Do Online Game Players Overplay?

Vinci Y.C. Chow*
University of California, Berkeley
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Abstract

The amount of time youth spent on game-playing is significant, with a recent representative survey pinning the number at 0.84 hours per day. To investigate the extend gameplaying can be attributed to self-control problems, I implemented a field experiment on a genre of widely played multiplayer online games. Treatment subjects were given a software they could use to limit their duration of play. Throughout the 3.5 month period subjects in the treatment group played on average 40 percent less than those in the control group, as measured by total hours played. The demand for commitment appears limited—while 79 percent of the treatment subjects used the software voluntarily in the first four weeks, the fraction dropped to around 5 percent towards the end of the experiment. 10.4 percent of treatment subjects manifested positive willingness-to-pay for the software at the end of the experiment. We find that the usage of commitment device reduces duration of play but not the frequency of plays, and that players overestimated how long they would play.

Keywords: Time-inconsistency, hyperbolic discounting, cues, commitment, gaming

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1 Introduction

Video game is a major form of entertainment nowadays. The video game industry generates sales over $30 billion, comparable to tobacco. The industry is expected

*508-1 Evans Hall #3880, Berkeley, California 94720-3880. Email: vincichow@berkeley.edu
to grow strongly for the next few years, with most of the growth coming from the online game sector [Gamasutra, 2008]. This paper investigates whether players of a genre of popular online games overplay and how well they can predict how much they play. I define “overplaying” as the amount of leisure consumption that contributes negatively to the player’s welfare, which the player may or may not be aware of.

There are three reasons why overplaying in online games is an interesting topic. First, it is a major leisure activity among younger generations. Teenagers surveyed by the Bureau of Labor Statistics in the most recent American Time Use Survey spent an average of 0.84 hours per day on “playing games and computer use for leisure”, more than any other form of leisure besides watching television [Bureau of Labor Statistics, 2010]. Second, in the midst of its rapid growth comes an increasing concern on overplaying. A number of Asian countries with large gaming populations have instated policies with the aim of reining game-playing behavior, such as shutting off internet connection to game servers at late-night [The Korean Herald, 2010], limiting sales of games to minors [Businessweek, 2010] and introducing in-game disadvantages after prolonged play [The Korean Times, 2010]. Medical clinics targeting video gamers have also started to appear [Computer Addiction Services, 2010, Smith and Jones Clinic, 2010]. The third reason why online games are interesting in the domain of overconsumption is the unique opportunities they present—that their functionality depends on communicating with online servers. This allows the investigator to collect data on individual player’s actual time of play instead of relying on aggregated or self-reported data. Furthermore, depending on the type of data it is looking for, an experiment on online-gaming could be effectively implemented over a large number of subjects for relatively low cost.

To illustrate how players might have demand for commitment, I present a model with a simple binary consumption setting with constant marginal utility and increasing marginal cost. It proposes that by providing different types of commitment devices, one can differentiates between different causes of overplay. Two possible sources of time-inconsistency are incorporated in the model to allow for the possibility of overplaying. First the player can be present-biased, in that she values the current period over future periods more than she would have in previous periods. Second the player can be cue-sensitive, such that in certain states her ability to make rational decision is impaired—for example, if the player happens to see an advertisement of the game, she might be cued and sense a strong desire to play the game at that moment. Psychology
and economics research in present-biasness (see for example Loewenstein and Prelec 1992) has argued for hyperbolic discount rates instead of a single constant discount rate. For analytical tractability, this model adopts the quasi-hyperbolic discounting structure proposed by Laibson [1997a], which captures the essential features of true hyperbolic discounting. As for cue-sensitivity I incorporate a simplified version of the structure proposed by Bernheim and Rangel [2004]—in each period there is a random chance that the player would always play, regardless of the cost involved. Lastly, following O’Donoghue and Rabin [2001], the player is allowed to be fully aware, partially aware or completely unaware of her time-inconsistency.

A field experiment was carried out from February 2010 to July 2010 on players of a popular genre of online games called Massive Multi-player Online Role-Playing Games (MMORPGs), recruited from the undergraduate population of five University of California campuses. The experiment has three parts. First, all subjects installed a software logging their duration of play, and via the software, treatment subjects were additionally provided with commitment devices that allowed them to limit their duration of play. The software sent logged information back to a data server between February 27th 2010 and June 20th 2010. Second, all subjects were asked to make a prediction on the duration they would play in a specific week. Their predictions were then compared to the actual logged hours of play. Third, treatment subjects’ willingness-to-pay for the software were solicited via a incentive-compatible mechanism.

This paper have four goals, of which the first three concern overplaying. The first is to investigate experimentally whether players have demand for commitment devices in a field-setting. By providing subjects with software devices that arguable have no use other than to restraint one’s own duration of play, I can estimate a lower-bound on the fraction of subjects who were aware of their overplaying behavior. In the experiment 79 percent of the treatment subjects used one or more of the provided commitment devices voluntarily, though the number of usage drops significantly after the fourth week.¹ Through a Becker-DeGroot-Marschak mechanism, I find that 10.4 percent of the subjects have a positive willingness-to-pay for the commitment devices I designed, at an average of $4.9.² To my knowledge this is the first study to report an actual willingness-to-pay for a commitment device.

¹All treatment subjects were required to use the commitment devices once. The percentage quoted above excludes these very first usages.
²So the overall average willingness-to-pay is 50 cents.
The second goal is to measure the effect commitment has on players’ behavior, with emphasis on controlling for self-selection. Correlation between commitment usage and reduced gameplay is not necessarily evidence for an effective commitment, as it could be due to light players self-selecting into using commitment. To that end a 2x2 experimental design was implemented—subjects’ preference for being in treatment were solicited in an incentivized method, and the final assignment was done separately for those who expressed a preference and those who did not. The average total hours played by the subjects in the treatment group was 52.3, compared to 85.8 in the control group. Controlling for preference for being in treatment, self-reported experience of play and campus-affiliation, the estimated difference in the number of hours played between the treatment and control groups is statistically significant. There is, on the other hand, no significant decrease in how often subjects started playing—treatment subjects played an average of 51.1 sessions throughout the experiment, while the control subjects played 67.5, both with standard deviations over 100.\textsuperscript{3} This suggests that players who used the commitment devices are likely concerned with suboptimal length of playing but not suboptimal frequency of play.

Third I attempt to distinguish between the different models that could potentially explain overplaying behavior. 29.2 percent of treatment subjects could potentially be categorized as \textit{cue-sensitive} as oppose to being purely \textit{present-bias}, though the evidence was largely suggestive.

The fourth goal is to investigate the accuracy of beliefs—can players accurately predict how much they would play in the future, or are there systemic biases in their projections? The existence of biases would call into doubt the optimality of players’ actions, including the setup of commitment. This investigation also acts as a check on the reliability of self-reported duration of play, which the existing literature on video-gaming has been relying on. Comparing the predictions solicited from subjects and the actual logged durations of play, I find that subjects in the experiment significantly overestimated how long they would play by a factor of two. This could have important implication for previous findings, for if the same overestimation exists in self-reported duration of play, estimates based on such reports would bias upwards. This finding is also inconsistent with the hypothesis that players are naive about their self-control problem in general, for naiveness should have resulted in underes-

\textsuperscript{3}A session is defined as a period of continuous game-play. A session is started by either launching a game or returning from a period computer inactivity.
timation. A shortcoming in this investigation is that prediction solicitations were not incentivized due to complex payment structure required just for answering the questions on overplaying.

Lastly, I was able to obtain the academic records of a substantial subsample of the subjects in the experiment. Subjects in the experiment has done relatively well in school, as measured by an average cumulative grade-point-average (GPA) of 3.22 within treatment and 3.12 within control. This challenges the notion that gameplaying is associated with lower academic performances.

Before turning to the model a brief summary on the existing literature on commitment and video-game overplaying is warranted. Despite advancement in theories that predict individuals have demand in commitment devices, studies on the usage of commitment are relatively sparse. See Bryan et al. [2010] for an overview of theories and evidence on demand for commitment. Read et al. [1999] provided subjects with the choice of “highbrow” and “lowbrow” movies, and they find that subjects who were being asked to choose ahead of the time were more likely to pick “highbrow” movies. Wertenborch [1998] examined scan data from a grocery chain and argues that the relative inelastic demands for vice goods are evidence of consumers exerting self-control. In a laboratory experiment, Ariely and Wertenbroch [2002] assigned subjects proofreading tasks and provided commitment in form of deadlines. They find that involuntary deadlines gave rise to the highest productivity in terms of proofreading accuracy, followed by voluntary deadlines and no deadlines. More recently, Thaler and Benartzi [2004] allowed employees in three companies to commit to a higher retirement contribution when a pay raise arrives. They found saving rates increased by as much as 10.1 percentage points with commitment. Ashraf et al. [2006] implemented a field experiment around a commitment savings product provided by a Filipino bank. 28.4 percent of those being offered the commitment utilized it, and utilization was correlated with lower discount rate. Gine et al. [2010] offered smokers a commitment savings account that confiscate their savings if they were tested positive for nicotine and cotinine six months after opening account. They found that smokers being offered the commitment were 3 percentage points more likely to pass the nicotine and cotinine test.

I am aware of two studies in this area that had utilized games as the activity of

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4Theories that predict demand for commitment include hyperbolic discounting and its variants [Laibson, 1997a, O’Donoghue and Rabin, 1999, Frederick et al., 2002], temptation utility [Gul and Pesendorfer, 2001] and cue-theories [Laibson, 1997b, Bernheim and Rangel, 2004].
interest. Miller and Navarick [1984] conducted perhaps the earliest laboratory study that used video game in a test of preference for immediate gratification. In another laboratory study, Fernandez-Villaverde and Mukherji [2006] explicitly test hyperbolic discounting through providing a commitment on duration of game play. Within this area, as far as I know this study is the first to carry out an experiment on actual game platforms with real players, rather than utilizing the game on experimental subjects who are not necessary players of the game in concern outside the experiment.

Psychologists have studied video-game and media addiction more extensively. Studies have shown that video game usage is correlated with lower academic performance [Anand, 2007, Skoric et al., 2009], higher hostility [Chiu et al., 2004] and lower social support [Longman et al., 2009]. In a national study, Gentile [2009] puts the percentage of pathological video-game players at 8 percent. He demonstrates that players who meet clinical-style criteria for pathological gaming had poorer school performance after controlling for demographics, and that pathological gaming is not identical to high amount of play. One potential drawback of these studies, however, is that all of them relied on self-reported duration of play, as a legitimate concern would be players reported their durations of play with bias. By designing commitment devices, this study contributes to a very small literature on treatment for videogame addiction. Griffiths and Meredith [2009] review existing treatment methods. Very few empirical studies of treatment exist (see for example Kuczmiczyk et al. 1987 and Keepers 1990), and I am unaware of any of controlled study of treatment.

The remainder of this paper is organized as follows. Section 2 presents a simple model of overplaying and usage of commitment, followed by the results of the experiment in Section 3. Section 4 concludes the paper.

2 Model

2.1 Setup

A player faces a finite dynamic optimization problem of 4 periods. At each period, if playing has not been initiated, the player first decides whether to initiate playing. I denote initializing the game in Period as \( z_t = 1 \) and not initializing as \( z_t = 0 \). The game only needs to be initialized once, so \( z_{t>1} = 1 \) if \( z_t = 1 \). If the player chooses not to initiate playing the period ends, giving her a utility of zero for that period.
instead she chooses to initiate playing, in each subsequent period she faces the choice of whether to play in that particular period. The assumption that no actual playing occur in the period of initialization represents the time it takes to set the game up for playing. Because of the assumption, there is at most three periods of actual gameplay in the model.

Denote playing by \( a_t = 1 \) and not playing by \( a_t = 0 \). The instantaneous utility from playing in period \( t \) is

\[
 u_t(a_t) = (\alpha_t + \epsilon) \cdot a_t
\]  

(2.1)

where \( \alpha_t \) is a non-random process depending only on \( t \) and \( \epsilon \) a mean-zero random variable with cumulative distribution \( F(\epsilon) \), realized immediately after the player decides to initiate play. This formulation captures in the simplest form a situation where instantaneous utility is random.

Playing incur a cost in the future. This is captured by a single cost paid in Period 5, \( C(n) \), where \( n \) is the number of periods played. I assume there is increasing maginal cost, so \( \forall n c(n) = C(n) - C(n - 1) > 0, c(n) > c(n - 1) \) and \( C(0) = 0 \).

I assume that the correct measurement of player’s welfare is the exponentially-discounted utility,

\[
 U_0 = \sum_{\tau=2}^{4} \delta^\tau u_\tau(a_\tau) - \delta^5 C \left( \sum_{i=2}^{4} a_i \right)
\]  

(2.2)

In any given period \( t \), however, the player is potentially present-biased [Laibson, 1997a],

\[
 U_t = u_t(a_t) + \beta \left[ \sum_{\tau=t+1}^{4} \delta^{\tau-t} u_\tau(a_\tau) - \delta^5 C \left( \sum_{i=2}^{4} a_i \right) \right]
\]  

(2.3)

where \( \beta \leq 1 \) and \( \delta \leq 1 \). When \( \beta < 1 \) then player is present-biased. I allow for the possibility that the player holds wrong belief about her inconsistency in the future. The player believes that

\[
 \hat{U}_t = u_t(a_t) + \hat{\beta} \left[ \sum_{\tau=t'+1}^{4} \delta^{\tau-t} u_\tau(a_\tau) - \delta^5 C \left( \sum_{i=2}^{4} a_i \right) \right] \text{ for } t' > t
\]  

(2.4)

where \( \beta \leq \hat{\beta} \leq 1 \). The player is \( \beta \)-naive if \( \hat{\beta} = 1 \), partially \( \beta \)-sophisticated if \( \beta < \hat{\beta} < 1 \) and fully \( \beta \)-sophisticated if \( \hat{\beta} = \beta \) [O’Donoghue and Rabin, 2001].

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5It can be shown that independent noises across time periods give the same results, as long as they are bounded by the increase in marginal cost and can only be learnt by the player through playing.
To capture the effects of cues I assume once the game is initialized, in every period the player get cued and enters a *hot* mode with probability $p$. When in hot mode the player plays for sure in that period, even if playing is not optimal. Furthermore she will not be able to initiate any of the commitment devices I introduce later on. If the player does not enter the hot mode she is in the *cold* mode, in which she chooses her action optimally. Similar to present-biasness the player can be sophistication or naive with respective to the value of $p$ to be $\hat{p}$. Because the player always play in the hot mode, throughout this paper I use “choose to play” or “willing to play” to denote gameplaying in the cold mode.

Because $\epsilon$ and states are random, at every period $t$ the player maximizes the expected value of $U_t$, given her beliefs $\hat{\beta}$ and $\hat{p}$. If the game has not been initialized and $t \in W$, the player solves

$$\max_{z_t} E_t[U_t | \hat{\beta}, \hat{p}, z_t]$$

After the game has been intialized, she solves instead

$$\max_{a_t} E_t[U_t | \hat{\beta}, \hat{p}, a_t]$$

Note that by 2.3, $U_t$ does not directly depend on the beliefs $\hat{\beta}$ and $\hat{p}$. The two affect $U_t$ indirectly through the player’s belief of what her future actions $a_{t+1},...,a_4$ would be.
Definition 1. The player is overplaying if \( a_t = 1 \) and

\[
u_t(1) < \delta^{5-t} \left\{ E_t \left[ C \left( 1 + \sum_{t' \neq t} a_{t'} \right) | a_t = 1 \right] - E_t \left[ C \left( \sum_{t' \neq t} a_{t'} \right) | a_t = 0 \right] \right\} \tag{2.7}
\]

Definition 1 defines overplaying as the marginal benefit of playing in a period being smaller than the expected marginal cost. By comparing benefit to expected cost, overplaying is not limited to the periods where playing is actually suboptimal—a player is also considered overplaying if she plays with the expectation that she will play sub-optimally in the future.

Since gameplay mostly occur in relatively short time-frame, I make the following simplifying assumption.\(^6\)

Assumption 1. \( \delta = 1 \).

To ensure that cue-triggered overplaying could happen over all game-playing strategy the player might choose, I also make the following modeling assumption,

Assumption 2. \( \alpha_4 + \epsilon < \beta c(1) \).

Assumption 2 bounds instantaneous utility in Period 4 from above, guaranteeing that playing in the last period will not be chosen even by a present-biased player,

Lemma 1. The player never chooses to play in Period 4.

Proof. See Appendix. \( \square \)

Lastly to simplify, I assume that instantaneous utility is identical in earlier periods,

Assumption 3. \( \alpha_2 = \alpha_3 \).

2.2 Strategy without Commitment

The solution to the model involves a series of thresholds for \( \epsilon \), one for each possible state, above which the player chooses to play in that state. I denote the threshold for Period \( t \) given \( \beta \) and \( \hat{p} \) as \( x_t(n_{t-1}, \beta, \hat{p}, \epsilon) \), the time-consistent threshold as \( \bar{x}_{t+1} = x_t(n_{t-1}, 1, \hat{p}, \epsilon) \) and the player’s perceived threshold as \( \hat{x}_{t+1} = x_t(n_{t-1}, \hat{\beta}, \hat{p}, \epsilon) \), where

\(^6\)While being a realistic assumption, Assumption 1 does rule out several interesting phenomena. See (appendix? another chapter/paper?) for a detailed discussion of the effects of \( \delta \).
the number of periods played on or before \( t - 1 \). The threshold depends on \( \epsilon \) itself because future thresholds also depend on \( \epsilon \), and they in turn affect the current threshold.

**Proposition 1.** \( x_t \) is strictly increasing in \( \beta \).

**Proof.** See appendix. \( \square \)

**Corollary 1.** Holding the initialization period fixed, number of periods the player chooses to play and the expected number of periods the player actually play are weakly decreasing in \( \beta \).

**Proof.** This follows from 1. The relationships are not strict due to indivisibility of a period, meaning that small changes in \( x_t \) might not have an effect. \( \square \)

**Proposition 2.** \( x_3 \) is strictly increasing in \( \hat{p} \) and does not change with \( \hat{\beta} \). For \( x_2 \), there exist an \( \tilde{\epsilon} \) such that \( x_2 \) is strictly increasing in \( \hat{p} \) and weakly increasing in \( \hat{\beta} \) if \( \epsilon < \tilde{\epsilon} \). If \( \epsilon > \tilde{\epsilon} \), \( x_2 \) is strictly decreasing in \( \hat{p} \) and \( \hat{\beta} \) at a finite set of points for every \( \epsilon \), but is strictly increasing in \( \hat{p} \) and invariant in \( \hat{\beta} \) otherwise. The finite set of points is given by \( \{ (\hat{p}, \hat{\beta}) | \epsilon = \hat{x}_3^0 \text{ or } \epsilon = \hat{x}_3^1 \} \).

**Proof.** See appendix. \( \square \)

It is intuitive that a more time-consistent player chooses to play fewer sessions. What is perhaps surprising is the non-monotonic relationship between sophistication and the threshold in Period 2. This is due to the fact that the threshold in Period 2 depends on whether the player believes she is going to play in Period 3. A change in belief induces discrete jumps in \( x_2 \), which is otherwise generally increasing in \( \hat{p} \). When the game is not very fun (low \( \epsilon \)), the player will only be playing if she believes she is not going to overplay (high \( \hat{\beta} \) and low \( \hat{p} \)), and she will choose to play early and only in Period 2. Because she is not expecting herself to choose playing in Period 3, an increase in \( \hat{\beta} \) or \( \hat{p} \) can only make her less likely to play in Period 2. But when the game is fun (high \( \epsilon \)), the player is likely playing in both Period 2 and 3. As \( \hat{\beta} \) or \( \hat{p} \) increases, she eventually believes she is not going to choose playing in Period 3, and this causes the threshold in Period 2 to discretely decrease.

**Corollary 2.** Holding the initialization period fixed, number of periods the player chooses to play as well as the expected number of periods the player actually play are weakly decreasing in \( \hat{p} \) and \( \hat{\beta} \).
Proof. 2 shows that $x_2$ only decreases if the player goes from planning to play in Period 3 to not doing so, and thus the drop in $x_2$ can at most induce the same number of planned periods of play. Outside of these cases, $x_2$ is strictly increasing in $\hat{p}$ and weakly increasing in $\hat{\beta}$, while $x_3$ is strictly increasing in $\hat{p}$ and invariant in $\hat{\beta}$. The number of periods the player would choose play is therefore weakly decreasing. □

**Proposition 3.** $x_2(0, \beta, \hat{p}, \epsilon) = x_3(1, \beta, \hat{p}, \epsilon)$, and if $\epsilon > x_2(0, \beta, \hat{p}, \epsilon)$ then $\epsilon > x_3(0, \beta, \hat{p}, \epsilon)$.

Proof. See appendix. □

**Corollary 3.** If the player is willing to play the first period in Period 2, she is also willing to play the first period in Period 3. And if she is willing to play in Period 3 regardless of whether she has played in Period 2, she will always play in Period 2.

Proof. The first part follows directly from second part of 3 and the second follows from the first part of 3. □

The first part of Corollary 3 says that when all else equal, the player is always more willing to play in a later period. Intuitively, the probability of cue-triggered playing is the lower in the later period, so for the same parameters the player is more willing to play. The second part says that if there is incentive to always play in the future, the same incentive would also induce playing now. This follows naturally from equal instantaneous utility in the two periods and the lack of exponential discounting.

**Corollary 4.** If the player believes she is always willing to play in Period 3 regardless of whether she has played in Period 2, she will always play in Period 2.

Proof. $\hat{x}_3^0 = x_3(0, \beta, \hat{p}, \epsilon) \geq x_3(0, \beta, \hat{p}, \epsilon)$. The later follows from 1. □

Corollary 4 holds because $\hat{\beta}$ is restricted to be bigger than $\beta$, so the player can either believe she is equally likely to play in the future than she actually is, or believe she is less likely to do so. As a result she is more weakly more willing to play in Period 2.

**Corollary 5.** Suppose the player currently only chooses to play in Period 2. As $\hat{p}$ increases, she would eventually switch and choose to play only in Period 3. If in addition $\epsilon < \tilde{\epsilon}$, she would also do so as $\hat{\beta}$ increases.
Proof. When $\epsilon < \tilde{\epsilon}$, this follows from $\epsilon > x_2(0, \beta, \hat{p}, \epsilon)$ and $x_t$ increasing in $\hat{p}$ and $\hat{\beta}$. When $\epsilon \geq \tilde{\epsilon}$, $\epsilon > x_2(0, \beta, \hat{p}, \epsilon)$ implies that $\hat{p} \notin \{(\hat{p}, \hat{\beta})|\epsilon = \hat{x}_0^0 \text{ or } \epsilon = \hat{x}_1^0\}$, so $x_t$ is increasing in $\hat{p}$.

The result that naive over present-biasness delays playing is a manifestation of the “preproperation effect” explored in O’Donoghue and Rabin [1999]. When the game is not very fun, the naive player erroneously believes that she would be able to refrain from playing in the future. This gives her incentive not to play in the current period, because she thought she would be able to completely avoid the negative consequence of playing. A sophisticated player, on the other hand, realize that she would play in the next period only if she refrain in the current period, and as such sees no reason to delay playing.

**Proposition 4.** $E_t[U_\tau|\hat{\beta}, \hat{p}, z_t]$ is strictly decreasing in $\hat{p}$ for $\tau \geq t$ and weakly increasing in $\hat{\beta}$ for $\tau = t$. Moreover, the change is weakly larger for $z_t = 1$.

**Proof.** See appendix.

**Corollary 6.** The player initializes the game weakly later as $\hat{p}$ increases and $\hat{\beta}$ decreases, and might not initialize if the former is larger enough or the later is small enough. $\beta$ has no effect.

**Proof.** The first part follows from 4. The second holds because both the benefit and cost of playing is inside $\beta$ before initialization, so $\beta$ is irrelevant to the decision to initialize.

A sophisticated player initializes late when the expected benefit from optional playing a second period is less than the expected cost from overplaying, which is more likely to be the case as $\hat{\beta}$ decreases and $\hat{p}$ increases. And when the expected cost of overplaying exceeds the positive benefit of playing, the player might choose not to initiate playing, even if playing is optimal for some realization of $\epsilon$.

### 2.3 An Example

Let $\alpha_2 = \alpha_3 = \alpha = 2.2$, $\alpha_4 = -0.6$, $\epsilon \in \{-1.25, 0, 1.25\}$ with equal chances and $C(n) = n^2$. Since there is no discounting for the exponentially-discounted expected
utility, it simply depends on the number of periods played. The welfare-maximizing strategy is given by

$$\max_n U_0 = \max_n \{(\alpha + \epsilon) \cdot n - C(n)\} = \max_n \{(2.2 + \epsilon) \cdot n - n^2\}$$

It is straightforward to verify that the optimal strategy is not to play if $\epsilon = -1.25$, play in one period if $\epsilon = 0$ and in two periods if $\epsilon = 1.25$.

Departing from time-consistency, I start with the case of a naive present-bias player who is cue-insensitive, with $\beta = 0.7$, $\hat{\beta} = 1$ and $\hat{p} = p = 0$. Such a player believes erroneously her future action would be time-consistent. To see how the player would overplay, consider the case where $\epsilon = 0$, in which the time-consistent strategy is to play in one period. With $\beta < 1$, the future cost of playing is being discounted relative to its current benefit, and therefore the naive player will at least play the time-consistent amount. Furthermore because of the discounting introduced by $\beta$, she would want to play as soon as possible. She would thus initiate the game in Period 1 and play in Period 2. In Period 3, the player faces the choice of whether to play a second period,

$$U_3(a_3 = 1) = \alpha \cdot 1 - \beta C(2) = -0.6$$
$$U_3(a_3 = 0) = \alpha \cdot 0 - \beta C(1) = -0.7$$

She would therefore play for a second period. A similar calculation would show that the player overplays for one period when $\epsilon = -1.25$.

Would Sophistication have helped in this case? The sophisticated player can foresee that she would overplay in Period 3, and as such she might choose not to play in Period 2. However, given the expectation that she would be playing in Period 3, her utility in Period 2 is

$$U_2(a_2 = 1) = \alpha + \beta [\alpha - C(2)] = 0.94$$
$$U_2(a_2 = 0) = 0 + \beta [\alpha - C(1)] = 0.84$$

Notice that the marginal utility from playing is identical to the case of the naive player, and thus the sophisticated player will behavior exactly identical to the naive player, despite knowing that she would be overplaying. In general, once playing is initiatized a sophisticated player will often behave very similarly to a naive player,
the intuition being that she is already under the influence of present-biasness. But initialization is the key here—the sophisticated player can opt to initiate the game later than the naive player. If the sophisticated player initializes at Period 2 instead, she can bindingly limit herside to at most one period of playing.\footnote{This is so because playing in Period 4 is suboptimal by assumption. The player will not play in Period 4 unless she is cue-sensitive.} The tradeoff of doing so is she would not be able to play two periods if $\epsilon$ turns up to be high. In this example,

$$E_1[U_1|z_1 = 1] = \beta \left\{ \frac{1}{3}[(\alpha - 1.25) - C(1)] + \frac{1}{3}[2\alpha - C(2)] + \frac{1}{3}[2(\alpha + 1.25) - C(2)] \right\}$$

$$= 0.76$$

$$E_1[U_1|z_1 = 0, z_2 = 1] = \beta \left[ \frac{1}{3}(\alpha - 1.25) + \frac{1}{3}\alpha + \frac{1}{3}(\alpha + 1.25) - C(1) \right] = 0.84$$

so the sophisticated player would indeed initialize the game late.

Turning to cue-sensitivity. If the player is naive about her cue-sensitivity, her strategy is essentially identical to the case without cue-sensitivity—play if the marginal benefit of playing is higher than the $\beta$-discounted marginal cost. She will be weakly playing more due to cue-triggering, but such playing is not under her control. In contrast, a sophisticated player has in every period an extra incentive not to play, so to accommodate for the possibility that she might overplay in future periods. Suppose
\( \hat{p} = 0.5 \) and using again \( \epsilon = 0 \) as an example, it can be shown that if the game is initialized in Period 1, the player would only play in Period 3 if she had not played in Period 2. She takes this into consideration in Period 2,

\[
E_2[U_2|a_2 = 1] = \alpha + \beta \left\{ \hat{p} [\alpha_4 - C(2)] + (1 - \hat{p}) [-C(1)] \right\} = 0.24
\]
\[
E_2[U_2|a_2 = 0] = 0 + \beta \left\{ \alpha + \hat{p} [\alpha_4 - C(2)] + (1 - \hat{p}) [-C(1)] \right\} = -0.42
\]

and play in Period 2 but not in Period 3. Such refrain do not, however, always happen. For example if \( \beta = 0.5 \), \( E_3[U_3|a_2 = 1, a_3 = 1] = -1.19 > -2.53 = E_3[U_3|a_2 = 1, a_3 = 0] \), so the player would not refrain from playing. Intuitively there is a tug-of-war between present-biasness and awareness of cue-sensitivity—the former makes the player more likely to choose to play early, while the later makes her less likely to do so.

Figure 2.2 plots the \( \epsilon \)-thresholds for the parameters in this example, varying with \( \beta \) and \( \hat{p} \), assuming that she never enters the hot mode in any period. The player becomes less likely to play as either \( \beta \) or \( \hat{p} \) goes up. As expected, present-biasness (lower \( \beta \)) makes the player more likely to play in earlier periods, while awareness of cue-sensitivity (higher \( \hat{p} \)) makes the player less likely to play in earlier periods.

### 2.4 Strategy with Commitment

In Corollary 6, the restriction that playing has to end after Period 4 is in essence being used by the player as a commitment device. This strategy has several drawbacks, however. First, the player does not observe the realized value of \( \epsilon \) when she picks an initiating period, when a more ideal commitment device would instead allow for a longer duration of play when \( \epsilon \) turns out to be high. Second, in many instances starting late might not be feasible, for example when there is no binding end to gameplay. Lastly, it imposes a delay in game-play, which is costly had exponential discount factor not being assumed to be one.

Because of the these reasons, an overplaying player has demand for commitment other than starting late. Suppose the player is offered one or both of the following two types of commitment devices: an ex-ante device \( (X) \) in which she can commit to a definite duration of play before initiating the game, and an in-game device \( (I) \), in which, if she is in cold mode, commit to end the game after the current period. With either device, once the designated last period of game-play has elapsed \( a_t = 0 \)
for all subsequent periods. Denote $X = 0$ and $I_t = 0$ as not using the respective commitment devices, $X = x$, $x > 0$ as using Device $X$ to set a limit of $x$ periods, and $I_t = 1$ as using Device $I$ in Period $t$. I assume that if the player is indifferent between using a device or not, she would not use it—this could be justified by assuming that there is a small cost involved in using any of the devices.

The two devices each have their advantages and disadvantages. $X$ is always available but cannot be adjusted with respect to realization of $\epsilon$, while $I$ can be adjusted but may not be available if the player falls prey to cues. Moreover, neither device prevents overplaying in Period 2.

**Proposition 5.** (*Usage of Commitment*)

- A naive player never uses a commitment device.
- A $\beta$-sophisticated player would use $X$ in Period $t$ if $X$ is the only commitment device available and $\exists x > 0$ s.t. $E_t[U_t|X_t = x] > E_t[U_t|X_t = 0]$. She would use $I$ in Period $t$ if $(\alpha_{t+1} + \epsilon) a_{t+1} - C(n_t + 1) < 0$ and $(\alpha_{t+1} + \epsilon) a_{t+1} - \hat{\beta} C(n_t + 1) > 0$.
- A $p$-sophisticated player would always use $X$. She would use $I$ in Period $t$ if $(\alpha_{t+1} + \epsilon) a_{t+1} - C(n_t + 1) < 0$.

*Proof.* See Appendix. \(\square\)

## 2.5 Example Continued: with Commitment

Consider again the player who is present-biased but cue-insensitive. If she is naive, she never foresee herself overplaying, so she never uses any commitment device. But if she is sophisticated, her awareness of her present-biasness might lead her to using a commitment.

If the player only has access to $X$, would she use it? In this example she would. Consider the choice of setting a limit of 1 period of gameplay,

\[
E_1[U_t|X = 0] = \beta \left\{ \frac{1}{3} (\alpha - 1.25) - C(1) \right\} + \frac{1}{3} [\alpha \cdot 2 - C(2)] + \frac{1}{3} [(\alpha + 1.25) \cdot 2 - C(2)] = 0.74
\]
\[
E_1[U_t|X = 1] = \beta \left[ \frac{1}{3} (\alpha - 1.25) + \frac{1}{3} \alpha + \frac{1}{3} (\alpha + 1.25) - C(1) \right] = 0.85
\]
Notice that setting $X = 1$ gives the same utility as initializing in Period 2 as calculated before. This should not be surprising, as both limit the maximum number of periods the player can play. The player does not always use $X$ though, but rather she would use it only if the expected cost of overplaying outweighs the expected benefit of optimal playing. For example, suppose $\epsilon \in \{-2.5, 0, 0.83\}$ with probability $(\frac{1}{6}, \frac{1}{3}, \frac{1}{2})$ respectively. This $\epsilon$ distribution raises the probability of high realization while maintaining the expected values. The same calculations now yield $E_1[U_1|X = 0] = 0.81 < 0.75 = E_1[U_1|X = 1]$, so she would not use $X$.

Back to the original $\epsilon$ distribution, suppose the player only has access to $I$ instead. If she uses $I$ at Period 1 when $\epsilon = 0$,

\[
E_2[U_2|a_2 = 1, I_2 = 1] = \alpha - \beta C(1) = 1.5 \\
E_2[U_2|a_2 = 1, I_2 = 0] = \alpha + \beta [\alpha - C(2)] = 0.94 \\
E_2[U_2|a_2 = 0, I_2 = 0] = \beta [\alpha - C(1)] = 0.84
\]

So using $I$ is indeed better. Furthermore, since $I$ completely eliminate her self-control problem when $\epsilon = 0$ while allowing an additional period of playing when $\epsilon = 1.25$, it is strictly better than $X$. The player therefore would only use $I$ regardless of whether $X$ is available.

Turn to cue-sensitivity. Notice that since playing in Period 4 is never optimal, a sophisticated player would always use $X$ whenever available, if only to prevent herself from playing in Period 4. In addition, she might set a shorter limit if the probability of being cue-triggered is high enough. Consider $\hat{p} = 0.5$ and $\hat{\beta} = \beta = 1$, so the player is not present-biasness.

\[
E_1[U_1|X = 2] = \frac{1}{3} \left\{ \hat{p}^2 [2(\alpha - 1.25) - C(2)] + 2(1 - \hat{p})\hat{p} [(\alpha - 1.25) - C(1)] + (1 - \hat{p})^2 \cdot 0 \right\} \\
+ \frac{1}{3} \left\{ \hat{p} [2\alpha - C(2)] + (1 - \hat{p}) [\alpha - C(1)] \right\} + \frac{1}{3} [(\alpha + 1.25) \cdot 2 - C(2)] \\
= 1.05 \\
E_1[U_1|X = 1] = \frac{1}{3} (\alpha - 1.25) + \frac{1}{3} \alpha + \frac{1}{3} (\alpha + 1.25) - C(1) = 1.2
\]

Thus setting a limit of one period is the better option. Furthermore, for the cue-sensitive player using $X$ is strictly better than initializing late—on one hand, initializing in Period 3 is never worthwhile by assumption, on the other hand initializing in Period 2 leaves room for the possibility of cue-triggered overplaying in Period 4.
Now consider the scenario when only $I$ is available. Whenever the player is in cold mode at the beginning of a period, she evaluates whether she should use the device. Suppose $\epsilon = 0$. Since playing in only one period is optimal, we would therefore expect the player to use $I$ at the earliest possible occasion. This is indeed the case. Taking into consideration that in Period 3 she would use $I$ if she is in cold mode, the player’s expected utility in Period 2 is

$$E_2[U_2|a_2 = 0, I_2 = 0] = \hat{p}^2 [\alpha + \alpha_4 - C(2)] + (1 - \hat{p})(1 + \hat{p}) [\alpha - C(1)] = 0.3$$

$$E_2[U_2|a_2 = 1, I_2 = 0] = \alpha + \hat{p}^2 [\alpha + \alpha_4 - C(3)] + \hat{p}(1 - \hat{p}) [\alpha - C(2)] + (1 - \hat{p}) [-C(1)] = -0.6$$

$$E_2[U_2|a_2 = 1, I_2 = 1] = \alpha - C(1) = 1.2$$

and so she would use $I$ in Period 2.

Finally, consider the case when both devices are available. As above it is optimal to set $X = 2$. If the player is also present-biased, she would also use $I$ in Period 2,

$$E_2[U_2|a_2 = 0, I_2 = 0, X = 2] = \beta [\alpha - C(1)] = 0.84$$

$$E_2[U_2|a_2 = 1, I_2 = 0, X = 2] = \alpha + \beta \{\hat{p} [\alpha - C(2)] + (1 - \hat{p}) [-C(1)]\} = 1.22$$

$$E_2[U_2|a_2 = 1, I_2 = 1, X = 2] = \alpha - \beta C(1) = 1.5$$

Note that present-biasness is needed here only because there is no exponential discounting. Were the later to be present, the cue-aware player would have used both devices even in the absence of present-biasness.

### 3 Field Experiment

#### 3.1 Design

The core idea was to design a controlled experiment in which treatment subjects receive commitment devices, with an emphasis on controlling for self-selection among players. Multiple commitment devices were provided, first for testing whether subjects have demand for multiple devices for the same occasion, and second to reduce the artificial limitation on when blocks have to be set up. Furthermore to increase the likelihood that self-control is indeed a problem, I chose to recruit players from a
genre of games that is known to be time-consuming.

World of Warcraft® is a Massive-Multiplayer Online Role-Playing Game (MMORPG in short). The game is set in a fantasy world, where players interact with the world via customizable avatars—a human warrior or a dwarf hunter for instance—and can engage in a variety of activities. While combating computer-controlled characters or other players are a prominent feature of the game, it is entirely possible for a player to engage only in “civic” activities such as tailoring or auctioneering. Playing World of Warcraft requires a paid subscription, which costs $15.99 a month in the United States. The price is substantially lower in countries like China. The maker of the game, Blizzard Entertainment, claims eleven million subscribers, making it one of the most popular, if not the most popular, subscription-based MMORPG [Guinness World Records]. To enlarge our potential subject pool I also recruited subjects playing MMORPG’s that are similar to World of Warcraft in nature, such as Everquest®, Maplestory® and Star Wars: Galaxy®. In our final subject pool 75 percent of the subjects played World of Warcraft.

I programmed a Windows®-based commitment software, which I named BlokSet, that allows a user to limit when, or for how long, she can play MMORPG’s. BlokSet allows the user to set blocks, which are time periods during which BlokSet prevents her from playing MMORPG’s. Blocks work in two ways. If the user try to launch a game software during a block that she had set, BlokSet will prevent the software from launching. If the user was playing when a block is scheduled to begin, BlokSet will terminate the game software at the beginning of the block. Before that happens, BlokSet gives verbal reminders and show a countdown timer so that the user can wrap up the game session before the block begins. BlokSet consists of three different devices, each allow the user to set blocks in a different way. The Calendar Blocker allows the user to schedule blocks on a calendar, for all or part of any day. The Pre-Game Blocker gives user the option to set a block for that session at the beginning of a game-session. The user chooses how long to play, and when that time is up the game-session ends and the block begins. In addition, the Pre-Game device allows the user to display a timer indicating how long she has been playing. The timer can be displayed with or without setting a block. Lastly, the In-Game Blocker allows the user to set a block in the middle of a game session, in a very similar fashion to the Pre-Game Blocker. There is a “settings” page where the user can select default options for how the three devices work. 3.1 demonstrates the interfaces of the three...
different blockers.

The provision of multiple devices allows for a flexible representation of the theoretical devices in the previous section. Available for setup far ahead of actual gameplay, the Calendar Blocker is an implementation of the ex-ante device \((X)\), while the In-Game Blocker, available only while playing, is an implementation of the in-game device \((I)\). The remaining device, the Pre-Game Blocker, can be interpreted as either an implementation of \(X\) or \(I\), depending on how far in the future does \(\beta\) comes into effect.

Subjects were recruited via two channels between mid-January to mid-February—recruitment postings on Facebook, as well as fliers posted throughout five campuses of University of California: Berkeley, Davis, Santa Cruz, Santa Barbara and San Diego.\(^8\) The recruitment materials included a link to a sign-up website and clearly described the criteria for getting into the experiment: potential subjects must be an UC undergraduate, a player of a supported MMORPG game and a user of any version of Windows operating system. In practice the last requirement was largely redundant since very few games support alternative operating systems, and virtually all players use Windows due to the fact that most games are optimized for that platform.

During signup, potential subjects were informed of how they would be pay if they were accepted into the experiment: a first payment of up to $15 after they installed a software package, and a second payment up to $25 at the end of the experiment. They were also told that their payments would be $15 and $25 unless they chose to give up some of the payments as part of the experiment.\(^9\) They were then given a tour of the features of Blokset, and were asked of whether they would be willing to pay $1 to try the software during the experimental period. This question allowed us to separate potential subjects who had an interest in commitment from those who did not. I describe these subjects as having a willingness-to-try. To avoid subjects strategically selecting an answer with the intention to increase their chance of entering the experiment, it was made clear in the description that I were looking for both subjects who answered “yes” and answered “no”, and that acceptance would not be announced until signup was over.

\(^8\)A small scale pilot experiment on 40 subjects was carried out in Spring 2008 on the Berkeley campus alone, following largely the same experimental design. The results from the pilot were statistically insignificant but qualitatively similar.

\(^9\)The framing of the experimental tasks as involving giving up part of their subject payment might result in a reference effect, reducing the subjects’ willingness to choose options that reduce their payments. This works against my hypothesis, and thus strengthen any result I find.
Figure 3.1: Interface of BlokSet

*Top:* Calendar Blocker. A grid representing the days in a given month. Clicking on a cell brings up a dialog box, allowing the user to set up blocks. BlokSet prevents any monitored games from being launched when a block is in effect.  

*Bottom-left:* Pre-Game Blocker. Whenever the user launches a monitored game, BlokSet freezes the game and pops up a dialog box, giving the user the option to set a limit and a block. In addition, the user can opt to display a timer and activate vocal reminders.  

*Bottom-right:* In-Game Blocker. While in-game, the user can click on a pre-assigned key to bring up a dialog on screen, allowing her to enter a limit. When the limit is reached, the game terminates.
With the information on the subjects’ interest in commitment I implemented a 2x2 experiment design. Half of the subjects who were willing to pay $1 to try the Blokset were assigned with it, so were half of the subjects who indicated otherwise. This minimizes the confound that could arise if awareness of self-control problem is correlated with less playing. All subjects were assigned a monitoring software that monitored their computer usage, with treatment subjects receiving BlokSet in addition.

Acceptance was announced via email between February 27th and March 2nd, 2010, in which subjects were instructed to download and install their assigned software packages from the experiment’s website. Subjects were not told of which package they were assigned with in the email. This was done so as to not affect their prediction on duration of play, which I solicited in the next step. When the subjects log in to the experiment’s website they were first asked to complete a pre-treatment survey, followed by making a prediction on how many hours they would play in the week of March 7th to March 13th, before they were informed on which package they would receive.10 After making the prediction, subjects were told of which package they were assigned to, and a link to download the package was provided. I mailed subjects their first payment in checks after I confirmed that the monitoring software was installed on their computer and sending us data. $1 payment was deduced from the first payment to those who indicated a willingness-to-pay and were assigned the Blokset.

To avoid complications in soliciting willingness-to-try I did not implement a passive observation period, so subjects assigned with BlokSet were able to access its feature the moment they installed the software. For the following 17 weeks I monitored when and for how long they played MMORPG’s, and for treatment subjects I also monitored their usage of the three commitment devices. Data collection was disabled on the servers on June 20th.

An email was sent on May 10th (Week 10) to treatment subjects reminding them of the availability of the commitment devices. This investigates whether there was any reduction in commitment usage due to subjects forgetting about the availability of the devices. Control subjects received an email simply notifying them of the time till the end of the experiment.

On June 28th subjects were instructed via email to complete a final step on the

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10 This created a potential complication, as subjects might have different expectations on how much they would play depending on the availability of commitment devices. 3.6 shows that this issue does not seem to be significant.
experiment’s website, before July 5th, in order to obtain their second payment of up to $25. On the website they were informed that BlokSet would cease to function on July 31st, and were offered the chance to forfeit a part of their second payment for the right to keep using BlokSet after July 31st. I used a Becker-DeGroot-Marschak mechanism [Becker et al., 1964]. To make the randomness credible, subjects were informed that the threshold used would be a transformation of the closing price of Dow Jones Industrial Average on July 5th. The transformation formula was shown on the same screen. At July 5th I provided subjects who indicated a willingness-to-pay higher that the random threshold with a code to activate BlokSet permanently. A second payment of $25, minus the randomly generated threshold for subjects who indicated a willingness-to-pay higher than it, was mailed to subjects subsequently.

3.2 Descriptive Statistics

Table I summarizes the characteristics of the data. 180 potential subjects signed up, 27 of whom indicated a willingness-to-try. Our initial assignment allocated 14 potential subjects with willingness-to-try to the treatment group and 13 to control, and 76/77 potential subjects with no willingness-to-try to each of the groups. 75 potential subjects never responded to the acceptance email, which left us with the final subject count of 105, of which 48 were in the treatment group. Attribution ratio was similar across willingness-to-try in the treatment group, with 6 out of 14 (42.9%) from those with willingness-to-try not responding, versus 36 out of 76 (47.3%) from
### TABLE I
SUMMARY STATISTICS

<table>
<thead>
<tr>
<th>Time Period Covered</th>
<th>3/7/2010-6/20/2010</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total Signup</td>
<td>180</td>
</tr>
<tr>
<td>w/ Willingness-To-Try</td>
<td>14</td>
</tr>
<tr>
<td>Attrition</td>
<td>75</td>
</tr>
</tbody>
</table>

|                          | Treatment | Control | Pr(|T|>|t|) |
|--------------------------|-----------|---------|-----------|
| No. of Subjects          | 48        | 57      |           |
| w/ Willingness-To-Try    | 8         | 11      |           |
| Total Hours Played, Avg. Across Players | 52.32 (119.67) | 85.83 (164.87) | 0.2440 |
| Avg. Hrs. Per Session, Avg. Across Players | 0.74 (0.53) | 1.20 (0.74) | 0.0058 |
| No. of Sessions, Average Across Players | 51.10 (107.75) | 67.53 (127.98) | 0.4834 |

**Self-Reported Survey Data**

|                          | Treatment | Control | Pr(|T|>|t|) |
|--------------------------|-----------|---------|-----------|
| Yrs. Playing MMORPG      | 5.91 (3.15) | 5.65 (2.81) | 0.6714 |
| Yrs. Playing Video Games | 13.38 (4.56) | 11.02 (3.48) | 0.0048 |
| Hours Played per Week    | 15.61 (10.93) | 14.53 (9.20) | 0.5977 |
| Happiness                | 5.36 (1.00) | 5.58 (0.94) | 0.2641 |
| Playing makes School Work Harder | 2.24 (0.74) | 2.40 (0.87) | 0.3381 |
| Health                   | 5.27 (1.39) | 5.58 (1.13) | 0.2275 |

**Academic Performance (GPA)**

|                          | Treatment | Control | Pr(|T|>|t|) |
|--------------------------|-----------|---------|-----------|
| Fall 2009                | 3.23 (0.60) | 3.14 (0.52) | 0.5675 |
| Spring 2010              | 3.19 (0.73) | 3.17 (0.49) | 0.8893 |
| Cumulative in August 2010| 3.22 (0.49) | 3.12 (0.39) | 0.4189 |

Standard deviations in parentheses. GPA based on a sub-sample of 50 students (26 treatment, 24 control) whom we were able to obtain data. Playing Makes School Harder is ranked from 1-4, 1 being “rarely” and 4 being “always”. Happiness and Health are ranked from 1-7, 7 being the highest.
TABLE II

ATTRITION RATE – VARIATION ACROSS 2X2 GROUPS

<table>
<thead>
<tr>
<th></th>
<th>(OLS)</th>
<th>(Logistic)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment Dummy</td>
<td>0.102</td>
<td>0.103</td>
</tr>
<tr>
<td></td>
<td>(0.073)</td>
<td>(0.073)</td>
</tr>
<tr>
<td>WTT Dummy</td>
<td>-0.144</td>
<td>-0.145</td>
</tr>
<tr>
<td></td>
<td>(0.103)</td>
<td>(0.097)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.387</td>
<td>***</td>
</tr>
<tr>
<td></td>
<td>(0.539)</td>
<td></td>
</tr>
<tr>
<td>N</td>
<td>180</td>
<td>180</td>
</tr>
</tbody>
</table>

*Significant at 10%, **Significant at 5%, ***Significant at 1%.

Table II regresses attrition on the 2x2 group assignments. Attrition Dummy = 1 if the potential subject never responded, = 0 otherwise. Treatment Dummy = 1 if the potential subject was assigned to the treatment group, = 0 if assigned to control group. WTT Dummy = 1 if the potential subject expressed a willingness-to-pay of $1 to try out the commitment devices during sign-up, = 0 otherwise. Coefficients in the logit regression are average marginal effects.

Within the 105 subjects, there is little difference over self-reported survey attributes between treatment and control. Treatment subjects were slightly more experienced players and played more, having spent an average of 13.38 years playing video games instead of 11.02 years, and played 15.61 hours per week versus 14.53 per week. They also reported themselves to be less happy and less healthy (5.36 versus 5.58 and 5.27 versus 5.58 respectively, out of 7). Only the difference in years playing video games is statistically significant.

I was able to obtain the academic records of a substantial subsample of subjects. Within these 50 subjects—26 in treatment, 24 in control—there was no statistically-significant difference in academic performances.\textsuperscript{11} The average GPA in the Fall

\textsuperscript{11}Given the sample size tradeoff and minimal difference between treatment and control, grades
semester/quarter of 2009 was 3.23 within the treatment group, versus 3.14 within the control group. In the Spring semester/quarter of 2010 it was 3.19 versus 3.17.

The relatively high academic performance suggests that gaming is not necessarily correlated with low academic achievement. One might argue that truly heavy players might not have seen our recruitment fliers on campus, which could give rise to self-selection bias. It would be very difficult, however, to make the parallel argument regarding Facebook advertisements. A more legitimate concern is self-selection in the formation of the subject population. As a preeminent university system, University of California admits students who performed well in high school. Students who struggled academically due to excessive gameplaying are unlikely to be admitted, yet they might well be the ones who could benefit the most from the availability of commitment devices. Nevertheless, while this issue weakens the representativeness of any observed relationship between academic performance and gameplaying in this experiment, it strengthens the argument regarding demand for commitment.

Overall, treatment subjects played less as measured by three different methods—total hours played, session length and number of sessions. Throughout the three-and-a-half month period, subjects in the treatment group played an average total of 52.32 hours, versus 85.83 hours in control. The average session length was 0.74 hours versus 1.2 hours, while the average number of sessions was 51.10 versus 67.53.

### 3.3 Usage of Commitment

To ensure that treatment subjects understand the functionality of the devices, they were required to use each type of device at least once. I drop these first usages in my analysis. Overall 38 out of the 48 treatment subjects (79%) used one of the three devices. Take-up rate is similar across willingness-to-try—of the 38, 6 indicated a willingness-to-try during signup, out of a total of 8 assigned to the treatment group (75%). Figure 3.3 on page 27 plots throughout time the usage of each device as a percentage of the total number of treatment subjects. Usage are heavily concentrated between Week 3 and 4, and dropped off significantly by Week 6. It is possible that this reflects the faltering of initial curiosity. The drop also coincides with the end of spring break, when the combination of returning to school and heavy playing during the break might have made self-control easier even without commitment.

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12Specifically, the first observed usage of each device within each subject is dropped.
This figure plots throughout time the usage of each device as a percentage of the total number of treatment subjects. In-Game represents usage of the In-Game Blocker while Pre-Game represents that of the Pre-Game Blocker. Cal. Setup represents the number of Calendar Blocks being set up, while Cal. Apply represents the number of Calendar Blocks that went into effect in the given week.

30 subjects voluntarily used the Calendar Blocker at least once, setting a median of 3 blocks and an average of 4.83. The number of calendar blocks in effect in each week is also shown in Figure 2 as Cal. Apply. 8 subjects had set up blocks that have effective dates before the time of setting. Since these blocks serve no functional purposes they are excluded from subsequent analysis. Among the remaining blocks, 35.1 percent of were set to be in effect within the same 24-hour period, while the rest were set to be effective by a median of 5.02 days and an average of 14.32 days later. Since calendar blocks need to be set up in advance, their usage might suggest that the decisions to launch a game were already time-inconsistent. For example, subjects might have anticipated cueing from friends or forum browsing. Habit formation could also be a contributing factor, as subjects could reduce their current play by making future playing prohibitively costly.

24 subjects voluntarily used the Pre-Game Blocker at least once, setting a median of 4 blocks and an average of 8.25. The median limit is 1.92 hours while the median block is 0.79 hours. While the numbers of users and uses are both lower than those of the Calendar Blocker, they were more spread-out through time—in the second half
of the experiment an average of 4.9 percent of treatment subjects used the Pre-Game Blocker, while only 0.9 percent of subjects used the Calendar Blocker. This difference was perhaps due to the prompt display of the Pre-Game Blocker every time the game started, which served as a form of reminder that the Calendar Blocker lacked in the later stages of the experiment.

10 subjects voluntarily used the In-Game Blocker at least once. All 10 subjects have also used another device voluntarily—all 10 used the Pre-Game Blocker, and 8 used the Calendar Blocker also. In all there were only 15 uses of the In-Game Device. The low utilization of In-Game Blocker was perhaps due to the continuous play-style of MMORPGs, as the constant attention required made triggering the device costly.

The evidence on overlapping commitment usage is light. Among the 38 subjects, 8 used all three devices and 10 used two. 7 of the 18 have set up blocks that have po-

| TABLE III                                                                 |
| Commitment Usage - Statistics                                           |
| Calendar Blocker                                                        |
| No. of Users 30                                                          |
| No. of Uses 164                                                          |
| Median Duration: Setup to Target (days) 0.9                               |
| Median Block Duration (days) 1                                            |
| Pre-Game Blocker                                                         |
| No. of Users 24                                                          |
| No. of Uses 171                                                          |
| Limit - Average. Within Subject                                          |
| Median, Across Subjects (hrs.) 1.92                                       |
| Average, Across Subjects (hrs.) 17.39 (50.94)                             |
| Block - Average Within Subject                                           |
| Median, Across Subjects (hrs.) 0.82                                       |
| Average, Across Subjects (hrs.) 0.89 (0.68)                              |
| In-Game Blocker                                                         |
| No. of Users 10                                                          |
| No. of Uses 15                                                           |
| Limit - Average Within Subject                                           |
| Median, Across Subjects (hrs.) 2.5                                        |
| Average, Across Subjects (hrs.) 5.05 (7.20)                              |

Standard deviations in parentheses.
potentially overlapping effects, defined as two different types of blocks coming into effect within 12 hours of each other, and both allowed for game-play before the sooner of the two blocks.\textsuperscript{13} The mean number of overlaps is 2.7 while median number of overlaps is 1. While these subjects used the commitment devices more often than the average subject (mean = 17.1, median = 15), the mean and median percentages of total usage the overlaps represent still stood at 41.9 percent and 40 percent, largely due to the high ratio of overlaps-to-usage among light users. The model in the previous section proposes that these 7 subjects, representing 14.6 percent of the treatment group, might have believed that they were vulnerable to cues. There are two alternative explanations to the behavior. First, it is possible that the subjects had simply forgotten that they had previously set up a block, though the software was designed to reduce the chance of such incidents by reminding the player of any blocks coming into effect within the same day. Second, given that the average session in the treatment group was only 0.74 hours long, a block set to be in effect 12 hours away might not be an effective one. One could argued, however, that this is effectively the same argument as cue-sensitivity. For the player to set up ahead of time the block that came into effect later, she must had anticipated a positive chance she would not be able to set up the block that would eventually came into effect earlier.

3.4 Demand for Commitment

Table IV predicts total device usage by information available at the beginning of the experiment. Only self-reported happiness is significantly correlated with number of uses in the ordinary least-square regression, with subjects who had reported they were happier before the experiment began being less likely to use the devices. This observation supports the theory that usage of commitment was a response to perceived sub-optimal playing. Moreover if overplaying decreases happiness, subjects were being happier would indicate that they were indeed playing optimally rather than being naive about their time-inconsistency.

The willingness-to-try dummy has negative coefficients in all three regressions, indicating that subjects with preference for being in treatment ended up using fewer commitments. This might be the result of these subjects having lower demand for gameplay, as well as some other subjects having developed a high demand for the

\textsuperscript{13} The number of subjects with overlaps is fairly stable to the bound on time difference—6 subjects if I lower the bound to 6 hours and 8 subjects if I raise the bound to 24 hours.
### TABLE IV
**PREDICTING DEVICE USAGE**

<table>
<thead>
<tr>
<th></th>
<th>Number of Uses (OLS)</th>
<th>Usage Dummy (OLS)</th>
<th>Usage Dummy (Logistic)</th>
</tr>
</thead>
<tbody>
<tr>
<td>WTT Dummy</td>
<td>-4.910 (4.112)</td>
<td>-0.398 ** (0.152)</td>
<td>-0.329 (0.231)</td>
</tr>
<tr>
<td>Predicted Hours Played 3/07-3/13</td>
<td>0.042 (0.269)</td>
<td>0.001 (0.010)</td>
<td>0.007 (0.009)</td>
</tr>
<tr>
<td>Self-Reported Survey Data</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yrs. Playing MMORPG</td>
<td>0.157 (0.523)</td>
<td>-0.008 (0.019)</td>
<td>-0.009 (0.017)</td>
</tr>
<tr>
<td>Yrs. Playing Video Games</td>
<td>-0.031 (0.383)</td>
<td>0.011 (0.014)</td>
<td>0.013 (0.012)</td>
</tr>
<tr>
<td>Hours Played per Week</td>
<td>-0.078 (0.273)</td>
<td>0.004 (0.010)</td>
<td>-0.001 (0.008)</td>
</tr>
<tr>
<td>Happiness</td>
<td>-3.414 ** (1.636)</td>
<td>-0.017 (0.061)</td>
<td>-0.048 (0.069)</td>
</tr>
<tr>
<td>Playing makes Sch. Work Harder</td>
<td>0.766 (2.356)</td>
<td>-0.027 (0.087)</td>
<td>-0.029 (0.060)</td>
</tr>
<tr>
<td>Health</td>
<td>0.600 (1.176)</td>
<td>0.041 (0.043)</td>
<td>0.044 (0.034)</td>
</tr>
<tr>
<td>Constant</td>
<td>22.370 (13.233)</td>
<td>0.695 (0.489)</td>
<td></td>
</tr>
</tbody>
</table>

*N* 45 45 45

*Significant at 10%, **Significant at 5%, ***Significant at 1%. Coefficients in the logistic regression are average marginal effects.

Table IV regresses two measures of device usage on information available before 3/7. *Number of Uses* is a player’s total voluntarily usage of all three devices. Usage Dummy = 1 if the player voluntarily used any of the three devices at least once, = 0 otherwise. *WTT Dummy* = 1 if the potential subject expressed a willingness-to-pay of $1 to try out the commitment devices during sign-up, = 0 otherwise. *Playing Makes Sch. Harder* is ranked from 1-4, 1 being “rarely” and 4 being “always”. *Happiness* and *Health* are ranked from 1-7, 7 being the highest. Three subjects were dropped from the regressions because they did not submit a prediction, resulting in the final observation count of 45 subjects.
commitment devices after being mandated to try them.

Five treatment subjects indicated a positive willingness-to-pay under the Becker-DeGroot-Marschak mechanism [Becker et al., 1964] conducted at the end of the experiment, for an average of $4.90. Given that BlokSet has no other use other than to block oneself from launching games, the positive willingness-to-pay manifested by these subjects suggests that they desired to use the BlokSet to limit their duration of play, which in turn implies that they believed they were suffering from overplaying. The small number of subjects (10.4 percent) with willingness-to-pay indicates that most subjects did not perceive the commitment devices to be useful.

Figure 3.4 on page 31 plots the willingness-to-pay for subjects with positive willingness-to-pay. Of the five subjects, two indicated a willingness-to-try during signup. Thus 25 percent of the subjects with a willingness-to-try has a positive final willingness-to-pay, versus 7.5 percent among those without. While the sample size is too small to draw a statistically meaningful inference, these are suggestive evidence that while most players could be correct on whether they need commitment, a non-trivial sub-population might be not.

---

14In comparison, Microsoft’s newest operating system, Windows 7, has a student price tag of $29.99, while the average price of a newly released computer game is $40. Windows 7 price as quoted on http://www.win741.com. Computer game prices are based on author’s survey of Amazon.com listing of newly released game in November.
This figure plots throughout time the average number of hours played in a given week. *Treat* represents the treatment group while *Ctrl* represents the control group. *p25* is the 25th percentile while *p75* is the 75th percentile.

### 3.5 Effects on Duration of Play

Figure 3.5 on page 32 plots the weekly hours played by the median, the 25th percentile and the 75th percentile subject in each group. The median treatment subject played on average 4.75 hours per week, while the median control subject played 9.29. Although treatment subjects played less than control in all three percentiles, the differences between the 75th and 25th percentiles within each group (16.03 and 17.71 respectively) indicate a high degree of variance. There is a general decline in hours played throughout the experiment. “Loss of interest” is the most common explanation being given by subjects when surveyed on why they stopped playing.

Table V compares duration-of-play measurements of the treatment group versus the control group, controlling for self-reported game-play characteristics and campus-affiliation. Subjects in the treatment group played significantly shorter duration, as measured by either total number of hours played (column 1) or hours played in a session (column 5). The estimated reduction in total hours played is 33.53 percent of the average figure of control subjects, while the reduction in mean session length is 39.95 percent. The change in number of sessions played (column 3), on the other hand,
is not significant. Consistent with the model in the previous section, this suggests that players who used the commitment devices believed that they might play with suboptimal duration, but not suboptimal frequency.

The second set of regressions breaks up the effects according to willingness-to-try. Control subjects without willingness-to-try serves as the baseline case. Treatment Dummy now captures the average difference being a treatment subject without willingness-to-try, while WTT Dummy captures the difference as a control subject with willingness-to-try. Adding the two dummies with the interaction term gives us the difference for treatment subjects with willingness-to-try. Just as one would expect, the estimated coefficients suggest that all three on average played less than control subjects without willingness-to-try. What is perhaps more surprising though is that the estimated difference is smallest for treatment subjects with willingness-to-try. Estimated reduction in total number of hours played (column 2) is 41.7 for treatment subjects without willingness-to-try, 46.3 for control subjects with willingness-to-try, but only 21.7 for treatment subjects with willingness-to-try. While I cannot reject the hypothesis that the estimated differences are identical, treatment subjects with willingness-to-try having the smallest difference is consistent with the story that willingness-to-try is a proxy for sophisticated players, who voluntarily play less in the absence of a commitment device. This is supported by the estimated differences in number of sessions played (column 4)—the hypothesis that treatment subjects with willingness-to-try are playing equal number of sessions as control subjects with willingness-to-try is rejected ($F = 15.32, p = 0.011$). Thus treatment subjects with willingness-to-try might be playing shorter sessions, but they were certainly playing more sessions. This is what one would expect from sophisticated players—with commitment, overplaying is no longer a threat, which allows these players to play on occasions which they believe they would otherwise overplay.

### 3.6 Prediction

Nine subjects had not yet installed their assigned software package for the week they were told to predict hours of play and are thus excluded. Figure 3.6 on page 35 plots subjects’ self-predicted durations of play against their actual durations of play. There was significant overestimation. The mean difference between predicted and actual durations is 4.72 hours ($T = 3.30, p = 0.001$). Given the average actual duration of play is 9.29 hours, this represents a 50.1 percent overestimation. The overestimation
Table V regresses three measures of duration-of-play on 2x2 group assignments, interactions and self-reported survey data. Treatment Dummy = 1 if the potential subject was assigned to the treatment group, = 0 if assigned to control group. WTT Dummy = 1 if the potential subject expressed a willingness-to-pay of $1 to try out the commitment devices during sign-up, = 0 otherwise.

<table>
<thead>
<tr>
<th></th>
<th>Total Hrs. Played</th>
<th>Session Count</th>
<th>Mean Session Length (hrs.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Treatment Dummy</td>
<td>-28.781 **</td>
<td>-41.692 ***</td>
<td>-9.208</td>
</tr>
<tr>
<td></td>
<td>(13.083)</td>
<td>(13.434)</td>
<td>(10.455)</td>
</tr>
<tr>
<td>WTT Dummy</td>
<td>-46.260 **</td>
<td>-35.251 ***</td>
<td>-0.454</td>
</tr>
<tr>
<td></td>
<td>(11.740)</td>
<td>(7.741)</td>
<td>(0.371)</td>
</tr>
<tr>
<td>Treatment Dummy</td>
<td>66.294 **</td>
<td>57.874 ***</td>
<td>0.334</td>
</tr>
<tr>
<td>* WTT Dummy</td>
<td>(22.284)</td>
<td>(8.035)</td>
<td>(0.540)</td>
</tr>
</tbody>
</table>

Self-Reported Survey Data

<p>| | | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Yrs. Playing MMORPG</td>
<td>-3.293</td>
<td>-3.504</td>
</tr>
<tr>
<td></td>
<td>(2.264)</td>
<td>(1.978)</td>
</tr>
<tr>
<td>Yrs. Playing Video Games</td>
<td>2.306</td>
<td>2.667</td>
</tr>
<tr>
<td></td>
<td>(2.286)</td>
<td>(2.004)</td>
</tr>
<tr>
<td>Hours Played per Week</td>
<td>3.401 *</td>
<td>3.247</td>
</tr>
<tr>
<td></td>
<td>(1.592)</td>
<td>(1.634)</td>
</tr>
</tbody>
</table>

$N$ | 105 | 105 | 105 | 105 | 105 | 105

Mean – Treatment | 52.32 | 51.10 | 0.74
Mean – Control   | 85.83 | 67.53 | 1.20

*Significant at 10%, **Significant at 5%, ***Significant at 1%. All regressions controlled for campus fixed-effects and all standard errors adjusted for campus clusters.
was driven by a large number of subjects who had predicted positive durations of play but ended up not playing. The overestimation was insignificant without those subjects (mean = 1.90, $T = 1.01$, $p = 0.315$). The difference persists even if one compares the prediction with average hours played per week from March 7th–March 27th (mean = 6.15, $T = 4.55$, $p = 0.000$), so the overestimation does not seem to be due to subjects using average duration of play as a heuristic for prediction.

One might hypothesize that the availability of commitment allowed some treatment subjects to commit to not playing, thereby causing what seems to be an overestimation. On surface this was not the case, as the difference is significant both within the control group (mean = 3.64, $T = 2.1$, $p = 0.041$) and the treatment group (mean = 5.98, $T = 2.53$, $p = 0.015$). Table VI regresses the prediction error—the absolute difference between actual and predicted hours of play—on predicted hours of play and self-reported play characteristics. The regression shows that subjects who predicted a longer duration of play made bigger errors. Furthermore, prediction error decreases with neither self-reported experience in gaming nor actual duration of play.

What could have driven the overestimation? It is possible that the subjects simply had no idea on how long they were going to play. It is also possible that subjects had access to more than one computer and reported duration of play on all computers,
TABLE VI
PREDICTION ERROR – OLS REGRESSION

<table>
<thead>
<tr>
<th>Actual Hrs. Played – Prediction</th>
<th>Treatment Dummy * Prediction</th>
<th>Control Dummy * Prediction</th>
<th>Treatment Dummy</th>
<th>Self-Reported Survey Data</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.664 **</td>
<td>0.208 ***</td>
<td>-4.029</td>
<td>Yrs. Playing MMORPG</td>
<td>97</td>
</tr>
<tr>
<td></td>
<td>(0.254)</td>
<td>(0.051)</td>
<td>(3.420)</td>
<td>0.094</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.287)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Yrs. Playing Video Games</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.075</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.262)</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Hours Played per Week</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>0.135</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.243)</td>
<td></td>
</tr>
</tbody>
</table>

*Significant at 10%, **Significant at 5%, ***Significant at 1%. All regressions controlled for campus fixed-effects and all standard errors adjusted for campus clusters.

This table regresses the players’ prediction error on treatment assignments, interactions with player-reported predicted hours and survey data. Actual Hrs. Played is the recorded number of hours the subject played in the week of 3/07-3/13. Prediction is predicted hours of play submitted by the subject for the same week. Treatment Dummy = 1 if the subject was assigned to the treatment group, = 0 if assigned to control group. Control Dummy = 1 – Treatment Dummy.

even though I specifically asked them to predict usage on the computer I was monitoring. Finally, subjects might have reported a prediction that was larger than their actual prediction, so as to avoid being observed as overplaying. The crucial question is how accurate are findings based on non-incentivized, self-reported data from players, which almost all existing studies of the effect of video-gaming relied on. If players reported a lower than actual duration of play, the estimates in existing studies would be biased towards reporting large effects. Given that subjects overestimated duration of play by over 50 percent, this bias could potentially be very large. And while an incentivized solicitation could mitigate the problem, it also has the downside of potentially downside of being used by the subjects as a commitment device.
4 Conclusion

To be written.

References


Appendix

Lemma 1. The player never chooses to play in Period 4.

Proof. The player chooses to play in Period 4 if

\[ \alpha + \epsilon - \beta C(n_3 + 1) \geq -\beta \delta C(n_3) \quad (4.1) \]
\[ \alpha + \epsilon > \beta c(n_3 + 1) \quad (4.2) \]

Assumption 2 thus rules out the player from choosing to play in this period. \(\square\)

Proposition 1. \(x_t\) is strictly increasing in \(\beta\).

Proof. First solve the model with backward induction.

**Period 3.** Because the player would never voluntarily choose to play in Period 4, the player only needs to take into consideration the probability of overplaying due to cues, and thus

\[ x_3(n_2, \beta, \hat{p}, \epsilon) = \beta \left[ \hat{p}c(n_2 + 2) + (1 - \hat{p})c(n_2 + 1) \right] - \alpha \quad (4.3) \]

Since the Period 3 threshold does not depend on \(\epsilon\), from now on I drop \(\epsilon\) from the argument list from \(x_3\). Taking derivatives with respect to \(\beta\),

\[ \frac{\partial x_3}{\partial \beta} = \hat{p}c(n_2 + 2) + (1 - \hat{p})c(n_2 + 1) > 0 \]

**Period 2.** The expected utility of choosing to play is

\[ E[U_2|a_2 = 1] = u_2(1) + \beta \{ \hat{q}_3^0 [\hat{p} (u_3(1) + u_4(1) - C(3)) + (1 - \hat{p}) (u_3(1) - C(2))] \]
\[ + (1 - \hat{q}_3^0) [\hat{p} (u_4(1) - C(2)) + (1 - \hat{p}) (-C(1))] \} \quad (4.4) \]

where \(\hat{q}_3^{n_2} = 1(\epsilon > \hat{x}_3^{n_2}) + [1 - 1(\epsilon > \hat{x}_3^{n_2})] \hat{p}\) is the perceived probability of playing in Period 3. The expected utility of not choosing to play is

\[ E[U_2|a_2 = 0] = \beta \{ \hat{q}_3^0 [\hat{p} (u_3(1) + u_4(1) - C(2)) + (1 - \hat{p}) (u_3(1) - C(1))] \]
\[ + (1 - \hat{q}_3^0) \hat{p} [u_4(1) - C(1)] \} \quad (4.5) \]
so
\[
x_2(0, \beta, \hat{p}, \epsilon) = \frac{\beta \{ \hat{q}_3^1 [\hat{p}c(3) + (1 - \hat{p})c(2)] + (1 - \hat{q}_3^0) [\hat{p}c(2) + (1 - \hat{p})c(1)] \}}{1 + \beta(\hat{q}_3^3 - \hat{q}_3^0)} - \alpha \quad (4.6)
\]

Because \( \hat{q}_3^{n2} \) does not depend on \( \beta \), \( x_2(0, \beta, \epsilon) \) is differentiable over \( \beta \) and we can take derivative,
\[
\frac{\partial x_2}{\partial \beta} = \left\{ \hat{q}_3^1 [\hat{p}c(3) + (1 - \hat{p})c(2)] + (1 - \hat{q}_3^0) [\hat{p}c(2) + (1 - \hat{p})c(1)] \right\}
\times \left\{ \frac{1}{1 + \beta(\hat{q}_3^3 - \hat{q}_3^0)} - \frac{\beta(\hat{q}_3^3 - \hat{q}_3^0)}{[1 + \beta(\hat{q}_3^3 - \hat{q}_3^0)]^2} \right\} \quad (4.7)
\]
\[
= \left\{ \hat{q}_3^1 [\hat{p}c(3) + (1 - \hat{p})c(2)] + (1 - \hat{q}_3^0) [\hat{p}c(2) + (1 - \hat{p})c(1)] \right\}
\times \left[ \frac{1}{1 + \beta(\hat{q}_3^3 - \hat{q}_3^0)} \right]^2 \quad (4.8)
\]
The first part of (4.8) is the expected cost of playing in Period 2, which is positive, so \( \partial x_2/\partial \beta > 0 \).

**Proposition 2.** \( x_3 \) is strictly increasing in \( \hat{p} \) and does not change with \( \hat{\beta} \). For \( x_2 \), there exist an \( \bar{\epsilon} \) such that \( x_2 \) is strictly increasing in \( \hat{p} \) and weakly increasing in \( \hat{\beta} \) if \( \epsilon < \bar{\epsilon} \). If \( \epsilon > \bar{\epsilon} \), \( x_2 \) is strictly decreasing in \( \hat{p} \) and \( \hat{\beta} \) at a finite set of points for every \( \epsilon \), but is strictly increasing in \( \hat{p} \) and invariant in \( \hat{\beta} \) otherwise. The finite set of points is given by \( \{(\hat{p}, \hat{\beta})| \epsilon = \hat{x}_3^0 \text{ or } \epsilon = \hat{x}_3^1\} \).

**Proof.** For \( x_3 \), (4.3) does not depend on \( \hat{\beta} \). As for \( \hat{p} \), take derivative of (4.3),
\[
\frac{\partial x_3}{\partial \hat{p}} = \beta [c(n_2 + 2) - c(n_2 + 1)] > 0
\]

For \( x_2 \), because \( \hat{q}_3^{n2} \) depends on \( \hat{p} \) and \( \hat{\beta} \), it is not differentiable over the two and the analysis has to be done piecewise. Suppose \( \epsilon > \hat{x}_3^n \) and denote this set of \( \epsilon \) as \( \epsilon_h \). Since \( \hat{x}_3^n > \hat{x}_3^{n'} \) for \( n > n' \), \( \hat{q}_3^n \leq \hat{q}_3^{n'} \) and \( \hat{q}_3^1 = 1, \hat{q}_3^0 = 1 \). (4.6) becomes
\[
x_2(0, \beta, \hat{p}, \epsilon_h) = \beta [\hat{p}c(3) + (1 - \hat{p})c(2)] - \alpha \quad (4.9)
\]
It is apparent that \( \partial x_2(0, \beta, \hat{p}, \epsilon_h)/\partial \hat{p} > 0 \) and \( \partial x_2(0, \beta, \hat{p}, \epsilon_h)/\partial \hat{\beta} = 0 \).

If the player believes instead she is going to choose to play in Period 3 only if she
Furthermore it is easy to see that

**Proof.**

Once again it is apparent that

\[
\frac{\partial x_2(0, \beta, \hat{\beta}, \epsilon_m)}{\partial \hat{\beta}} > 0 \text{ and } \frac{\partial x_2(0, \beta, \hat{\beta}, \epsilon_1)}{\partial \hat{\beta}} = 0. \]

However since \( \frac{\hat{\beta}}{1 + \beta (\hat{\beta} - 1)} \leq 1 \), \( x_2(0, \beta, \hat{\beta}, \epsilon_h) \geq x_2(0, \beta, \hat{\beta}, \epsilon_m) \). So if \( \epsilon = \hat{x}_3 \) and \( \hat{\beta} \) or \( \hat{\beta} \) increases, \( x_2 \) weakly decreases.

\[
x_2(0, \beta, \hat{\beta}, \epsilon_m) > x_3(0, \beta', \hat{\beta}) \text{ gives}
\]

\[
\frac{\hat{\beta} \cdot \beta / \beta'}{1 + \beta (\hat{\beta} - 1)} > \frac{\hat{\beta} c(2) + (1 - \hat{\beta}) c(1)}{\hat{\beta} c(3) + (1 - \hat{\beta}) c(2)}
\]

(4.11)

Notice that (4.11) always holds when \( \beta = \beta' = 1 \) and never holds when \( \hat{\beta} = 0 \).

Lastly, denote the set of \( \epsilon < \hat{x}_3 \) as \( \epsilon_1 \). Because \( q_{\hat{3}}^{1} = \hat{\beta} \forall n_2 \),

\[
x_2(0, \beta, \hat{\beta}, \epsilon_1) = \beta \{ \hat{\beta} [\hat{\beta} c(3) + (1 - \hat{\beta}) c(2)] + (1 - \hat{\beta}) [\hat{\beta} c(2) + (1 - \hat{\beta}) c(1)] \} - \alpha
\]

(4.12)

Once again it is apparent that \( \partial x_2(0, \beta, \hat{\beta}, \epsilon_1) / \partial \hat{\beta} > 0 \) and \( \partial x_2(0, \beta, \hat{\beta}, \epsilon_1) / \partial \hat{\beta} = 0 \). Furthermore it is easy to see that \( x_2(0, \beta, \hat{\beta}, \epsilon_1) \geq x_2(0, \beta, \hat{\beta}, \epsilon_1) \) and \( x_2(0, \beta, \hat{\beta}, \epsilon_1) \geq x_3(0, \beta, \hat{\beta}) \).

\[
x_2(0, \beta, \epsilon_1) > x_2(0, \beta, \epsilon_m) \text{ gives}
\]

\[
\frac{\beta \hat{\beta}}{1 + \beta (\hat{\beta} - 1)} < \frac{\hat{\beta} c(2) + (1 - \hat{\beta}) c(1)}{\hat{\beta} c(3) + (1 - \hat{\beta}) c(2)}
\]

(4.13)

By (4.11) \( x_2(0, \beta, \hat{\beta}, \epsilon_1) < x_2(0, \beta, \hat{\beta}, \epsilon_m) \) when \( x_2(0, \beta, \hat{\beta}, \epsilon_m) > x_3(0, 1, \hat{\beta}) \). Since the transition from \( \epsilon_m \) to \( \epsilon_1 \) depends on \( \hat{x}_3^0, \hat{\epsilon} \) corresponds to \( x_3(0, \beta, \hat{\beta}) \), where \( \hat{\beta} \) and \( \hat{\beta} \) satisfies \( x_2(0, \beta, \hat{\beta}, \epsilon_1) = x_3(0, 1, \epsilon_m) \). For the same reasoning as the case of \( \epsilon = \hat{x}_3 \) above, \( x_2 \) weakly decreases if \( \epsilon > \hat{\epsilon} \) and \( \epsilon = \hat{x}_3^0 \). Thus only if \( \epsilon < \hat{\epsilon} \) will \( x_2 \) be weakly increasing in \( \hat{\beta} \) and \( \hat{\beta} \).

**Proposition 3.** \( x_2(0, \beta, \hat{\beta}, \epsilon) = x_3(1, \beta, \hat{\beta}, \epsilon) \), and if \( \epsilon > x_2(0, \beta, \hat{\beta}, \epsilon) \) then \( \epsilon > x_3(0, \beta, \hat{\beta}, \epsilon) \).

**Proof.** When \( \epsilon \in \epsilon_h \), (4.9) \( x_2(0, \beta, \hat{\beta}, \epsilon_h) = x_3^1 \). Since \( x_3^1 > x_3^0 \), this proves the first part of the theorem. Furthermore, \( \epsilon > x_2(0, \beta, \epsilon) \) implies \( \epsilon > x_3(0, \beta) \) if \( \epsilon \in \epsilon_h \).
When \( \epsilon \in \epsilon_m \), if (4.11) holds for \( \beta' = \beta \) then \( \epsilon > x_2(0, \beta, \hat{p}, \epsilon) \) implies \( \epsilon > x_3(0, \beta, \hat{p}) \). When (4.11) does not hold for \( \beta' = \beta \), there exists a range of \( \epsilon \) where \( x_2(0, \beta, \hat{p}, \epsilon_m) < \epsilon < x_3(0, \beta, \hat{p}, \epsilon_m) \). However, since \( x_3(0, \beta, \hat{p}, \epsilon_m) < x_3(0, \hat{\beta}, \hat{p}, \epsilon_m) = \hat{x}_3 \), any \( \epsilon \) in this range is also smaller than \( \hat{x}_3 \), which is outside \( \epsilon_m \).

Lastly when \( \epsilon \in \epsilon_1 \), it is easy to see from (4.12) that \( x_2(0, \beta, \hat{p}, \epsilon_h) \geq x_2(0, \beta, \hat{p}, \epsilon_1) \) and \( x_2(0, \beta, \hat{p}, \epsilon_1) \geq x_3(0, \beta, \hat{p}) \). So \( \epsilon > x_2(0, \beta, \hat{p}, \epsilon) \) implies \( \epsilon > x_3(0, \beta, \hat{p}) \) if \( \epsilon \in \epsilon_1 \). This proves the second part of the theorem.

Proposition 4. \( E_t[U_t|\hat{\beta}, \hat{p}, z_t] \) is strictly decreasing in \( \hat{p} \) for \( \tau \geq t \) and weakly increasing in \( \hat{\beta} \) for \( \tau = t \). Moreover, the change is weakly larger for \( z_t = 1 \).

Proof. Strictly decreasing in \( \hat{p} \): If \( \epsilon \neq \hat{x}^{n_t-1}_3 \), by 2 the number of periods the player believes she would choose to play does not change with a small change in \( \hat{p} \). Her perceived probability of overplaying, on the other hand, strictly increases with \( \hat{p} \), and so \( E_t[U_t|\hat{\beta}, \hat{p}] \) is strictly decreasing in \( \hat{p} \). At \( \epsilon = x^{n_t-1}_3 \), \( E_t[U_t|\hat{\beta}, \hat{p}] \) does not change with \( a_\tau \), so again \( E_t[U_t|\hat{\beta}, \hat{p}] \) is strictly decreasing in \( \hat{p} \).

Weakly increasing in \( \hat{\beta} \): By assumption the player never chooses to play in Period 4, so \( E_3[U_3|\hat{\beta}, \hat{p}] \) does not depend on \( \hat{\beta} \). For \( E_t[U_t|\hat{\beta}, \hat{p}] \) where \( t < 3 \), suppose the player currently perceive her future action to be suboptimal. As \( \hat{\beta} \) increases, eventually the player would perceive her future action to be identical to that of a time-consistent player, which is optimal for any future period. Thus \( E_t[U_t|\hat{\beta}, \hat{p}] \) is weakly increasing in \( \hat{\beta} \).

Change larger for \( z_t = 1 \): If \( z_t = 0 \), the maximum of periods the player can play is at least one fewer than if \( z_t = 1 \). An increase in \( \hat{p} \) therefore decreases marginal utility in at least one fewer period when \( z_t = 0 \). Similarly for \( \hat{\beta} \), with more periods the deviation from time-consistent strategy in future is weakly larger, so the change is weakly larger when \( z_t = 1 \).

Proposition 5. (Usage of Commitment)

- A naive player never uses a commitment device.
- A \( \beta \)-sophisticated player would use I in Period \( t \) if \( (\alpha_{t+1} + \epsilon)a_{t+1} - C(n_t + 1) < 0 \) and \( (\alpha_{t+1} + \epsilon)a_{t+1} - \hat{\beta}C(n_t + 1) > 0 \). She would use \( X \) in Period \( t \) if \( X \) is the only commitment device available and \( \exists x > 0 \) s.t. \( E_t[U_t|X_t = x] > E_t[U_t|X_t = 0] \).
A p-sophisticated player would always use X. She would use I in Period t if 
\((\alpha_{t+1} + \epsilon)a_{t+1} - C(n_t + 1) < 0 \) and X is not coming into effect in \( t + 1 \).

**Proof. Naive player.** Since the naive player never forsees herself playing suboptimally in the future, she never uses any commitment by the assumption that a player would not use a commitment if she is indifferent.

**\( \beta \)-sophisticated player.** \((\alpha_{t+1} + \epsilon)a_{t+1} - C(n_t + 1) < 0 \) and \((\alpha_{t+1} + \epsilon)a_{t+1} - \hat{\beta}C(n_t + 1) > 0 \) is the condition which a \( \beta \)-sophisticated player would perceive herself over-playing in the next period. Setting \( I_t = 1 \) thus allows her to commit to the optimal duration of play, which is also the reason why X would not be used as a response to present-biasness if I is available. For X, \( \exists x > 0 \) s.t. \( E_t[U_t|X_t = x] > E_t[U_t|X_t = 0] \) is the condition in which an expected-utility increasing limit exists.

**p-sophisticated player.** Setting X to block playing in Period 4 is always optimal. \((\alpha_{t+1} + \epsilon)a_{t+1} - C(n_t + 1) < 0 \) is the condition for future playing to be suboptimal. Because the player perceives herself to overplay with at least probability \( \hat{p} \), it is optimal to block playing in the next period if a block is not already in place.