. Therefore, while the bias produces quantitative differences across the full sample and the sub-sample, it does not have any effect on our qualitative results.
<table>
<thead>
<tr>
<th>Quarter</th>
<th>Mean Long</th>
<th>Mean Short</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>0.84±0.04</td>
<td>0.74±0.06</td>
</tr>
<tr>
<td>2</td>
<td>0.92±0.04</td>
<td>0.90±0.05</td>
</tr>
<tr>
<td>3</td>
<td>1.01±0.07</td>
<td>1.05±0.08</td>
</tr>
<tr>
<td>4</td>
<td>1.15±0.09</td>
<td>0.90±0.12</td>
</tr>
</tbody>
</table>

**Result 3:** Both the subsample and the full sample present crossing hazard curves, with high hazard rates in the Short treatment for high values of the firm, but even higher hazard rates for the Long treatment when the value of the firm is near the break-even point.

Finally, we use the PL estimate to calculate the mean stopping value for each quarter. In both the full sample and the subsample the Short treatment has mean stopping values that are (in general) lower than in the Long treatment. Table 5 presents the results of the subsample estimates with their bootstrapped standard errors.

Therefore, once censored observations are added, our analysis reveals two things: First, that on average the Short treatment has lower stopping values than the Long treatment. Second, that while the Short treatment might have lower stopping values, its hazard function is higher for high values of the collateral. That is, when the value of the firm is high, a significantly lower number of subjects decide to stop rolling over their credit in the Long treatment. But, once the value of the firm gets near the fire-sale break-even point, the hazard function of the Long treatment shoots upwards, making the market extremely prone to freezes.

**Result 4:** The PL estimator shows that Short contracts have a lower mean stopping value than Long contracts.

### 6 Conclusion

In the *Handbook of Experimental Economics* (Kagel and Roth, eds., 1995), Al Roth explains in his Introduction why experiments are run. He mentions three reasons: To test a theory, to find new data that potentially complement the theory, and finally to offer policy advice (“whisper in the ear of princes”). This is exactly what we try to do in this paper.

To be specific, we try to compare the effects that maturity lengths have on the functioning of ABCP markets. Building on the theoretical model of He and Xiong (2012), we compare two markets where the
only difference is the duration of ABCP maturities. Our main result is that while, on average, ABCP markets with short contracts are less prone to freezes (Result 4), the optimal policy should be state-dependent, favoring long contracts when the economy is in good shape, and allowing for short-term contracts when the economy is in a recession (Result 3). This latter result comes from estimating the survival curves of our markets, and observing the hazard functions of the Long and Short treatments cross at a value near the “break-even” point, with the Long market shooting up in risk when their value of collaterals is low.

Our second result (Result 2) is reporting, for the first time in a laboratory setting, “frantic runs”, which are runs on firms that are able to pay all their debts even in a fire-sale (i.e., runs on firms with strong fundamentals). This is an important result as it cannot be observed in the canonical static models of financial panics. As Bernanke (2008) put it, the loss of access to secured borrowing was “surprising” and firms had no contingency plans prepared to face this situation. Being able to reproduce them in the lab is a first step to better understand how they work, and how they can be prevented.

Our last result is reporting rich experimental dynamics, with a consistent bubble-and-crash pattern across our sessions (Result 1). And, while analyzing the learning dynamics behind these results is beyond the scope of this paper, we do believe that the structure of our experiment can be an interesting starting point to analyze sophisticated learning in any kind of dynamic markets.

Finally, we hope this is the first of a series of papers on financial panic experiments, and that a buildup of similar papers might allow economists to whisper, with more confidence, in the ear of financial princes.
Appendix A:

Figure 9: Density estimates for both treatments

If we break up the sessions into sixth’s (1/6) of a session, we have a more cluttered graph, but a more precise description of the dynamics of the experiment. As we can see, the underlying dynamics are the same, with the “boomerang effect” taking place. The difference now is that we can better see the timing of events. While the first sixth is again of a plateau-like shape, we see that risk taking actually starts by the second sixth (earlier than we expected) taking place by the second sixth. On the other hand, our first guess of a panic taking place by the end of the second quarter, and across the next periods was correct, as we can see from the fourth and fifth sixth of the data.

A very interesting result of this more detailed breakdown of estimated pdf’s is that we can better appreciate how polarized are the stopping thresholds in the Short treatment. This is clear when observing that most sixths have a bimodal shape, especially in the “panic” (third) one, but also in the final ending distribution. While in the Long treatment this distribution is a sharp hill, in the Short treatment we see a more spread-out result.
Appendix B:

Flipping the data is a simple procedure where we just need to find a constant, $S$, large enough such that $S > \max y_{ij}$ for all subject $i$ and round $j$. Since all data are in the interval $[0, 4)$, $S = 4$ can be used to flip the data. Therefore, for every subject $i$ and round $j$, we can define $z_{ij}$ such that $z_{ij} = 4 - y_{ij}$. A graphical explanation of the process is found in the Appendix B figure.

Figure 10: Left to right censoring switch
Appendix C:

As the PL estimates graphed below show, our dropping of some data does not affect the underlying relationship of the hazard functions but, as predicted by the theory, it shifts the estimates in the opposite direction of the censoring.

Figure 11: KM Survival estimates for all data and subsample.
References


