### *Appendix A: Design and details of dynamic-treatment replication*

***Participants.*** Thirty-eight subjects were recruited from UCLA (19 females; mean age ± *SD* = 20.9 ± 4.1 years) and 25 subjects were recruited from Caltech (4 females; mean age ± *SD* = 21.3 ± 2.2 years).[13] Before the experiment, subjects gave informed consent to participate according to a protocol approved by Institutional Review Boards of UCLA, and the California Institute of Technology, Pasadena CA. Subjects were paid their earnings from the experiment, with the conversion rate of 200 Experimental Dollars equal to 1 US $, in addition to a $5 show-up fee.

In the following subsections, we examine the trials where selling decisions were *observed*. Hence, *Delay* herein means left-censored or uncensored *Delay*. When the trials with right-censored *Delay* were included, the qualitative aspects of the data did not change (see Supplementary Table 2, Supplementary Figures 2 and 5). Note that right-censored *Delay* is likely to underestimate actual *Delay*, and the left-censored *Delay* could overestimate the actual *Delay* which could have been negative, potentially biasing the results.[14] Therefore, we repeat all the analyses using static trials only, with a redefinition of *Delay* as the length of periods between the submitted selling time and the signal arrival time; $Delay_{uncensored}$ denotes this new definition of *Delay*. Using $Delay_{uncensored}$ yields results qualitatively identical to those obtained using the *Delay* measure (See Section C in the Supplementary Materials).

In theory, all sellers are *ex ante* identical in a sense that in equilibrium they all employ the same unique symmetric strategy. Hence in analyzing data, we do not distinguish the behavior of 1st, 2nd, and 3rd sellers (following BM, who do the same). Also, it should be noted that we discarded dynamic trials in which subjects exited too early (in the first 5 periods) since some UCLA subjects reported, during debriefing, that they accidently pressed the enter key at the start of a trial. A total of 22 such dynamic trials were hence discarded.[15] Reported *p*-values are two-sided unless noted otherwise.

[13] Seven additional subjects participated in the experiment, but were excluded because of very rapid response times (RTs < 2 secs) or selling times highly correlated with the random cursor starting points in static trials. These subjects do not behave according to optimal theory, but also do not give us much insight about alternative theories, and further, they create poor within-subject control to measure anxiety in the dynamic games.

[14] BM is aware of this censoring problem. To get around the problem, they estimated the delay measure using the Tobit procedure, assuming that the right-censored delay is normally distributed with mean $\tau$. However, this specification is likely to yield an estimate in favor of $\tau$.

[15] BM also eliminated observations where subjects sold in the first 10 periods.

*Descriptive Statistics.* First, we describe some very basic properties of the data. Due to sampling variation, the distribution of signal arrivals happened to be slightly different between the dynamic and static trials for UCLA subjects. As a result, the durations of trials are also slightly different (see Supplementary Figure 1). Later analyses control for these differences.

The success rates between dynamic ($M = 0.57$; $SD = 0.13$) and static trials ($M = 0.57$; $SD = 0.14$) were not significantly different in the two conditions or between subject pools (Paired sample t-test, $t(62) = -0.17$, $p > 0.5$; signed rank test, $z$-value $= -0.003$, $p > 0.5$). However, it is noticeable that the success rates are substantially above 50%, which is the fraction expected if subjects are playing the equilibrium delay strategy—it is the first indication that subjects are generally selling earlier than the theory predicts. The success rates for dynamic and static trials are also highly correlated within subjects ($\rho = 0.59$, $p < 4.24 \times 10^{-7}$), which indicates stable individual differences in waiting.

The next analyses look at the distribution of delays after signal arrival, before selling. There are four basic results. Delays are: Shorter than predicted; longer in the static condition; depend on signal arrival time; and grow longer with experimental experience.

*Delays are shorter than predicted.* Figure 3 shows the average of *Delay* in the two conditions, and within each subject pool. The group averages are significantly smaller than the theory prediction of $\tau^* = 21$ (Dynamic: $t(62) = -18.86$, $p < 2.30\times10^{-27}$; Static: $t(62) = -10.37$, $p < 3.55\times10^{-15}$, one-sample t-tests; Also see Supplementary Figure 2). The short average delays are substantially influenced by the left-censored observations (i.e., early pre-signal selling) which are set to zero. In static trials where all selling decisions are observed, the mean $Delay_{uncensored}$ is 19.23 (*Median* = 16.76; $SD = 10.35$), which is insignificantly different than $\tau^* = 21$ in a parametric test (one sample $t$-test, $t(62) = -1.36$, one sided $p = 0.18$), but is significantly different using a non-parametric test (one-sample signed-rank test, $z$-value $= -2.27$; $p = 0.02$).

*Static delays are longer than dynamic delays.* Figure 3 compares delays in the static and dynamic conditions. Mean *Delay* was greater in static trials than in dynamic trials (paired sample $t$-test, $t(62) = -5.32$; $p < 1.52\times10^{-6}$; signed rank test, $z$-value $= -4.40$; $p < 0.0001$). A two-way ANOVA indicated a significant difference between dynamic and static delay (the static delays are longer) for both groups and significantly shorter delays for the UCLA group (due to the difference in dynamic trials only).[16] Supplementary Figure 3a shows distributions of individual average *Delay*.

*Delays depend on signal arrival time*. In both the dynamic and static clock games, delays in selling after receiving a signal should be constant, due to the *memoryless* property of the exponential distribution underlying $t_0$. However, this property is quite counterintuitive; it means that conditional on not having received a signal, agents should behave in exactly the same way independently of the period they are in.

This predicted independence of delay from signal arrival is not evident in the data. *Delay* decreased as a function of the signal arrival time in both dynamic and static trials (Supplementary Figure 4a). Further, in each of six signal arrival time bins,[17] mean *Delay* in static trials was longer than in dynamic trials (Figure 4). This pattern was preserved when the trials with right-censored *Delay* were included (Supplementary Figure 3 and Supplementary Table 2). $Delay_{uncensored}$ also decreased as a function of the signal arrival period (Supplementary Figure 9). Tobit regression analysis of *Delay* supports the graphical conclusion (Table 1). Signal arrival time strongly affects *Delay* across various model specifications. Both of the UCLA and the Caltech groups waited longer to sell in static trials than in dynamic trials. With some other minor differences in these graphs, *Delay* in static trials was longer in the Caltech group. $Delay_{uncensored}$ also showed a similar pattern (Supplementary Figure 10 and Supplementary Table 3).

*Delays grow longer with experimental experience.* Next we report whether subjects' selling strategy was adaptive over time (see below for more detail). Comparing the first 50 and last 50 trials, *Delay*s are longer in the last 50 trials and the dynamic vs. static difference in *Delay* is smaller in the last 50 trials (Figure 4). Regression analyses of $Delay_{uncensored}$ confirm these significant learning effects in both groups (Supplementary Table 4).

**A comparison with the BM data**

[16] Significant main effects were observed for *Condition* ($F(1, 61) = 23.88$, $p < 0.001$) and for *Group* ($F(1, 61) = 6.64$, $p = 0.0124$), and their interaction was marginally significant ($F(1, 61) = 3.72$, $p = 0.0585$) according to a two-way analysis of variance with one within-subject factor, *Condition*, and one between-subject factor, *Group*.

[17] The signal arrival periods were binned into 6 bins, by 25 periods starting from 0, except for the last bin (rare signal periods over 125 periods were binned all together).

In this section, we briefly report some new analyses of the subset of BM's data (which included other interesting treatments) to compare them with our data. The unobservable treatment in their paper has an identical formal structure to the dynamic trials in our experiment, except that parameter values and some procedures are different. As a result, the (risk-neutral) equilibrium strategic delay $\tau^*$ is 23 periods in their experiment, a little longer than the equilibrium $\tau^* = 21$ in our design.

Like BM, we discard the few observations in which selling (or exit) occurs within the first 10 periods after the start of the trial. All the definitions of the variables remain the same except for *Experience*.[18] Herein only trials with uncensored and left-censored *Delay*s (trials with successful selling) were analyzed.

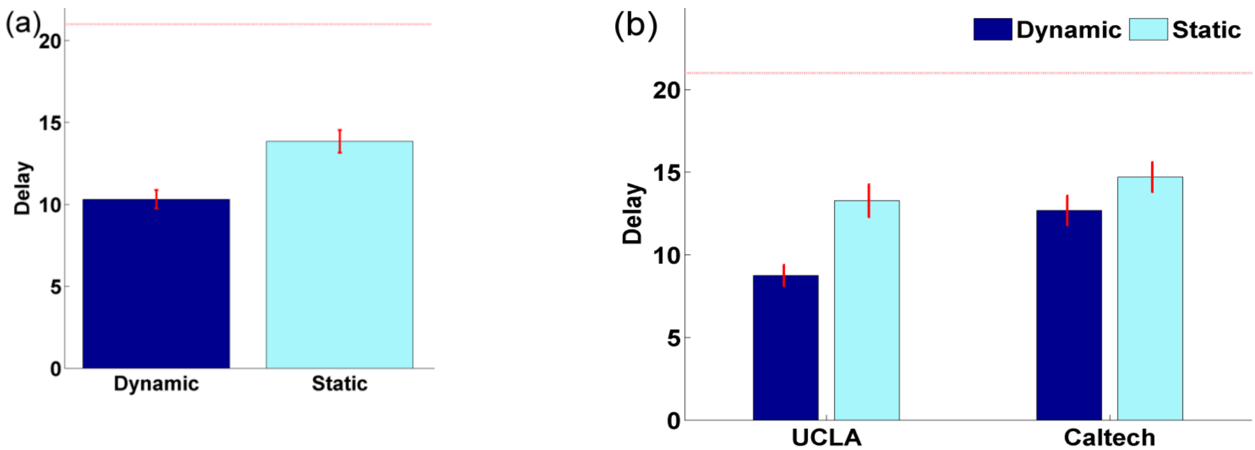
In BM's data, mean *Delay* was shorter than the prediction, $\tau^* = 23$, in all sessions (Supplementary Figure 6). Most of the subjects exhibited mean *Delay* that is significantly shorter than 23 periods (one sample $t$-test, $t(71) = -18.81$; $p < 2.74 \times 10^{-29}$; signed rank test, $z$-value = $-7.33$; $p < 2.23 \times 10^{-13}$).

*Delay* was also negatively correlated with the signal arrival time (4c; Table 1). Note that the coefficient for the signal arrival time in the Tobit delay models (−0.209 in Table 1) is very close to those in our data (−0.196 ~ –0.205; see Table 1).

We examined the effect of learning on *Delay*, which BM left to future work. Regardless of experience, the signal arrival time remained as a significant predictor of *Delay* (Figure 4c; Table 2). However, *Delay* increased in the last 25 trials compared to the first 20 trials as observed in our data (see *Delay* in the dynamic trials in Figure 5a).

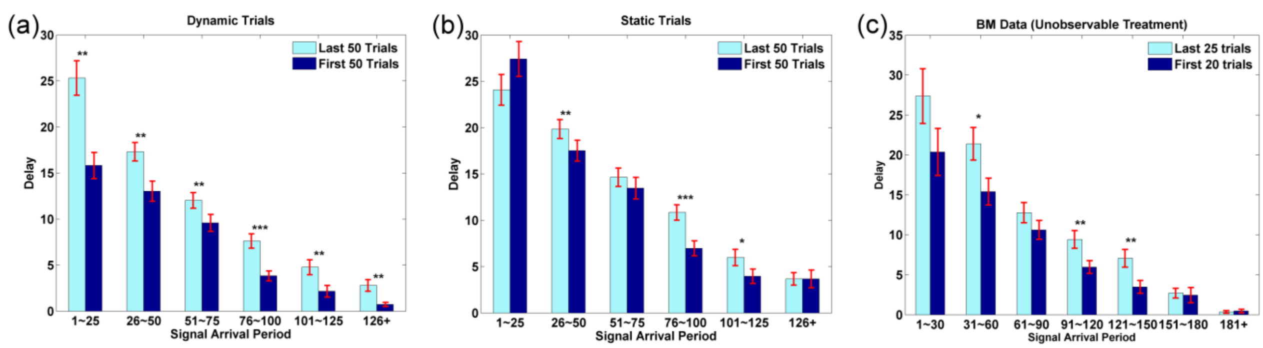
In summary, despite some differences in design and procedures BM's pioneering findings are quite closely replicated.

[18] There were only 45 trials in BM's original experiment. Hence, we defined *Experience* as 0 if a given trial was in the first 20 trials, and as 1 if it was in the last 25 trials.




*Notes.* Red horizontal lines indicate the theory-predicted delay, τ* = 21. Error bars indicate standard errors. (a) *Delay* by *Condition*. (b) *Delay* by *Group* and *Condition*.

**Figure 3 Difference in Average *Delay* between Dynamic and Static Trials: Experiment I**




Notes: (a) Dynamic trials in Experiment I. (b) Static trials in Experiment I. (c) The data from the BM experiment. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (paired sample t-test, one sided).

**Figure 4 *Delay* as a Function of the Signal Arrival Period, Moderated by Experience**

| Variable | A | B | C | D | BM |
|---|---|---|---|---|---|
| Constant | 22.720† | 22.367† | 22.318† | 22.047† | 36.667† |

| | | | | | |
|---|---|---|---|---|---|
| | (0.678) | (0.903) | (0.721) | (0.955) | (0.794) |
| *Signal* | −0.196† | −0.205† | −0.205† | −0.204† | −0.209† |
| | (0.004) | (0.007) | (0.007) | (0.009) | (0.007) |
| *Static* | 2.780† | 3.955† | 4.541† | 4.538† | |
| | (0.317) | (0.645) | (0.704) | (0.901) | |
| *Signal*Static* | | −0.019* | −0.020* | −0.019 | |
| | | (0.009) | (0.009) | (0.012) | |
| *Caltech* | | −0.026 | | 0.646 | |
| | | (1.299) | | (1.455) | |
| *Caltech*Signal* | | 0.039† | 0.041† | 0.039** | |
| | | (0.009) | (0.008) | (0.013) | |
| *Caltech*Static* | | | −1.283* | −1.293 | |
| | | | (0.621) | (1.290) | |
| *Caltech*Signal*Static* | | | | −0.001 | |
| | | | | (0.018) | |
| Log likelihood | −11411.10 | −11397.68 | −11395.55 | −11395.42 | −4399.32 |
| Wald $\chi^2$ test statistics | 1998.57† | 2018.18† | 2023.58† | 2024.40† | 847.16† |
| Left-censored at *Delay* = 0 | | | | 581 | 452 |
| Uncensored | | | | 3012 | 1055 |
| Right-censored | | | | 0 | 0 |
| Included Observations | | | | 3593 | 1507 |

Notes. *Delay* is censored at 0 (subject random-effects incorporated). Standard errors are reported in parentheses. * $p < 0.05$, ** $p < 0.01$, **†** $p < 0.001$.

**Table 1 Results of Random-Effects Tobit Regression Analyses of *Delay*: Experiment I**

| Variable | A | B | C | D | BM-1 | BM-2 |
|---|---|---|---|---|---|---|
| Constant | 21.779† | 21.314† | 21.377† | 21.311† | 34.973† | 34.342† |
| | -0.756 | (0.772) | (0.766) | (0.772) | (0.872) | (1.118) |
| *Signal* | −0.209† | −0.209† | −0.209† | −0.209† | −0.211† | −0.202† |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.007) | (0.011) |
| *Experience* | 1.952** | 2.878† | 1.973** | 2.305** | 2.907† | 4.143* |
| | (0.640) | (0.714) | (0.639) | (0.811) | (0.697) | (1.432) |
| *Signal*Experience* | 0.026** | 0.025** | 0.037† | 0.034** | | −0.014 |
| | (0.009) | (0.009) | (0.010) | (0.011) | | (0.014) |
| *Static* | 2.702† | 3.645† | 3.509† | 3.645† | | |
| | (0.313) | (0.451) | (0.402) | (0.451) | | |
| *Static*Experience* | | −1.812** | | −0.662 | | |
| | | (0.625) | | (0.993) | | |
| *Static*Signal*Experience* | | | −0.024** | -0.018 | | |
| | | | (0.008) | (0.012) | | |
| Log likelihood | −11342.52 | −11338.32 | −11337.43 | −11337.20 | −4388.37 | −4388.93 |
| Wald $\chi^2$ test statistics | 2151.16† | 2160.83† | 2159.90† | 2161.21† | 867.93† | 870.51† |
| Left-censored at *Delay* = 0 | | | | 581 | | 452 |
| Uncensored | | | | 3012 | | 1055 |
| Right-censored | | | | 0 | | 1507 |
| Included Observations | | | | 3593 | | 3593 |

*Notes*. *Delay* is censored at 0 (subject random-effects incorporated). Standard errors are reported in parentheses. *Static*Signal* not significant in any of the specifications above. * $p < 0.05$, ** $p < 0.01$, † $p < 0.001$.

**Table 2: Results of Random-Effects Tobit Regression Analyses of *Delay* on the Signal Arrival Time and Experience: Experiment I (A~D) and BM (BM-1 and BM-2)**