A Check for Rational Inattention

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Abstract

Who is rationally inattentive, and in what situations? Models of rational inattention allow agents to make mistakes in their actions, but assume they optimize their allocation of attention. Using millions of online chess moves, I test this assumption, by comparing the marginal benefits (better moves) and marginal costs (less time for future moves) of attention. I find that high-skilled players equalize marginal benefit and cost, but low-skilled players have higher marginal cost, i.e. they spend too long on moves. I also find that having less time leads to deviations from rational inattention. A simple intervention improves players' attention allocation.

Keywords: attention allocation, deterministic games, cognitive costs

JEL Codes: D83, D91, C72

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> Most decisions should probably be made with somewhere around 70% of the information you wish you had. If you wait for 90%, in most cases, you're probably being too slow.
>
> Jeff Bezos, '2016 Letter to Shareholders"

Rational inattention is an increasingly-popular tool in economics. It allows for agents to regularly make mistakes while maintaining the ability to calculate welfare and do counterfactual analysis. The key assumption is that agents allocate attention optimally. I investigate when this assumption is appropriate. Does rational inattention come naturally, or is it a skill? More generally, for which decision-makers and in what situations is attention allocated rationally?

I study rational inattention in online chess, where players face a trade-off between using time to make a better move and losing time for future moves.[1] By measuring the marginal benefit and marginal cost, I test whether agents optimally allocate their attention.[2] For skilled players and players with sufficient time, I am unable to reject that the marginal benefit equals marginal cost. But for unskilled players when time is limited, I find evidence that marginal benefit is below marginal cost. In other words, by spending too long when they have little time, unskilled players do not adjust their attention as much as a rationally-inattentive agent.

Testing whether attention is allocated optimally is difficult. First, it is unusual to observe direct measures of attention. Second, economists do not often have a way to evaluate whether an agent made the "right" choice, and even more rarely observe how bad the choice was. Third, empirical tests often require an exogenous change in the attention cost. Finally, if allocating attention is a skill, economists would like a setting in which agents have seen similar problems repeatedly, but still regularly make mistakes.

Chess meets all these criteria. The game is played with clocks, so an economist can observe how long a player spent choosing their move. Computer engines are significantly better than humans and are commonly used to evaluate moves. Time controls vary, giving plausibly exogenous variation in attention.[3] Finally, online chess attracts both casual players and the best players in the world.

[1] Chess is played with a time limit. In the games I focus on, the time limit applies to all of the moves, so there is a tradeoff between allocating time to the current move versus future moves. Online chess is fast compared to classical chess, which readers may be familiar with due to the Queen's Gambit on Netflix. This paper focuses on games lasting under 30 minutes.

[2] Throughout the paper, I assume that attention can be measured by the time it takes to make a move. If the reader is unwilling to buy that assumption, they may interpret this paper as testing whether agents can optimally allocate their time, rather than their attention.

[3] A time control is a limit on how long each player has to make all their moves in the game. If they run out of time before the game ends on the board, they lose.

I use millions of moves played on Lichess, one of the leading online chess websites, to test whether players optimally allocate attention. Because the setting is so rich, I go beyond simple tests of whether attention is valuable or whether attention is strategically allocated. Rather, I directly test whether the marginal benefit of attention, making better moves, is the same as the marginal cost of losing time for future moves, on average.

The test requires estimating the value function of the players. I use local linear regression to estimate the empirical probability of a victory based on the time remaining for each player and a strong computer engine's evaluation of the position. This procedure also calculates the marginal cost of spending additional time. Then, matching moves across time controls, I calculate the marginal benefit of attention using the time control as an instrumental variable.

In several of the shorter time controls, I reject the hypothesis that the average marginal cost and average marginal benefit are equal. Players in these time controls have a lower marginal benefit than marginal cost. Digging into these results, I reject the hypothesis of equalized marginal benefit and marginal cost specifically for unskilled players or when there is little time remaining in the game. Unskilled players spend too long on moves, across a variety of time controls. For the best players, I am unable to reject the optimality condition in any time control.[4] Similarly, players spend too long thinking when time is already scarce.

To relate these findings to the psychology literature, one could say the inexperienced chess players suffer from "decision paralysis" (Iyengar and Lepper, 2000; Anderson, 2003; Mick et al., 2004), in which additional choices can lead to status quo bias or delayed decision-making. In this setting, the decision-makers are faced with a complex choice and are unable to decide as quickly as would be optimal. The psychology literature mostly focuses on regret to examine whether the decision-making is "correct" (e.g. Gourville and Soman, 2005), unlike in this setting where I can measure the marginal cost of time.

To provide complementary evidence that some players are not optimizing their allocation of attention, I show that a simple intervention by Lichess when the clock gets low—a small beep and changing the color of the clock—lowers the amount of time spent on moves and raises the average mistake size. This result is consistent with players adjusting their strategy to better optimize attention. Unskilled players react more strongly.

One thing to note about the interpretation of attention costs is that I measure attention using the time spent on a move, which is appropriate in a fast online chess game where attention is likely undivided.[5] In chess, the primary cost of attention is the opportunity cost

[4] This is not the only possible test of rational inattention, but this test is one for which the assumptions necessary to test rational inattention are defensible and for which there is statistical power. It could be that even if this test were not falsified, there are other dimensions on which agents are not allocating attention optimally.

[5] Caplin et al. (2020) show in a rational inattention framework that the attention paid to a decision is

of time for future moves. In contrast, many rational inattention models have a fixed abstract attention cost. One interpretation in those models is that attention has an opportunity cost of not allocating attention to unmodeled future decisions, which corresponds to my framework. Another interpretation is that agents get direct disutility from having to process information, which makes less sense in a game people play for fun. I discuss further in Section 4.4.

One of this study's strengths is that it focuses on a choice being made in its natural environment, but the setting raises the question of external validity. Of course, making chess decisions is different than making other decisions, but like many decisions, chess decisions are made under time pressure in a complex environment where agents know many things but have far from a complete understanding of it. For example, a stock trader in a quickly-changing environment would also be a similar situation, where they need to trade off attention between sequential decisions. So would a teacher grading a large number of papers in a constrained amount of time.

In terms of selection, the sample is highly selected: the only people used for analysis are people that are choosing to play chess online, and who are analyzing their games in multiple time controls. Because I find that the less good players are the ones that are failing to optimally allocate time, we can reasonably expect that the deviations from rational inattention would be even larger for the general population.

In the end, the main contribution of the paper is to show that theories of rational inattention are not universally applicable in terms of its quantitative predictions, but that such theories work well for experts or when there is plenty of time. For the claim that rational inattention is not universally applicable, this paper is only the second—to my knowledge—to test such hypotheses in the field (Bronchetti et al., 2020, which utilizes a very different setting and methods).[6] For the claim that rational inattention is more applicable for experts, the appropriateness will depend on the setting, but this paper is the first that I am aware of to directly compare rational inattention by people who are skilled and unskilled. It will be valuable to see whether the hypothesis can extend to other situations in future research.

correlated to the time taken. Rubinstein (2013) and Chabris et al. (2009) both use time as a proxy for attention in laboratory settings, where they show that having less time leads to mistakes and more high-stakes decisions take more time, respectively.

[6] A new paper (La Nauze and Myers, 2023) studies the willingness to pay for information and compares it to the value of that information, finding very little correlation.

Related Literature

Rational inattention was first used in economics by Sims (2003).[7] Many recent papers have derived empirical predictions of rational inattention (Matějka, 2015; Fosgerau et al., 2020; Caplin et al., 2016, 2019). And many papers have now tried to test some of these predictions in the lab (Caplin and Dean, 2015; Dean and Neligh, 2019; Dewan and Neligh, 2020; Bronchetti et al., 2020). See Section 4.4 of Maćkowiak et al. (2020) for a review. Of note, Gabaix et al. (2006) investigates the trade-off of spending time on different decisions in a laboratory.

Increasingly, economists have looked outside the laboratory for evidence of rational inattention. There is a wide variety of settings: firm returns and mutual funds (Cohen and Frazzini, 2008; Menzly and Ozbas, 2010; Kacperczyk et al., 2016), baseball (Phillips, 2017; Bhattacharya and Howard, forthcoming; Archsmith et al., 2021), online finance behavior (Mondria et al., 2010; Sicherman et al., 2016; Olafsson et al., 2018), rental search (Bartoš et al., 2016), online shopping (Taubinsky and Rees-Jones, 2018; Morrison and Taubinsky, 2020), health insurance (Brown and Jeon, 2020), migration (Porcher, 2020), and forecasting (Coibion et al., 2018; Gaglianone et al., 2020; Xie, 2019). The general finding is that agents are allocating attention in strategic ways.[8]

For most of these papers outside the laboratory, finding evidence of rational inattention is twofold: first, establish a role for attention, i.e. "when attention costs change, does that affect the quality of decisions?;" and second, show that attention is allocated on a rational basis, i.e. "when the stakes increase, do people pay more attention?"[9] Sometimes these are combined into "are better decisions made when the stakes are high?"

The test I propose in this paper is not aimed at whether agents are behaving strategically. Unlike other applications, I empirically measure both the benefit and cost of attention. With that measurement, I test not whether people adjust attention, but whether they adjust optimally. This is important if we want to take the quantitative implications of rational

[7]The idea of agents optimizing their decision process predates rational inattention. One related paper is Lipman (1991), which notes that it seems inconsistent to consider agents that cannot optimize their actions but can optimize their decision procedures. This paper is providing empirical evidence on this question. (Note that Lipman (1991) argues theoretically that one can instead consider agents as using the optimal decision procedure under different priors.)

[8]Olafsson et al. (2018), La Nauze and Myers (2023), and Bronchetti et al. (2020) find interesting deviations from rational inattention, with Olafsson et al. (2018) showing that some types of information are directly utility-enhancing, Bronchetti et al. (2020) showing that agents undervalue information relative to the rational inattention prediction, and La Nauze and Myers (2023) showing that the value of information and the willingness to pay for it are barely correlated. These results are consistent with my findings regarding unskilled chess players, who do change their allocations of attention across time controls, but not as much as they would if they were optimizing their allocation.

[9]These are the tests I perform in Appendix A.

inattention seriously, such as in many macro models (e.g. Sims, 2003, and many others).

The findings regarding skill shed light on when rational inattention is an appropriate theory. Maćkowiak et al. (2020) defend rational inattention because agents that make repeated decisions figure out which information to pay attention to. They give the example of a driver being rational in processing traffic signals because driving is a regular activity, but they hypothesize the same driver would be less rationally-inattentive if the car were to spin out because that is a new situation. This paper confirms the chess analogue of that hypothesis by showing that skilled chess players are better at allocating attention.

The results on skill also speak to the literature that takes place in the laboratory. Rejecting rational inattention for a decision in which test subjects are inexperienced or have little time may not be informative for whether they would be rationally inattentive in situations where they have more experience or time.

I also contribute to the literature on whether experience reduces behavioral anomalies. This literature began with List (2003) and a discussion can be found in DellaVigna (2009). In very related work, Palacios-Huerta and Volij (2009) and Levitt et al. (2011) look at experienced chess players' ability to conduct backward induction and compare them to the general population.

This paper also contributes to a literature using chess to explore other economic and psychological phenomena, from gender's effect on strategic decisions (Gerdes and Gränsmark, 2010; Smerdon et al., 2020) to risk-taking (Dreber et al., 2013; Holdaway and Vul, 2021) to level-k thinking (Biswas and Regan, 2015) to the roles of instinct (Burns, 2004) to the role of complexity in choice (Salant and Spenkuch, 2022) to the effect of masks on cognition (Smerdon, 2022). This paper is also similar to Romer (2006) who estimates a value function to judge decision-making in football.

1 Setting and Data

Chess is a deterministic game, but the number of possibilities is extensive, and chess has not been solved. Still, computers are much better than humans. In fact, professional chess players use computers extensively in their preparation for games against other humans. And computers are an important tool for humans to evaluate their play and identify mistakes.

The most ubiquitous computer engine is Stockfish, and it is amongst the strongest in the world. If Stockfish sees all the way to the end of the game, it might evaluate the position as "checkmate for black in 4 moves." More commonly, Stockfish evaluates the position using pawn equivalents, e.g. "white is winning by 0.92 pawns."[10]

[10]A rough way to evaluate a position–commonly used by chess players without access to an engine–is to

Lichess is one of the most popular chess websites, and millions of games are played there each month, by novices and the world's best chess players (including Ding Liren, the world champion). Lichess games are available for download (Duplessis, 2017). This includes each move, the time remaining after each move, the players, the player's Elo rating,[11] the time control, and the result. For some games, the data include Stockfish's evaluation of each position.[12]

I use data from April 2017, which includes about 68 million moves with the computer evaluation. Throughout the paper, I focus on moves in seven of the most popular time controls: 15 second, 30 second, 1 minute, 3 minute, 5 minute, 10 minute, and 15 minute.[13] Together these make up a majority of the moves on Lichess. I only include games that are rated and not part of a tournament. Summary statistics are presented in Table 1.

The setting selects for people interested in chess. Beyond the obvious reason that no one has to play chess, the setting also selects for players who review their games. My empirical strategy further selects for players that play and analyze enough games in multiple time controls such that I can use the exogenous variation of the time control. None of these selections are inherently problematic, but they affect the interpretation and external validity of the results. When I find that the unskilled players do not optimally allocate attention, those players are nonetheless invested in chess.[14]

One reason chess is a good place to study attention is that the players understand the importance of allocating time. In Appendix A, I show that attention is valuable by regressing the strength of a move on how long it takes to make, instrumenting using the time control to get the causal effect. I also show that players are strategic by showing the ordinary least squares version of the same regression is biased because players take longer on harder moves.

The evidence in Appendix A is also important for what it cannot do: tell us whether players are allocating attention *optimally.* Such a limitation is typical of empirical tests in the literature because it is rare that economists observe both the benefits and the costs of attention. I aim to overcome this limitation in the rest of the paper.

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sum the point value of the pieces on the board per side, where a pawn is worth 1, bishops are worth 3, etc.

[11] Elo rating is a system used by chess players to rank how good the player is, based on their past results. Throughout the paper, I use this as a measure of skill.

[12] To have a Stockfish evaluation included in the dataset, someone must review the game. The most common reason is that one of the players wants to see where their mistakes were. Sometimes, people also look at games in which they were not involved. Occasionally, Lichess will look at the evaluation to detect cheaters.

[13] Some time controls, which I do not use, also include an increment in which players get extra time every move.

[14] An even less problematic selection is that draw offers and resignations are common, so situations in which the outcome is not obvious are more common.

Table 1: Summary Statistics

Time Control	(1) Number of Observations	(2) Unique Players	(3) Unique Games	(4) Mover's Elo Rating	(5) Time Spent (Seconds)	(6) Stockfish Evaluation (Pawns)	(7) Stockfish Eval. Change (Pawns)
15 seconds	446,760	3,098	8,529	1601	0.47	0.85	-1.75
				(216)	(0.62)	(8.51)	(3.37)
30 seconds	329,770	2,538	6,076	1836	0.87	0.57	-1.23
				(268)	(0.92)	(7.36)	(2.91)
1 minute	5,953,052	20,169	101,917	1592	1.60	0.54	-1.13
				(269)	(1.45)	(7.61)	(2.78)
3 minutes	4,210,627	24,771	67,774	1758	3.77	0.46	-0.92
				(320)	(4.64)	(7.02)	(2.52)
5 minutes	6,199,133	37,914	101,148	1563	6.08	0.48	-0.96
				(283)	(7.34)	(7.36)	(2.59)
10 minutes	6,853,468	42,954	112,429	1571	10.34	0.49	-1.01
				(268)	(12.94)	(7.77)	(2.65)
15 minutes	1,054,168	9,766	16,033	1664	14.13	0.44	-0.87
				(239)	(19.11)	(7.42)	(2.41)

Notes: Reported values in columns (4) to (7) are means, with standard deviations in parentheses. Stockfish evaluation is censored at $+20$ and -20 pawns. Stockfish Evaluation is at the start of the turn, from the perspective of the moving player. The change is how much the evaluation changes as a result of the turn. Source: Lichess

2 Theory

In chess, attention is valuable and strategically allocated (see Appendix A). However, rational inattention models make a stronger assumption: agents optimize their attention. Often, the first order conditions of such an optimization determine behavior. In this section, I propose a test based on such a first-order condition.

To model chess, I consider four state variables: whose turn it is, the position of the board, and the clocks of the two players. Players do not know the value of possible moves but have some prior and can receive signals about it. The cost of these signals is measured in the time it takes to process them. Denote the value function as V. On white's turn,

$$V(w, s, t^w, t^b) = \max_{S,\tau} \max_{s' \in L(s)} \mathbb{E}[V(b, s', t^w - \tau, t^b)|S]$$

such that

$$\mathbb{I}(S|Q^w(s)) = \tau$$

where the first argument of V represents whether it is white or black's turn, s is the position on the board, t is the time remaining for each player. A move is when the agent chooses a new position, s'. s' must be chosen from $L(s)$, the set of legal moves given s. The interior maximization says that given a specific signal, the agent chooses the move that maximizes their expectation of the value function.

While the arguments of V are all trivially observable, V is not known to the player; they only have a prior $Q^w(s)$ over the continuation value of each move. In the outer maximization, the agent chooses their signal structure S, a random variable, giving them information about V.[15] The cost of a signal, $\mathbb{I}$, depends on the prior and how informative the signal is about V. The cost is measured by τ, the time the player takes on the move.[16]

Implicit in this notation is the assumption that the prior information, Q^w is a function of the board position only. This assumption still allows for the signal to affect future priors, but only if it is intermediated through the move the player makes.[17] I discuss whether this is likely to bias my estimates in Section 4.4.

Black faces a similar problem, with the difference that they minimize V:

$$V(b, s, t^w, t^b) = \min_{S,\tau} \min_{s' \in L(s)} \mathbb{E}[V(w, s', t^w, t^b - \tau)|S]$$

such that

$$\mathbb{I}(S|Q^b(s)) = \tau$$

There exist some terminal states s such that the value function is 0, 1, or in relatively rare cases, $\frac{1}{2}$, i.e. white wins, black wins, or there is a draw.[18] Furthermore, if t^w reaches 0, the value function is 0, and if t^b reaches 0, the value function is 1.

Because of the symmetry, I consider only white's problem for the rest of this section. When I take the model to data, I test the first-order conditions of the agents combined.

[15] In the outer maximization, the agent chooses the the random variable, including how informative it is. In the inner maximization, the random variable is realized, and the agent maximizes their expectation of the value function over possible moves, given the realization of the signal.

[16] For example, in many economic models, $\mathbb{I}$ is proportional to the reduction in entropy between the prior information and the posterior (Shannon, 1948). I do not impose that $\mathbb{I}$ is Shannon.

[17] This assumption does not rule out forward-looking behavior. However, this is a stronger assumption than that agents do not acquire information that will be useful for future decisions beyond its use for current decisions (see Afrouzi and Yang (2021) for a discussion of how this relates to the linearity of information). My assumption further imposes that the whether you spend 1 second to play s' or 10 seconds to play s', the signal you received provides the same information for subsequent moves.

[18] Draws are rare in online chess (2.9 percent), unlike in classical chess.

A standard result in the rational inattention literature is that the actions are a sufficient statistic for the signal the agent receives, as there would be no reason for an agent to want any extra information. Hence I assume that $S = s'$ without loss of generality. Define

$$\vec{\pi}_s(\tau) = \arg\max_{\vec{p}(s')}\{\vec{p}(s') \cdot V(b, s', t^w - \tau, t^b) | \mathbb{I}(s'|Q^w(s)) \leq \tau\}$$

where $\vec{p}(s')$ is a vector of probabilities of getting the signal s'. $\vec{\pi}$ is the best achievable strategy within time τ. The problem the white player tries to solve is:

$$V(w, s, t^w, t^b) = \max_{\tau} \vec{\pi}_s(\tau) \cdot V(b, s', t^w - \tau, t^b)$$

If $\vec{\pi}(\tau)$ is continuous and differentiable, then the first-order condition is:

$$\vec{\pi}_{s'}(\tau) \cdot \frac{\partial V}{\partial t_w}(b, s', t^w - \tau, t^b) = \frac{d\vec{\pi}_{s'}}{d\tau} \cdot V(b, s', t^w - \tau, t^b)$$

Intuitively, the left-hand side measures the expected marginal cost of time, in terms of the future value function. The right-hand side measures the marginal benefit of spending more time to have a better position. Another way to write this is

$$\mathbb{E}_{s'} \frac{\partial V}{\partial t^w}(b, s', t^w - \tau, t^b) = \sum_{s'} \frac{d\pi_{s'}}{d\tau} V(b, s', t^w - \tau, t^b) \tag{1}$$

Equation (1) is a necessary condition for maximization and is the focus of my empirical tests.

3 Empirical Strategy

This section outlines how to estimate the left- and right-hand sides of equation 1. Section 3.1 shows how I estimate the left-hand side, and Section 3.2 shows how I estimate the right-hand side.

3.1 Estimating the Value Function

Both sides of equation (1) require estimating the value function. There are estimated to be more than 5×10^{42} possible chess positions (Shannon, 1950). So a key challenge is reducing the dimensionality. I use the Stockfish evaluation as a stand-in for s.[19]

[19] Sigman et al. (2010) also argues that the evaluation, along with the clocks, can reliably estimate winning likelihood.

If s becomes a single-dimensional argument, then I can approximate $V(b, s, t^w, t^b)$ using a local linear regression.[20] In practice, I split the evaluation into 40 bins, and each clock into 12 bins per time control.[21] For each bin, I estimate a regression

$$\text{Result}_i = \beta_0 + \beta_1 \text{Stockfish Evaluation}_i + \beta_2 t_i^w + \beta_3 t_i^b + \epsilon_i$$

on the observations i that fall into the bin, as well as bins that are adjacent in all three categories. Result_i is a variable that is 1 for a win, $\frac{1}{2}$ for a draw, and otherwise 0. I use the predicted value for V from the regression above, and $\hat{\beta}_2$ as the marginal cost of time for observations in that bin, i.e. the left-hand side of equation (1).

I run this separately for each time control and, within each time control, five bins of player skill based on their Elo rating.[22] I drop the estimate if the bin of interest has fewer than 10 total observations. I check that the value function estimation is able to match the data in Appendix B.

One concern is that some dimensions of the position may not be captured by Stockfish: for example, the difficulty for a human. In Section 4.4, I discuss in detail how this might bias the results. To preview the argument, I find such large deviations from theory that the other dimensions of the position would have to be implausibly large to explain the results.

3.2 Variation in the Time Spent on a Move

To get at the marginal benefit, I use a matching strategy combined with instrumental variables.

For each move, I attempt to match it to a move in another time control. I match the player, the number of turns into the game, and the approximate computer evaluation before the start of the move. The turn and player must match exactly. I split starting evaluations into 50 bins, and the moves must be in the same bin.[23] The matching is done without

[20] As an alternative estimation procedure, I also use LASSO splines (Osborne et al., 1998) to estimate the value function, and get very similar results. See Appendix Figures C.5 and C.6.

[21] The evaluation is split into 38 equally sized bins in which Stockfish evaluates the position in terms of pawns. I censor estimation at 20 pawns because there are a few extreme outliers that otherwise make local regression a bad approximation. Above 20 pawns, there is not much of an effect on the probability of winning. There is also a bin for an evaluation of "checkmate for white in [x] moves" and another bin for "checkmate for black in [x] moves." For the clocks, there is significantly more curvature in the win probability close to zero seconds remaining (see Figure B.1). So for each time control, I have a bin for the first 1/60th, 1/60th to 1/30th, 1/30th to 1/20th, 1/20th to 1/15th, 1/15th to 1/10th, 1/10th to 1/5th, and then every fifth remaining of the total time control.

[22] One can think of the strength of the two players as an important state variable, which was implicitly included in the V in the theory section, but which is important to account for now that we are aggregating across players.

[23] This resembles coarsened exact matching in that I throw out many data points that are not similar,

replacement, so each move is never used more than once. For the seven time controls, I match each with the next longer time control (e.g. 3 minutes to 5 minutes), and repeat the analysis matching it with the next shorter time control (e.g. 3 minutes to 1 minute).

Denote the pair of moves using i. The original move is i, c and the paired move is i, c^* where c denotes the time control and c^* is the matched time control. For most time controls, I match several hundred thousand moves.[24]

The right-hand side of equation (1) is the marginal improvement in the board position, holding the clocks fixed. So to measure how much the quality of the move improves in the other time control, I evaluate the value function using the time remaining after the move in the original time control, but the engine evaluation after the move in the matched time control. Comparing the value function of the original move to the value function of the matched move differs only on the positions on the board, and not on the clocks. In math,

$$V_{i,c^*} = V(b, s_{i,c^*}, t^w_{i,c}, t^b_{i,c})$$

Finally, I run the two-stage least-squares regression:[25]

$$\begin{aligned} V_{ik} &= \beta\tau_{ik} + \alpha_i + \epsilon_{ik} \\ \tau_{ik} &= \gamma \text{Time Control Indicator}_k + \delta_i + \eta_{ik} \end{aligned}$$

where k is either c or c^*. α_i and δ_i are fixed effects for each pair of observations. $\hat{\beta}$ measures a weighted average of the marginal benefit of spending more time on the quality of the move.

$$\hat{\beta} = \frac{\mathbb{E}[V_{ic^*} - V_{ic}]}{\mathbb{E}[\tau_{ic^*} - \tau_{ic}]}$$

which is a weighted-average of the right-hand side of equation (1) across many moves.

The instrument is needed because the amount of time taken by the player is endogenous to the difficulty of the move, as established in Appendix A. This would bias the estimated coefficient downwards because players spend longer on harder moves, and the difficulty of

unlike nearest-neighbor matching (Iacus et al., 2012). The difference in starting evaluation across matched observations averages less than two-hundredths of a pawn in each pair of time controls, and none are statistically significant.

In Appendix Figures C.3 and C.4, I show that the results are robust to having 100 bins of starting evaluations.

[24] Summary statistics for matched moves can be seen in Appendix Tables C.1 and C.2. The average Elo rating of players in the matched sample is slightly higher in some but not all cases, and the average change in Stockfish evaluation is less negative.

[25] To handle the large dataset with many fixed effects, I use the Stata command reghdfe from Correia (2019).

the move makes the estimated change in the win probability lower. With the instrument, the identifying assumption is that the time control, conditional on the turn and starting engine evaluation, is not correlated to the post-move value function except through the amount of time the player takes.[26]

3.3 Aggregation

Across many different positions, times, and skill-levels, the marginal benefit is not constant. So the estimate from the two-stage least squares regression is a weighted average of marginal benefits. In this section, I show how to compute a marginal cost that is weighted the same way, so that I can compare them to see if they are the same.

For each observation, imagine what the same person would do in the same situation, but in a different time control. Denote this counterfactual with a tilde: $\tilde{V}_{i,c^*}$ is the "true" counterfactual outcome in the other time control, and $\tilde{\tau}_{i,c^*}$ is the "true" counterfactual amount of time. The two-state least squares regression estimates

$$\beta_{MB} = \frac{\mathbb{E}[\tilde{V}_{i,c^*} - V_{i,c}]}{\mathbb{E}[\tilde{\tau}_{i,c^*} - \tau_{i,c}]} = \sum_i w_i \beta_i$$

where $\beta_i = \frac{\tilde{V}_{i,c^*} - V_{i,c}}{\tilde{\tau}_{i,c^*} - \tau_{i,c}}$, i.e. the benefit per second for observation i, and $w_i = \frac{\tilde{\tau}_{i,c^*} - \tau_{i,c}}{\sum_j \tilde{\tau}_{j,c^*} - \tau_{j,c}}$, i.e. a weight proportional to the amount of extra time the player takes in the other time control.[27]

This presents a challenge for comparing marginal benefit to marginal cost because I should not compare a weighted average of marginal benefit to an unweighted average of marginal cost. So I need to either undo the weights on the marginal benefit, or figure out how to weight the marginal cost.

Either would be easy if the true weights were observed, but they are not. Instead, I observe the outcome of a matched observation (τ_{i,c^*} is observed instead of $\tilde{\tau}_{i,c^*}$). Fortunately, the difference in times within the matched pair, $\tau_{i,c^*} - \tau_{i,c}$, is an unbiased estimate of the difference in times of the true counterfactual, $\tilde{\tau}_{i,c^*} - \tau_{i,c}$. So we can use that as the weight instead. Define the observed weight as:

$$\hat{w}_i = \frac{\tau_{i,c^*} - \tau_{i,c}}{\sum_j \tau_{j,c^*} - \tau_{j,c}}$$

Then we can recover a marginal cost that has the same weights as the marginal benefit by

[26]This assumption is more plausible, although one still might worry about whether the difficulty of moves is similar across time controls. While difficulty is not directly measurable, I can add controls for proxies of difficulty to see if the results are meaningfully different. I discuss this bias and more in Section 4.4.

[27]Since $\mathbb{E}V_{i,c^*} \to \mathbb{E}\tilde{V}_{i,c^*}$ and $\mathbb{E}\tau_{i,c^*} \to \mathbb{E}\tilde{\tau}_{i,c^*}$, $\hat{\beta}_{MB} \to \beta_{MB}$.

measuring

$$\hat{\beta}_{MC} = \sum_i \hat{w}_i \frac{\partial \hat{V}}{\partial t^w}$$

where the $\frac{\partial \hat{V}}{\partial t^w}$ is the coefficient on t^w in the estimation of the value function.[28]

With this reweighting, if agents were everywhere equating marginal benefit with marginal cost, then $\hat{\beta}_{MB} = \hat{\beta}_{MC}$ because each estimate is weighted the same. In practice, the reweighting does not have much effect on the estimates of average marginal cost.

There is one additional consideration. In general, $\hat{\beta}_{MB}$ is only a bound on the true marginal benefit because the returns to attention are highly concave and the time controls are not that close together.[29] In particular, when $\hat{\beta}_{MB}$ is estimated by matching moves to a shorter time control, the estimate is an upper bound for the marginal benefit, and when $\hat{\beta}_{MB}$ is estimated by matching moves to a longer time control, the estimate is a lower bound for the marginal benefit. Therefore, each matching strategy provides a one-sided test to reject rational inattention, where the null hypothesis is that $\beta_{MB} \leq \beta_{MC}$ if the matching is to a longer time control and $\beta_{MB} \geq \beta_{MC}$ if the matching is to a shorter time control.

4 Results

4.1 Comparing Marginal Benefit and Marginal Cost

Figure 1 plots the results of $\hat{\beta}_{MB}$ and $\hat{\beta}_{MC}$ for each time control. Both panels are presented in a log-log scaling. In panel (a), moves are paired with moves from a longer time control. In panel (b), moves are paired with moves from a shorter time control. Hence, theory implies the marginal benefit estimate should be below the marginal cost estimate in panel (a) and vice versa in panel (b). Panel (a) conforms with the null hypothesis. For one point estimate, the order is reversed, but the marginal cost line is well within the marginal benefit's confidence interval.[30]

In Panel (b), there are three observations in the shorter time controls for which the marginal cost is higher than the marginal benefit, in contrast to theory. In these time

[28] Note that because we are using the observed weights as opposed to the true weights, we cannot undo the weights (although not the primary issue, it would involve dividing by zero a lot). So the best I can do is compare the weighted marginal benefit with the weighted marginal cost.

Note also that there are some parallels to Abadie's κ (Abadie, 2003). However, the typical Abadie's κ does not apply to continuous treatment effects. In this setting, I can exploit the matched pairs to recover estimates of the weights, which I could not do generally.

[29] I show the concavity in the next section.

[30] The tables with the precise estimates and standard errors are in Appendix C. Of particular note, the first-stage F-statistic for the IV-regressions range from 300 to 25,000.

controls, players are spending too much time to decide on a move.[31] One might say they suffer "decision paralysis."

Note that the marginal benefit lines are decreasing as the time control increases, consistent with the idea of concave returns to attention.

How large are these deviations from rational inattention? For the three largest deviations (30 seconds, 1 minute, and 5 minutes), the marginal benefit is about half of the lower-bound on the marginal cost. I can use this statistic to guess how much extra time players are taking compared to an agent for which we would not be able to reject rational inattention. According to Table 1, time usage scales roughly linearly with the time control, and Figure 1 suggests that when the time controls goes up by a factor of 10 that the marginal benefit falls by a little bit less than a factor of 100. So a back-of-the-envelope calculation would suggest that players are spending about 40 percent too long thinking about their moves on average, in those three time controls.[32]

Standard Errors

Following Abadie and Spiess (2021), I cluster standard errors on the matched pair. Of note, the matching is done without replacement, a key assumption of Abadie and Spiess (2021).

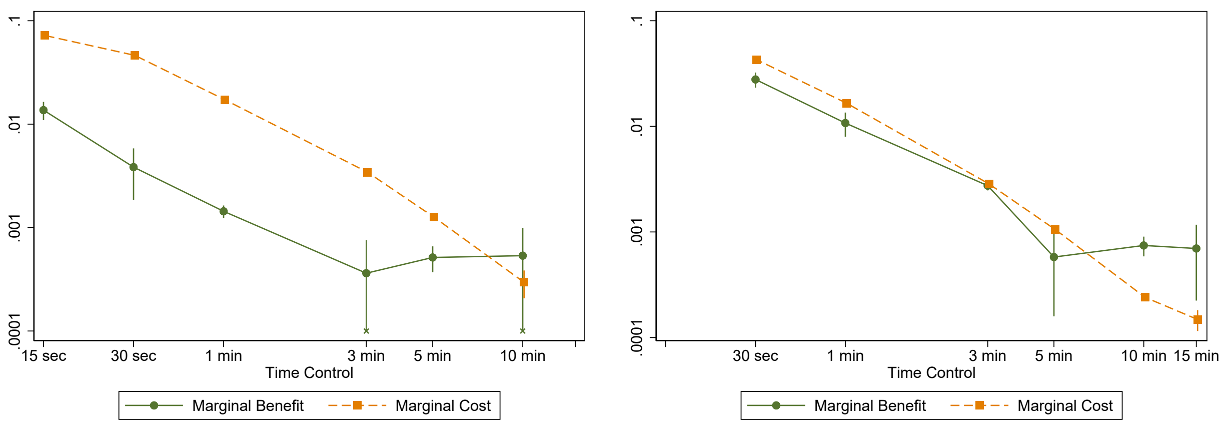
I do not adjust standard errors for the estimation of the value function which occurs before the main regression. The fact that the value function is measured with error is not inherently problematic: classical measurement error would not bias the regression or the standard errors. In principle, the fact that the value function estimates may be correlated across observations could bias the standard errors. However, the value function is estimated on an order of magnitude more data, so the additional uncertainty due to the value function estimation is likely quite small.[33]

Given the twelve comparisons in Figure 1, a brief discussion of multiple hypothesis testing is warranted. In the figure, standard two-sided 95 percent confidence intervals are displayed, which neither accounts for the fact that the test is one-sided nor for multiple hypothesis testing. Making the adjustment for the one-sided tests, the p-values in Figure 1b are $1.4 \times$

[31] The orange dashed lines in Panel (a) and Panel (b) are quantitatively quite close to each other but not exactly the same, as they both measure the average marginal cost of taking longer on a move, but are using different matched samples and have different weights.

[32] I arrive at 40 percent by solving the equation: $\log(x)/\log(2) = \log(10)/\log(100)$, where x is the excess amount of time taken, 2 is the factor by which the players have too low of a marginal benefit, and 10 and 100 define the empirical relationship between marginal benefit and time usage. A major caveat is that the downward slope of Figure 1 does not need to represent the true elasticity, since the sample for each regression is different, and so involves different players, evaluations, and parts of the game. However, it serves as a reasonable benchmark for a back-of-the-envelope calculation.

[33] Recall that the value functions are estimated on the dataset prior to looking for matched pairs across time controls.



(a) Lower Bound of Marginal Benefit

(b) Upper Bound of Marginal Benefit

Figure 1: **The average marginal benefit and marginal cost of spending additional time on a move.** Intervals are 95 percent confidence intervals, censored at .0001. Censoring is denoted by an X at the bottom of the interval. Tables of these results, including precise estimates and standard errors, clustered by matched pair, can be found in Appendix Tables C.4 and C.6. In some cases, the 95 percent confidence intervals on the marginal cost are too small to show up in the figure. The rational inattention prediction from Section 3 is that the marginal benefit should fall below the marginal cost in Panel (a) and should be above marginal cost in Panel (b).
Source: Lichess, author's calculations.

10^{-10} (30 sec), 2.2×10^{-5} (1 min), 0.22 (3 min), and 0.0138 (5 min).[34] Since there are twelve hypotheses, the Bonferroni (1935) correction—known to be conservative (Romano and Wolf, 2005; List et al., 2019) but extremely transparent—would multiply each p-value by twelve. The first two are still significant at the 10^{-8} level and the .0005 level.[35] The third was never significant, and the fourth's p-value becomes 0.16.

4.2 Heterogeneity by Skill

Theory predicts that $\beta_{MC} = \beta_{MB}$ not only on the entire dataset but also on any subset of the data. So in this subsection, I split the data into categories to examine whether there are deviations from the optimal allocation of attention.

I divide players into five groups per time control based on their Elo rating (which reflects the results of previous games). A higher Elo rating is better. I repeat the analysis from Figure 1 for each of the five groups within each of the time controls.

The results are presented in Figure 2. Because each time control is presented with its

[34] All the p-values can be found in Appendix C.

[35] A more conservative correction would also multiply by the 100 tests that I do when looking for heterogeneity in Figures 2 and 3. Even then, multiplying by 112, the results for 30 seconds and for 1 minute are significant at the 10^{-7} level and the 0.005 level.

own y-axis, there is no log-scale. In the panels on the upper row, I match moves to the next-shorter time control. In the the lower row, I match moves to the next-longer time control. Because the marginal benefit of attention is concave, that means that theory would predict that the marginal cost line should be above the marginal benefit line in the top row, and the opposite in the bottom row.[36]

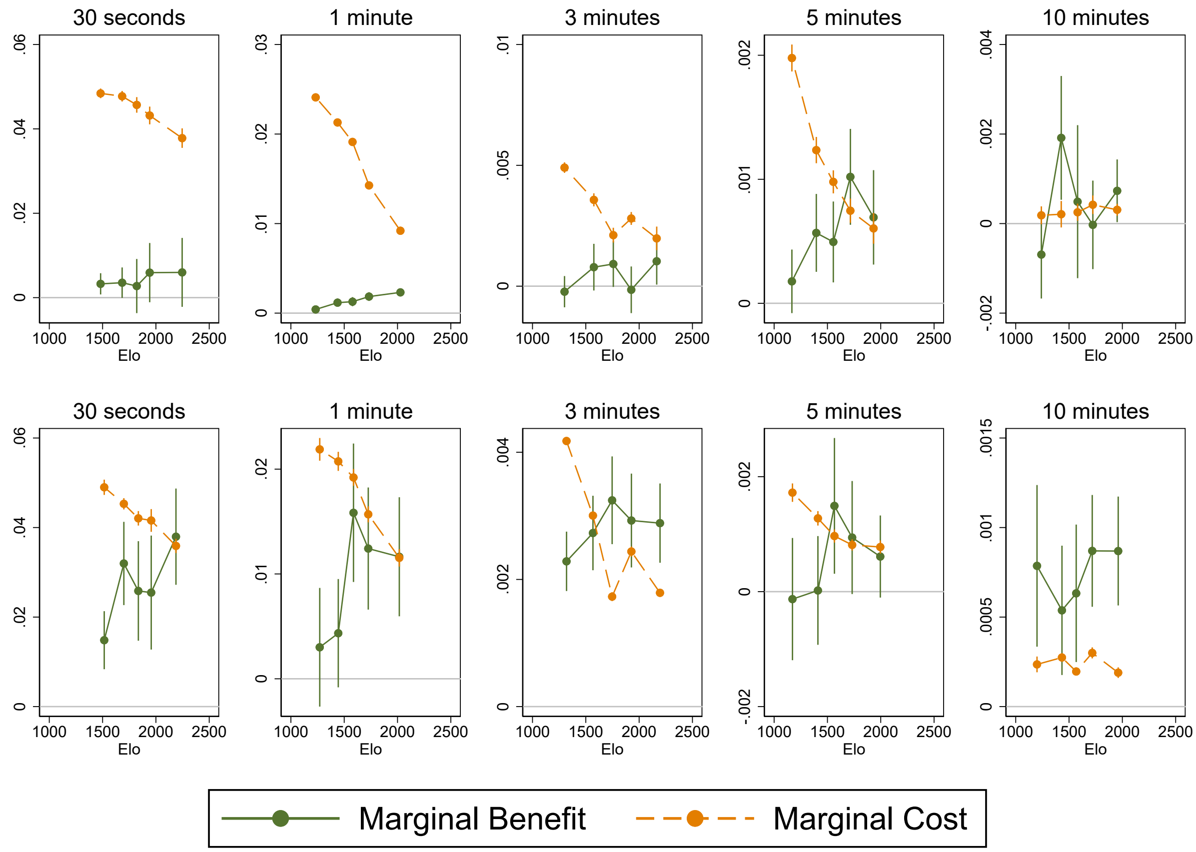


Figure 2: **Heterogeneity in Marginal Benefit and Marginal Cost by Elo rating.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. In some cases, the 95 percent confidence intervals on the marginal cost are too small to show up in the figure. Standard errors clustered by matched pair.
Source: Lichess, author's calculations.

In general, the top row conforms with theory. There are several notable exceptions to the theory in the bottom row. In the panels corresponding to 30 second, 1 minute, 3 minute, and 5 minute chess, worse players all have a lower marginal benefit than marginal cost. In 30 second chess, all but the highest quintile have lower marginal benefit. For 1 minute and 5 minute chess, the two lowest quintiles exhibit the same pattern, and for 3 minute chess,

[36]See the discussion in Section 3.3.

the lowest quintile exhibits the pattern as well. For the least-skilled players in the fastest time controls, the marginal cost is three to six times higher than the point estimate of the marginal benefit and double the largest point in the 95 percent confidence interval.[37]

The fact that marginal benefit is far below marginal cost for low-Elo players means that optimally allocating attention is associated with skill. Unskilled players are do not equalize marginal benefit and marginal cost. Skilled players are at least close enough so as not to be detectable using my empirical strategy. This is not to say that unskilled players do not respond partially to changes in marginal cost. Across time controls, the marginal benefit of moves is still generally increasing as the time controls get shorter (remember that the scale of the figures is changing across panels in Figure 2). It is just not increasing as fast as the marginal cost is.

High-Elo players differ from low-Elo players in a variety of ways. Unsurprisingly, the high-Elo players are better at chess, and are more likely to win, conditional on the opponent's skill.[38] However, the high-Elo also play more. Looking at only the 3-minute time control, a player in the top quintile plays more than twice as many moves in a given month as a player in the bottom quintile. Unfortunately, the data does not allow me to disentangle which features of skilled players make them better at allocating attention.

4.3 Heterogeneity by Time Remaining

Figures 1 and 2 suggest that any deviations from equation (1) are most prominent in short time controls. In this section, I investigate the role of available time by looking at the heterogeneity of results at different stages in the game. To do that, within each time control, I consider 5 equally sized bins of how much time is remaining in the game. As before, I then run the same analysis within each of these bins.

The results are presented in Figure 3. Like in Figure 2, in the top row, the marginal cost line should be above the marginal benefit line, and in the bottom row, the opposite.

In general, the top row again seems to conform to the theory. The bottom row rejects the hypothesis of optimal attention allocation near the end of games. For every time control, the marginal cost is below the marginal benefit at the start of the game, but when time is running low, the marginal benefit is consistently below the marginal cost. Because in the

[37]Here are the Bonferroni (1935)-adjusted p-values for the left-most points in the bottom-row of figures, from left to right: 5×10^{-21}, 1×10^{-8}, 2×10^{-13}, 0.041, and 1. These are the unadjusted one-sided p-values multiplied by 112, to reflect the 112 tests in Figures 1, 2, and 3.

[38]The fact that unskilled players are optimizing attention suboptimally is not why they are worse. A player in the bottom quintile in 15 minute games makes larger mistakes as a player in the top quintile in a 30 second game, as measured by the average change in Stockfish evaluation. So allocating time better within a 30 second game is not going to make a bottom quintile player as good as a top quintile player.

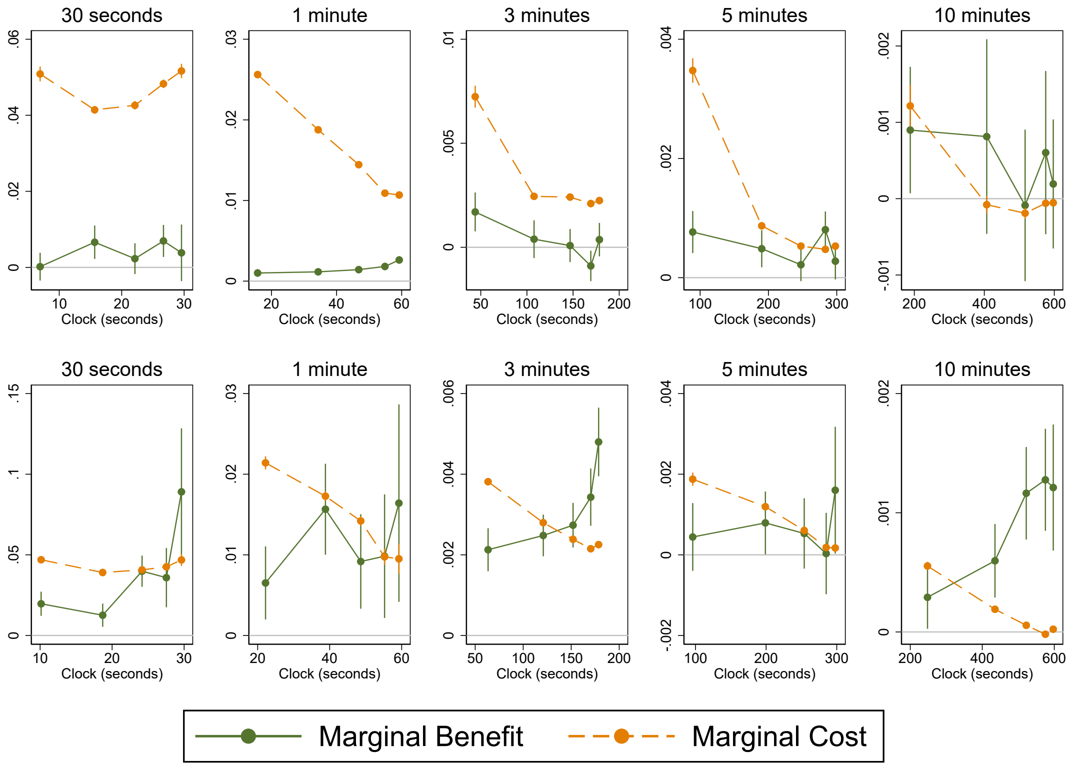


Figure 3: **Heterogeneity in Marginal Benefit and Marginal Cost by Time Remaining at the Beginning of the Turn.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. In some cases, the 95 percent confidence intervals on the marginal cost are too small to show up in the figure. Standard errors clustered by matched pair.
Source: Lichess, author's calculations.

bottom row, the marginal benefit is an overestimate, this rejects the hypothesis of rational inattention.[39]

4.4 Discussion of Potential Bias

In this section, I consider whether players playing optimally would necessarily satisfy the tests I propose. It should be noted that for many of the results, the marginal cost was several times larger than the marginal benefit. While some of the following concerns could bias the estimates on the margin, they are too small to explain the large differences from theory.

[39]Here are the Bonferroni (1935)-adjusted p-values for the left-most points in the bottom-row of figures, from left to right: 3×10^{-10}, 2×10^{-8}, 7×10^{-8}, 0.06, and 1. These are the unadjusted one-sided p-values multiplied by 112, to reflect the 112 tests in Figures 1, 2, and 3.

Dimensions of the Position Not Captured by the Stockfish Evaluation

One potential threat is that the Stockfish evaluation is not a sufficient summary of the position on the board to effectively calculate win probabilities. The main challenge is that the complexity of the game is not something the engine considers, but does matter for who wins the game. For example, when a player has an advantage, simplifying the position by trading off pieces is generally considered advantageous. A computer evaluating the position might not consider this, and so complexity does not factor into my measure of the marginal benefit.[40]

The analysis in Appendix Table B.1 shows that the gains in predictiveness of win probability when accounting for such a strategy is quite small compared to the predictiveness based on the Stockfish evaluation and the clocks. Including a dummy variable for a capture flexibly interacted with the clocks of both players and the computer evaluation only improves the R^2 of a regression of winning the game on my value function by a tiny fraction. If playing such a move only matters a tiny bit for the win probability, then a player cannot gain much benefit from such a move, meaning that even if they are more likely to play moves that might help them along non-Stockfish dimensions, the amount of bias this leads to must be quite small compared to the size of the marginal benefit based on Stockfish.

Appendix B also looks at the idea of putting the king in check—which constrains the number of moves for the opponent—and makes a similar conclusion.

Of course, there is a possibility that there is some other dimension of the position—not considered in Table B.1—that does explain a significant amount of win probability. For example, if a player plays an intentionally bad move to surprise their opponent, that would not be captured. In cases where this would work (which are rare), where both players have very little time, the value function is primarily determined by the two clocks anyway, so any bias should be small.

Difficulty of Moves across Time Controls

Another concern might be that the difficulty of moves varies across time controls, biasing the estimated marginal benefit. One reason to not be too concerned about this is that I make comparisons only between adjacent time controls, and the difference between a 3 minute and 5 minute game, in terms of the difficulty of decisions being made, is likely to be small. In Appendix Table C.7, I compare three variables that may be correlated to difficulty, to see how different they are within matched pairs. I look at three things: if the opponent

[40] A compelling direction for future research might be to use machine learning to create better measures of difficulty in chess. Carow and Witzig (2023) makes progress on this question, using a convolutional neural network.

had made a check, the number of possible moves is reduced, meaning the move is likely less difficult; if the opponent captured a piece, the response is more likely to be to retake the piece, and is often straightforward; and if there are more pieces on the board, the more complex the position typically is. In general, while there are some statistically-significant differences, they are economically small.

Further, controlling for proxies of difficulty does not make a meaningful difference in the estimated coefficients. In Appendix Figures C.7 and C.8, I run the same regressions as in Figures 2 and 3, but controlling for whether the opponent made a check and/or capture, and separately, for the number of pieces on the board. All of these controls are interacted with the matched pair, so these controls effectively drop observations in which these properties are not also matched. While the standard errors increase, especially for the number of pieces control, the conclusions are not substantially different.

Corner Solutions

Another possible objection is that if the solution is not interior, the first-order condition may not be a necessary condition for optimality. If players are constrained to take a certain amount of time by the physical amount of time it takes to click,[41] then it could be that the marginal benefit is lower than the marginal cost.

However, the estimator implicitly puts zero weight on situations in which players are at corner solutions. The reason is that players at corner solutions take the same amount of time in either time control, so the moves get zero weight, by equation (3.3). So corner solutions cannot be driving the results.

Hopeless Situations

A related worry is that in a game that the player is certain to lose (or win), they no longer bother to make rational decisions. If this drove the results, the finding would not have much validity for the cases where people do care. However, in this case, the measured marginal benefit and marginal cost will both be zero, so it could not contribute to the results.

Having Already Thought Through the Move

Another objection is that the player could have already spent time thinking through the move. In a longer time control, there is potential for more unmeasured time spent considering the move. This is likely to be a small bias, as the player must usually think about many

[41] Or if the players take zero time because they "pre-moved," which means to have already clicked on a move before their opponent made their move.

possible positions ahead of time. Moreover, this would mean that I am overestimating the marginal benefit, so this bias could not explain the deviation from equal marginal benefit and marginal cost.

Thinking Ahead

A related story is that players think several moves into the future, and since this store of knowledge is not included in the state-space, the marginal benefit might be underestimated. This is the opposite concern from the previous one, and like that, I think any bias will be small. The first reason is theoretical: under rational inattention, it is suboptimal to spend time processing information that is not relevant for the current move when the costs are linear, as you can always think about future moves when they arrive.[42]

Nonetheless, a signal that a player should play a certain move might indicate that a subsequent move is good. However, at these time controls, players are unlikely to plan moves for far ahead that are different than their initial instincts in that position. For example, playing a move that forks the queen and the king is not going to bias the results because taking the queen on the next turn is obvious.

Further, the heterogeneity suggests this is not the operative story. If planning ahead was an important confounder, rejections of the null hypothesis would occur more for skilled players and when players have more time. But it is the unskilled players and the players with little time that exhibit do not optimally allocate attention.

Learning

A final concern is agents sacrificing time to become better chess players, biasing the estimated marginal benefit down. However, players have unlimited time after the game for analysis. In fact, because someone has to analyze the game to be included in the dataset, the players in my data are more likely to use this option.

This argument extends to other costs and benefits of attention outside the model. For example, economists sometimes assume that cognitive effort is directly utility-lowering. Explaining my empirical results would require the opposite assumption: that cognitive effort is directly utility-enhancing. This assumption may be plausible for chess, which is voluntary and fun. However, it is hard to square with why players devote too much attention during the game rather than afterward.

[42] Linearity means that the cost of a signal about a future move is the same now or later.

5 Improving the Allocation of Attention

Lichess reminds players that their time is running low by playing a beeping noise and changing the color of the clock that the player sees on their screen from black to red. This reminder occurs at a set time: in 1 minute chess, the beep is when the player's clock is at 10 seconds.

This section shows that players take shorter time and make larger mistakes after this intervention. Because players were previously taking too long when they had little time, this analysis suggests that the intervention improves their allocation of attention.[43] It adds to the evidence that players are allocating attention sub-optimally because they make better choices when given a simple reminder.[44]

The nature of this intervention does not allow for a regression discontinuity design. That is because the beep occurs at 10 seconds in 1 minute games, and the time at which players make their move is endogenous. As expected, I observe significant bunching in the number of moves made at times just past the threshold rather than just before it (Figure 4a).[45] Consistent with previous results that marginal cost exceeds marginal benefit for unskilled players, I show the bunching is stronger for lower-rated players in Panels (d) and (e).[46]

Even though I cannot do a regression discontinuity analysis, the data still tells us about the players' behavior in response to the beep. The bunching itself reveals that the beep reminds players that the marginal cost of their time is high. Figure 4b shows the average amount of time spent on a move, binned by the number of seconds remaining when they make the move. Moves made with 9 seconds remaining are not that much shorter because some of those moves are made after a player has thought for awhile, and then the beep reminds them how high the marginal value of time is. But there is a large drop-off between 9 and 8 seconds, suggestive that players do move much faster after having heard the beep.

Figure 4c shows the change in the computer evaluation. Players make larger mistakes on average after they hear the beep.[47] The intervention is presumably beneficial to players because it causes them to speed up. Based on the analysis from the previous section, the marginal benefit of making better moves is smaller than the marginal cost of time. So even

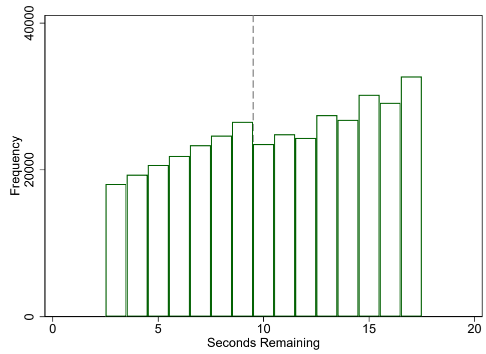
[43] One caveat to this claim is that I do not rule out that the beep leads to overshooting. The beep pushes people in the direction of optimal attention allocation, but I cannot rule out that the reminder pushes them so far that they end up worse off.

[44] Antioch (2020) also shows players blunder more after the beep. Antioch argues the beep is a bad thing, but my previous results argue that speeding players up is beneficial. A few things that differentiate our analysis is that Antioch defines blunders in a binary way and does not look at bunching or the time spent on the move.

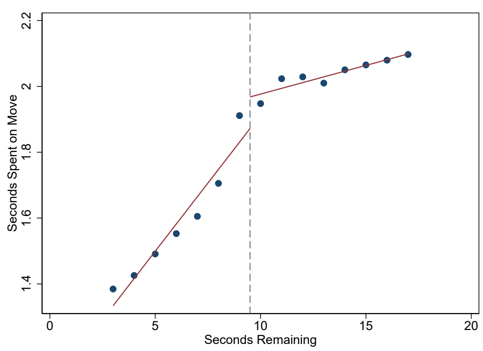
[45] I only consider moves where the computer evaluation before the player's turn is less than a 5 pawns advantage. I show robustness for 30 seconds in Appendix Figure C.1.

[46] All five quintiles can be seen in Appendix Figure C.2.

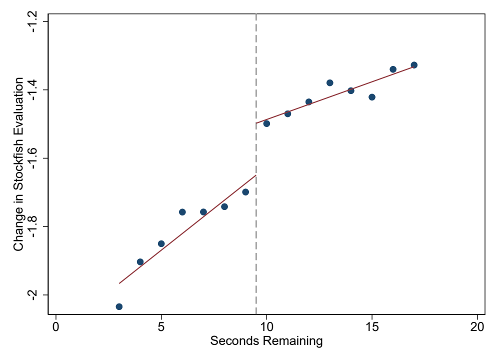
[47] The coefficient is 0.37 at the discontinuity, and the robust standard error is 0.06. With player fixed effects, the coefficient and robust standard error are 0.34 and 0.06.



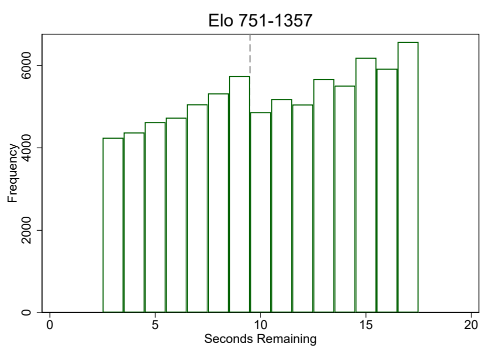
(a) The Distribution of Time Remaining After a Move



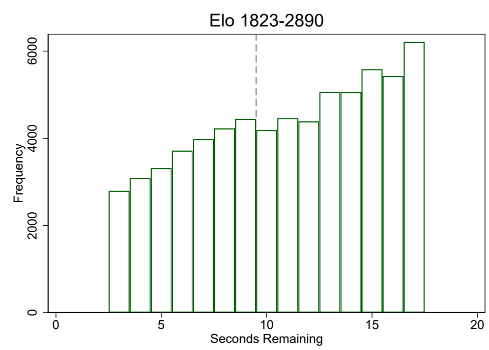
(b) The Amount of Time Spent



(c) The Change in Computer Evaluation



(d) The Distribution of Time Remaining After a Move, Unskilled Players



(e) The Distribution of Time Remaining After a Move, Skilled Players

Figure 4: **The Low-Time Reminder.** Panel (a) shows a histogram of the number of moves played by the number of seconds remaining in the 1 minute time control. The number of seconds remaining is measured discretely, and only 3 seconds to 17 seconds are plotted. The grey dashed line indicated the time at which the beep occurs. Panels (b) and (c) show the average amount of time spent and the change in computer evaluation, by the number of seconds remaining at the end of the move. Panels (d) and (e) show the same as Panel (a), but only for the lowest and highest quintile of the mover's Elo.
Source: Lichess, author's calculations.

though the players make larger "mistakes," the additional speed helps their win probability.

6 Conclusion

The results in this paper have implications for both models of rational inattention and for further empirical work on rational inattention.

I have shown that the quantitative predictions of rational inattention seem to hold for good chess players and for chess players with sufficient time. The first-order condition associated with equalizing the marginal benefit and marginal cost of attention cannot be rejected in the data, despite the large dataset. This is generally good news for the way economists model rational inattention in the literature. However, the paper also shows that low-Elo players suffer from "decision paralysis," i.e. rational inattention is a skill. To be clear, low-Elo players still are strategic in adjusting their attention, but not to the optimal degree. Hence, taking the quantitative conclusion of rational inattention models seriously might make more sense for experts and experienced agents. Economists might need to consider other behavioral theories for amateurs.

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Online Appendix

A The Benefit and Allocation of Attention

To those experienced with chess, it is unsurprising that additional attention improves the quality of play or that players strategically allocate their time. In the words of former world champion Vladimir Kramnik, "time is precious when you don't have enough of it" (ChessBase News, 2003). Nonetheless, in this appendix, I present econometric evidence to demonstrate these two facts.

I consider the relationship between the time spent on and the quality of a move. As a naive measure of the quality of the move, I use the change in the Stockfish evaluation of the position from before and after the move. Computers anticipate the best move, so if the player plays that, the evaluation changes only slightly. Therefore, the vast majority of evaluation changes are negative. A big negative evaluation change would occur when the players makes a big mistake.

I run the regression

$$\text{Stockfish Evaluation Change}_i = \beta\tau_i + \text{Player-Turn Fixed Effects} + \epsilon_i$$

where τ_i is the time spent on the move. I run the regression separately on moves in the 3 minute time control and in the 5 minute time control. I only include moves where the computer, before the move is made, evaluates the position to be within 5 pawns of being even.

I also run this regression on the pooled 3- and 5-minute sample, instrumenting for τ_i using an indicator variable for the 5 minute time control.

The results of all three regressions are presented in Table A.1. The instrumental-variables regression has a positive and statistically significant effect.[48] These results would suggest that the causal effect of spending an extra second on your move is worth about 0.03 pawns.

Nonetheless, there is a negative correlation between time spent and the engine evaluation: for moves on which a player spends an extra second, the resulting position is 0.02 or 0.04 pawns worse. The most likely reason that the OLS and the IV regressions give different answers is that there is an important omitted variable in the OLS that biases it away from the causal effect, namely the difficulty of the move. Players spend longer considering harder moves, and then end up playing worse moves because the move is harder.

[48]The F-statistic indicates weak instruments are not an issue. The first-stage regression shows that players spend 1.7 additional seconds on a move in the 5 min time control compared to the 3 minute time control. For comparison, the average time spent on a move in 3 minute time control is 3.7 seconds.

Table A.1: Motivating Facts

	(1) Evaluation Change	(2) Evaluation Change	(3) Evaluation Change
Seconds Spent	-0.0375	-0.0230	0.0306
	(0.000263)	(0.000146)	(0.00196)
Observations	3886279	4938789	9129274
F-statistic			30753.3
Time Control	3 min	5 min	3 and 5 min
Fixed Effects	Player-Turn	Player-Turn	Player-Turn
Instrument	–	–	Time Control

Notes: Evaluation change measures the difference in the Stockfish evaluation of the position at the end of the move, minus its evaluation at the start of the move. Seconds spent is measured using the difference in the clock from the start to the end of the move. In column (3), the seconds spent on the move is instrumented using an indicator variable for being in the 5-minute time control. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Source: Lichess

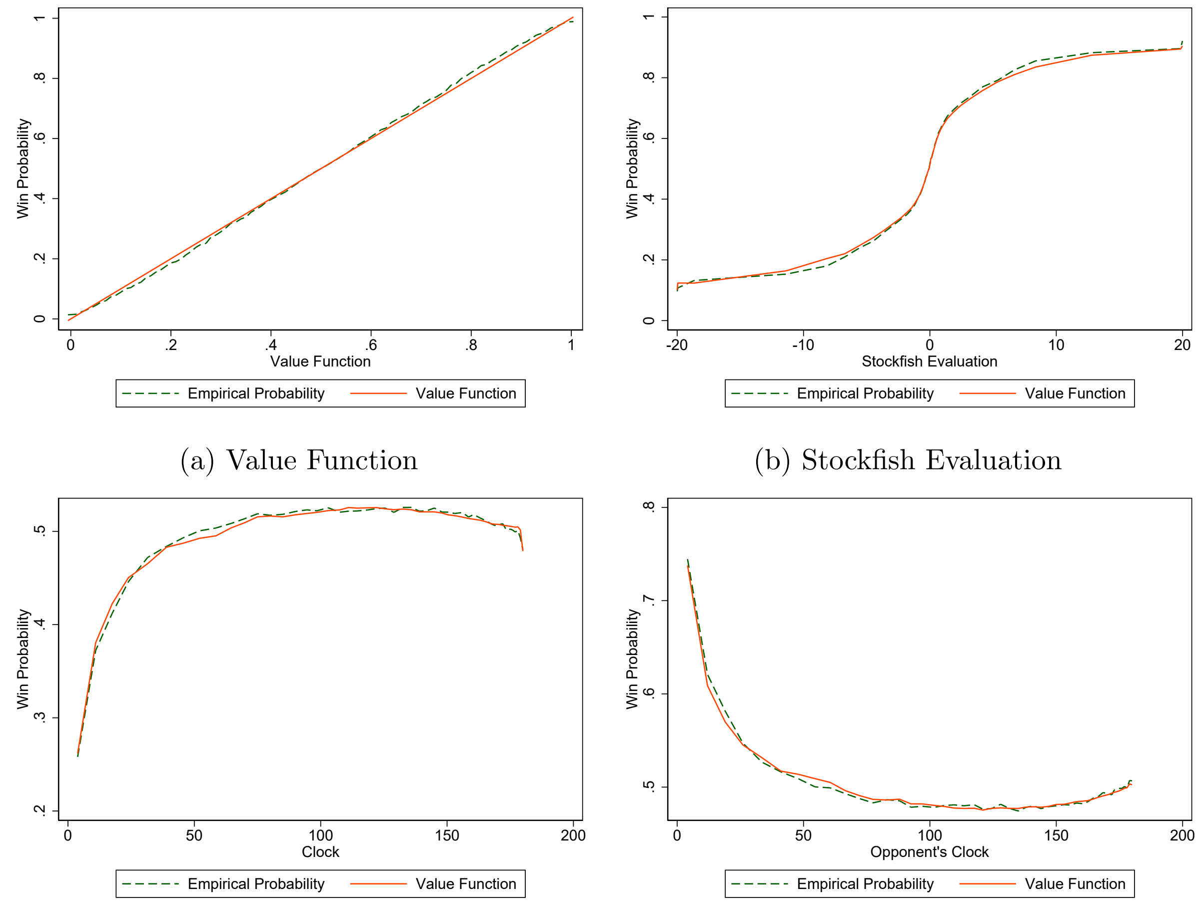
These results establish that chess is a good place to study how players allocate attention. First, the results establish that attention is valuable, by the positive sign of the IV-coefficient. Second, the results establish that players are making choices on how to allocate attention based on the difficulty of the move, by the negative OLS-coefficient.

B Checking the Fit of the Value Function Estimation

In this section, I check how well the value function estimation works. First, I look at the marginal probability of winning across each state variable. Second, I check to see if the distributions of the value function and the marginal value of time look as I expect them to. Throughout this section, I show the estimated value function on the 3 minute time control, but the results look qualitatively similar for all time controls.

In Figure B.1, I plot the average value of the game result alongside the average value of our estimated evaluation for 50 bins of the value function, 50 bins of the Stockfish evaluation, 50 bins of the player's clock, and 50 bins of the opponent's clock. The lines seem to match fairly well, suggesting that the local regression strategy is able to adequately capture the curvature of the value function. Note that the downward sloping part of Panel (c) or the upward sloping part of Panel (d) does not imply that time has negative value, as time is correlated to the Stockfish Evaluation.[49]

[49] In Figure B.2, few of the observations are estimated to have a negative marginal value of time.



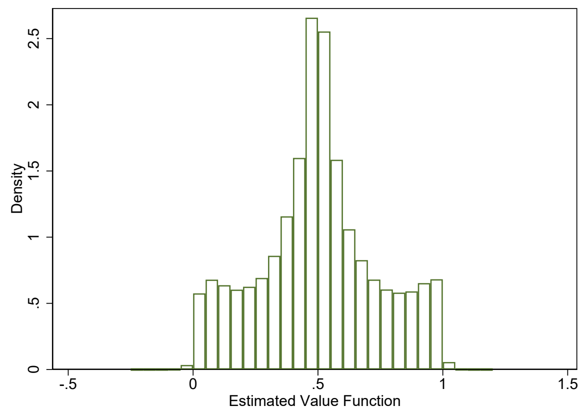
(a) Value Function

(b) Stockfish Evaluation

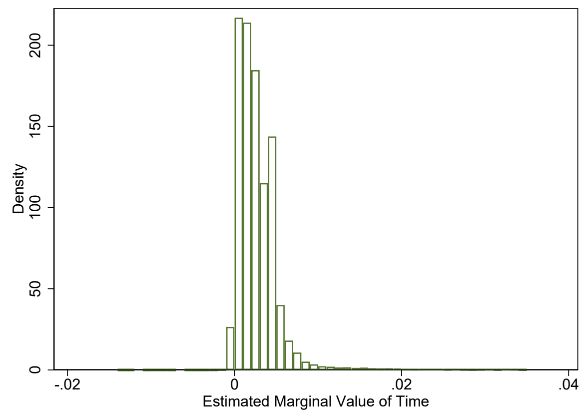
(c) Time

(d) Opponent's Time

Figure B.1: **Checking the Fit of the Value Function Estimation.** The green dashed line is the empirical relationship between the game result the x-axis variable. All observations are split into 50 equal-sized bin of the x-variable, and the average of the x-value is plotted against the average value of the game result in that bin, where wins are 1, losses are 0, and draws are 1/2. The orange solid line plots the same, but with the average value function instead of the average game result. Data is from the 3-minute time control. For the Stockfish Evaluation, evaluations of "checkmate in [x] moves" are not graphed.
Source: Lichess, author's calculations



(a) Value Function



(b) Marginal Value of Time

Figure B.2: **Distributions of the Estimated Value Function.** Panel a presents a histogram of the estimate value function, with bins 0.05 across. Histogram is trimmed at -0.5 and 1.5 (a total of nine points are trimmed). Panel b presents a histogram of the estimate of the marginal value of time, with bins 0.001 across. Histogram is trimmed at the 99th percentile. Number of observations: 4,239,728.
Source: Lichess, author's calculations.

In Figure B.2, I show the distribution of the estimated value function, and the estimated marginal value of time. Although nothing constrains it to be so, there are few values outside of the [0,1] range. The marginal value of time is almost everywhere positive, and the distribution peaks at a number close to 0 and has a long right-tail. I attribute the facts that a small minority of value function estimates lie outside of $[0, 1]$ and that the marginal value is sometimes negative to measurement error. The tests I propose take averages over many data points, so the measurement error will wash out by the law of large numbers.

The last thing that I want to check is whether the Stockfish evaluation is an adequate proxy for the position. Specifically, because the chess position is many dimensional, it is not obvious that the Stockfish evaluation captures most of the information about the position that could be useful to evaluate who is likely to win.

In Table B.1, I check whether a few of the observable things about the position add to the predictive power of our value function. In column (1), I regress the result of the game (0, $\frac{1}{2}$, or 1) on the value function. This explains about a quarter of the variance in the result of the game, and the coefficient is close to 1. In column (2), I add a regressor for whether the move played was a capture, and in column (3), I add a regressor for whether the move was a check, i.e. threatened the opposing king. Both of these are potentially another dimension on which a player might seek an advantage: in the first case, the player is simplifying the game, as fewer pieces typically make the game easier to evaluate; and in the second case, the player is limiting the other player's possible moves, as when the opponent is in check, the opponent must play a move that takes them out of check.[50]

In both of these columns, the additional variable is statistically significant, which is not surprising with over 4 million observations. However, the controls do not explain a significant amount of the variation. The R^2 increases from 0.24382 to 0.24392 or 0.24386, so about one-hundredth of a percent of the variation in outcomes.

In column (4), I add a dummy for each possible move notation,[51] which includes which type of piece moved, which square the piece moved to, whether there was a piece captured, and whether the move was made with check. This has over 4000 coefficients, so I do not report them, but even then, the R^2 increases by only one-tenth of a percent.

Table B.1: Sufficiency of the Value Function

	(1) Game Result	(2) Game Result	(3) Game Result	(4) Game Result	(5) Game Result	(6) Game Result	(7) Game Result
Value Function	1.030	1.032	1.032	1.032	1.034	1.031	1.009
	(0.000881)	(0.000886)	(0.000893)	(0.000916)	(0.00580)	(0.00579)	(0.00857)
Capture		-0.0119					
		(0.000487)					
Check			-0.0124				
			(0.000845)				
Constant	-0.0144	-0.0127	-0.0147				
	(0.000487)	(0.000492)	(0.000487)				
Observations	4240552	4240552	4240552	4239371	4240229	4239492	3354087
R^2	0.24382	0.24392	0.24386	0.24495	0.24752	0.24743	0.30922
Adjusted R^2	0.24382	0.24392	0.24386	0.24426	0.24394	0.24409	0.17838
Fixed Effects				Move Notation	Capture × Bins	Check × Bins	Notation × Bins
Num of Non-Dropped Fixed Effects				3876	20080	18766	534130

Notes: Game result is 1 for a win, 0 for a loss, and 1/2 for a draw. "Value Function" is the value function described in Section 3. "Capture" and "Check" are dummy variables for capturing an opponent's piece and putting the opponent's king in check, respectively. Move notation includes indicator variables for the way the move is denoted which includes information on which type of piece is moved, the square the piece is moved to, whether a piece is captured, and whether the king is checked. In columns (5)-(7), those variables are interacted with the bins of computer evaluation, clock, opponent's clock, and Elo rating, as described in the text. Robust standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Source: Lichess, author's calculations.

In columns (5), (6), and (7), I interact the regressors in Columns (2), (3), and (4) with the same bins of the player's clock, the opponent's clock, the Stockfish evaluation, and the Elo rating of the player.[52] This could be important if putting the opponent in check was valuable but only in certain situation, such as when the opponent was low on time. In columns (5)

[50] At some levels of skill, there may also be a psychological factor in which the opposing player finds being placed in check to be intimidating. With little time remaining, it is also sometimes a strategy to place the other player in check when they do not anticipate such a move because it will take them longer to respond than if they can play any move.

[51] An example would be "Qxg3+", where the "Q" represents that the queen moved, the "x" represents that a piece was captured, "g3" is the square the piece moved to, and "+" indicates that the move was with check.

[52] Recall that there are 12 bins for each clock, 40 bins for the evaluation, and 5 bins for the Elo rating. Some of these bins are sparsely populated.

and (6), I am estimating about 20,000 additional coefficients, but the R^2 goes up only about three-tenths of one percent.[53] Finally, in column (7), where about a fifth of the sample is dropped because there is only one observation within the bin, and where there are 534,000 fixed effects where the observations are not dropped, the R^2 finally increased significantly, by about 7 percent. But much of this is likely because there are simply so many fixed effects: the adjusted R^2 has actually decreased by 6 percent.

This analysis confirms that the computer evaluation and the clocks are capturing the vast majority of the variation in who is likely to win the game. Of course, there are additional dimensions of the position that can help determine the win probability, but based on the fact that several of the most likely components had negligible impacts on the predictive power of the value function, I am confident that the computer evaluation and the clocks are capturing a large majority of the variation.

[53]Note that the adjusted R^2 goes up less.

C Appendix Figures and Tables

Tables C.1 and C.2 report the same summary statistics as Table 1, but only on the subsample of the data that is matched. As expected, the number of observations, unique players, and unique games are substantially lower. In some cases, the average Elo rating is higher, though not universally. Time spent is similar to the full sample. Of note, the change in Stockfish evaluation is less negative in the matched samples (column 7). Players playing and analyzing games in multiple time controls are probably slightly higher skilled. The matching is also more likely to match moves near the start of the game, when evaluations tend to change less.

Table C.1: Summary Statistics for Matched Data (Shorter Time Control)

Time Control	(1) Number of Observations	(2) Unique Players	(3) Unique Games	(4) Mover's Elo Rating	(5) Time Spent (Seconds)	(6) Stockfish Evaluation (Pawns)	(7) Stockfish Eval. Change (Pawns)
30 seconds	21,140	825	3,750	1632	0.76	0.35	-0.87
				(219)	(1.05)	(6.59)	(2.22)
1 minute	30,125	936	7,147	1707	1.36	0.59	-0.88
				(257)	(1.34)	(6.11)	(2.75)
3 minutes	128,628	5,140	22,127	1638	3.52	0.31	-0.68
				(310)	(4.54)	(6.06)	(2.06)
5 minutes	71,422	4,645	16,926	1644	5.51	0.15	-0.55
				(296)	(7.11)	(4.92)	(1.72)
10 minutes	136,626	7,225	30,062	1494	9.46	0.24	-0.50
				(269)	(11.89)	(4.66)	(1.65)
15 minutes	27,781	2,356	5,749	1692	12.89	0.14	-0.43
				(246)	(17.95)	(4.00)	(1.28)

Notes: Reported values in columns (4) to (7) are means, with standard deviations in parentheses. Stockfish evaluation is censored at $+20$ and -20 pawns. Stockfish Evaluation is at the start of the turn, from the perspective of the moving player. The change is how much the evaluation changes as a result of the turn.
Source: Lichess

Table C.2: Summary Statistics for Matched Data (Longer time control)

Time Control	(1) Number of Observations	(2) Unique Players	(3) Unique Games	(4) Mover's Elo Rating	(5) Time Spent (Seconds)	(6) Stockfish Evaluation (Pawns)	(7) Stockfish Eval. Change (Pawns)
15 seconds	21,000	825	4,314	1812	0.46	0.41	-1.00
				(252)	(0.62)	(6.05)	(2.27)
30 seconds	25,665	936	3,559	1729	0.93	0.44	-0.97
				(266)	(0.86)	(5.95)	(2.23)
1 minute	157,925	5,140	35,586	1634	1.47	0.25	-0.69
				(309)	(1.40)	(5.93)	(2.13)
3 minutes	73,665	4,645	16,556	1661	3.80	0.18	-0.51
				(297)	(4.59)	(4.62)	(1.72)
5 minutes	143,274	7,225	28,990	1587	5.97	0.24	-0.59
				(261)	(7.72)	(5.68)	(1.76)
10 minutes	30,458	2,356	7,960	1696	10.54	-0.14	-0.51
				(243)	(14.25)	(5.74)	(1.64)

Notes: Reported values in columns (4) to (7) are means, with standard deviations in parentheses. Stockfish evaluation is censored at $+20$ and -20 pawns. Stockfish Evaluation is at the start of the turn, from the perspective of the moving player. The change is how much the evaluation changes as a result of the turn. Source: Lichess

Tables C.3 to C.6 show the first-stage estimates and the precise numbers behind Figure 1. The first two tables correspond to Panel (a) and the second two tables to Panel (b).

Table C.3: First-Stage, Figure 1a

	(1) 15 second	(2) 30 second	(3) 1 min	(4) 3 min	(5) 5 min	(6) 10 min
Time Control Indicator	0.294***	0.431***	2.099***	1.724***	3.509***	2.388***
	(0.00929)	(0.0106)	(0.0139)	(0.0323)	(0.0387)	(0.133)
Observations	34208	41156	224198	121862	229328	49956

Notes: First-stage of regression estimating the marginal benefit of attention. Each column represents the difference in time taken between the time spent in the time control indicated by the column title, and the next highest time control (i.e. 1 min for 30 seconds). Each regression includes a fixed effect for the matched variable, i.e. the player who is moving, the turn of the game, and the approximate computer evaluation. The outcome is measured in seconds. Standard errors clustered by matched pair. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Source: Lichess.

Table C.4: IV Regression, Figure 1a

	(1) 15 second	(2) 30 second	(3) 1 min	(4) 3 min	(5) 5 min	(6) 10 min
Marginal Benefit	0.0137***	0.00384***	0.00144***	0.000362	0.000514***	0.000535*
	(0.00140)	(0.00101)	(0.000101)	(0.000200)	(0.0000735)	(0.000235)
Marginal Cost	0.0720***	0.0462***	0.0172***	0.00343***	0.00126***	0.000296***
	(0.00132)	(0.000355)	(0.0000733)	(0.0000688)	(0.0000259)	(0.0000453)
Observations	34208	41156	224198	121862	229328	49956
One-sided p-value	1	1	1	1	1	0.159
First stage F-statistic	999.6	1660.0	22708.9	2844.5	8243.5	323.9

Notes: Instrument variables regression estimating the marginal benefit of attention. Each column represents the regression of the probability of winning based on the position and the clocks in the designated time control on the time spent on the move, using matched moves from the next highest time control and the instrumental variables strategy described in the text. Each regression includes a fixed effect for the matched variable, i.e. the player who is moving, the turn of the game, and the approximate computer evaluation. The coefficient can be interpreted as the marginal benefit in the probability of winning based on the resulting position per second spent on the move. Standard errors clustered by matched pair. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Source: Lichess.

Table C.5: First-stage, Figure 1b

	(1) 30 second	(2) 1 min	(3) 3 min	(4) 5 min	(5) 10 min	(6) 15 min
Time Control Indicator	-0.304***	-0.440***	-2.104***	-1.721***	-3.534***	-2.370***
	(0.00788)	(0.0103)	(0.0132)	(0.0319)	(0.0403)	(0.134)
Observations	33578	43518	225144	122336	229578	49668

Notes: First-stage of regression estimating the marginal benefit of attention. Each column represents the difference in time taken between the time spent in the time control indicated by the column title, and the next lowest time control (i.e. 30 seconds for 1 minute). Each regression includes a fixed effect for the matched variable, i.e. the player who is moving, the turn of the game, and the approximate computer evaluation. The outcome is measured in seconds. Standard errors clustered by matched pair. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

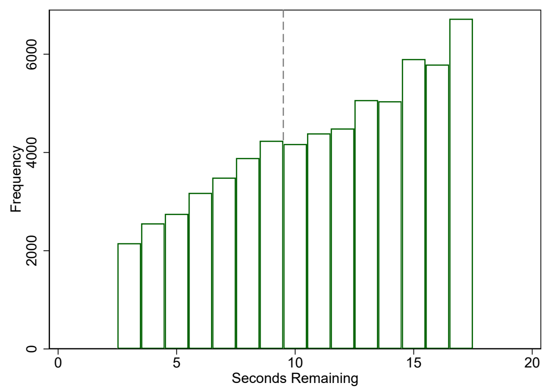
Source: Lichess.

Table C.6: IV Regression, Figure 1b

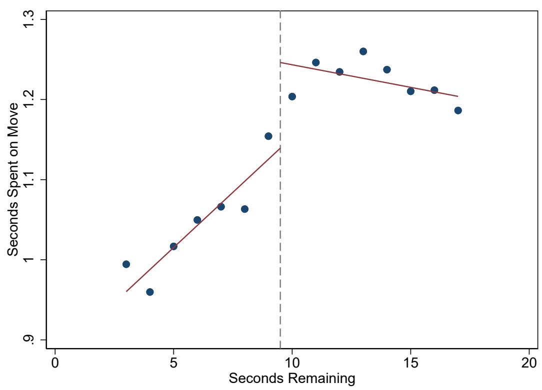
	(1) 30 second	(2) 1 min	(3) 3 min	(4) 5 min	(5) 10 min	(6) 15 min
Marginal Benefit	0.0278***	0.0107***	0.00274***	0.000578**	0.000745***	0.000697**
	(0.00231)	(0.00141)	(0.000137)	(0.000214)	(0.0000806)	(0.000242)
Marginal Cost	0.0427***	0.0166***	0.00285***	0.00106***	0.000240***	0.000148***
	(0.000409)	(0.000220)	(0.0000149)	(0.0000291)	(0.00000680)	(0.0000169)
Observations	33578	43518	225144	122336	229578	49668
One-sided p-value	1.35e-10	0.0000221	0.217	0.0138	1.000	0.989
First stage F-statistic	1485.1	1828.1	25449.9	2907.6	7698.8	312.4

Notes: Instrument variables regression estimating the marginal benefit of attention. Each column represents the regression of the probability of winning based on the position and the clocks in the designated time control on the time spent on the move, using matched moves from the next lowest time control and the instrumental variables strategy described in the text. Each regression includes a fixed effect for the matched variable, i.e. the player who is moving, the turn of the game, and the approximate computer evaluation. The coefficient can be interpreted as the marginal benefit in the probability of winning based on the resulting position per second spent on the move. Standard errors clustered by matched pair. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

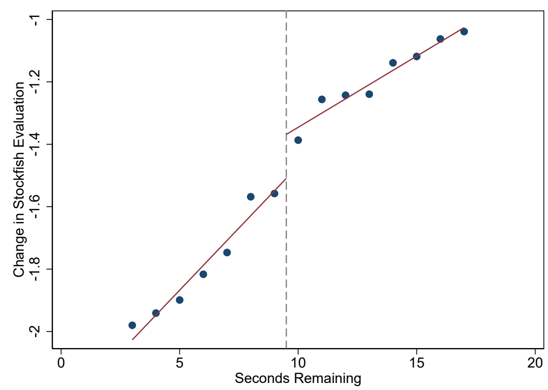
Source: Lichess.



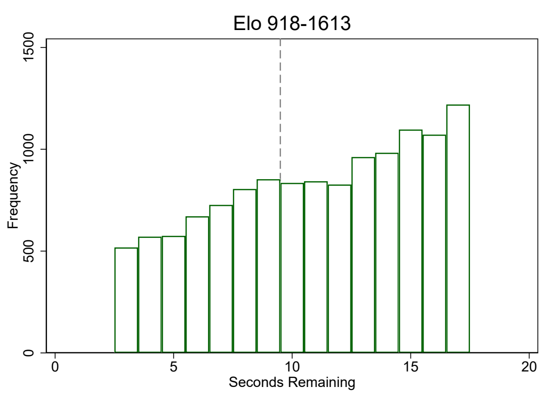
(a) The Distribution of Time Remaining After a Move



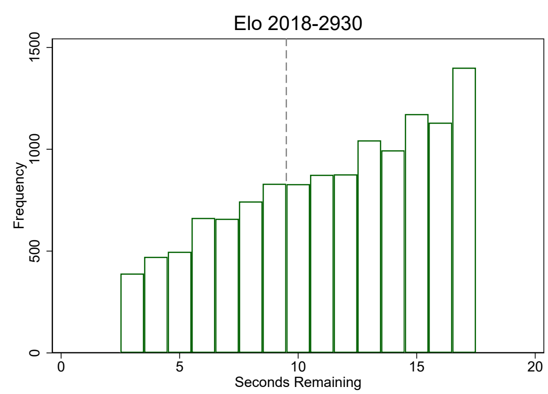
(b) The Amount of Time Spent



(c) The Change in Computer Evaluation



(d) The Distribution of Time Remaining After a Move, Low-Elo



(e) The Distribution of Time Remaining After a Move, High-Elo

Figure C.1: **The Role of the Low-Time Reminder, 30 second time control.** Panel (a) shows a histogram of the number of moves played by the number of seconds remaining. The number of seconds remaining is measured discretely. The beep occurs when there are 10 seconds remaining. Panels (b) and (c) show the average amount of time spend and the change in computer evaluation, by the number of seconds remaining at the end of the move. Panels (d) and (e) show the same as Panel (a), but only for the lowest and highest quintile of the mover's Elo.
Source: Lichess, author's calculations.

Figure C.2 shows all five quintiles of Elo for the distribution of time remaining after a move. There is significantly more bunching for the lower-rated players.

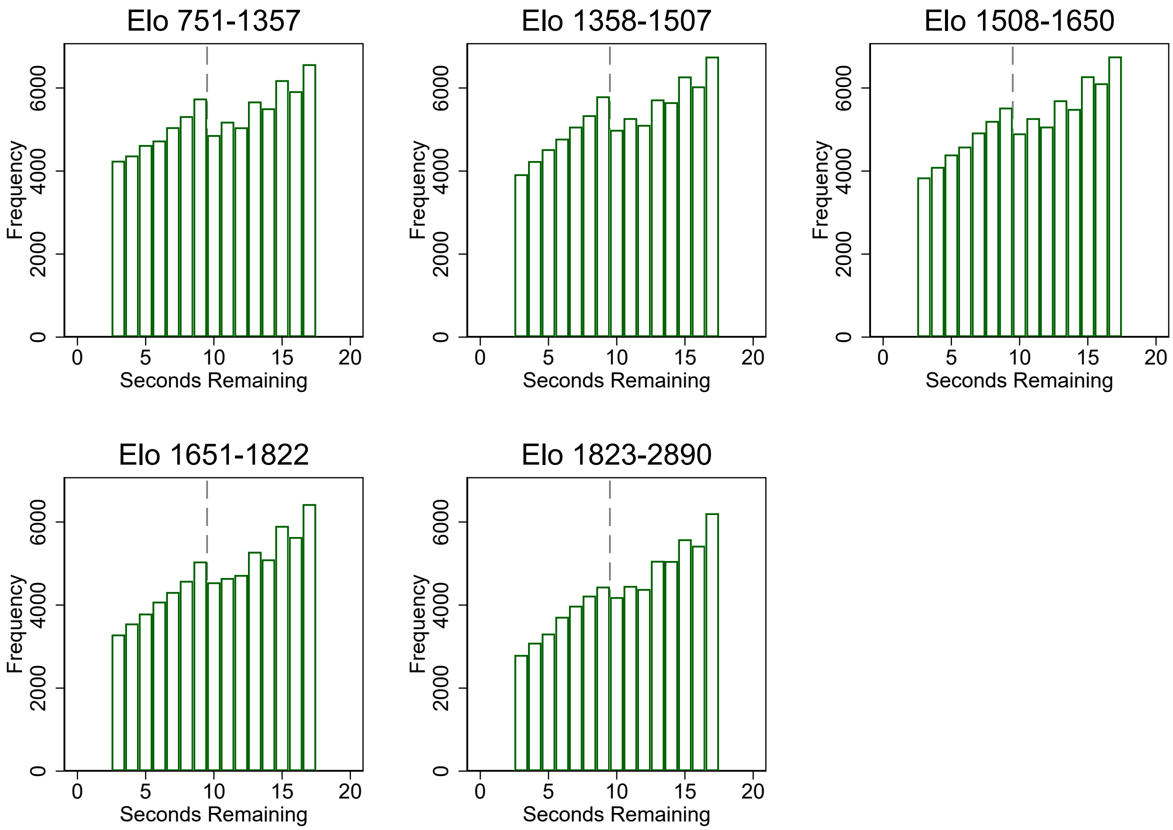


Figure C.2: **Distribution of the Time Remaining, Split over Elo-rating bins.** The figures shows a histogram of the number of moves played by the number of seconds remaining in 1 minute games. The number of seconds remaining is measured discretely. The beep occurs when there are 10 seconds remaining.

Source: Lichess, author's calculations.

Figures C.3 and C.4 show the robustness of Figures 2 and 3 to matching based on 100 bins of the initial Stockfish evaluation, instead of 50 bins.

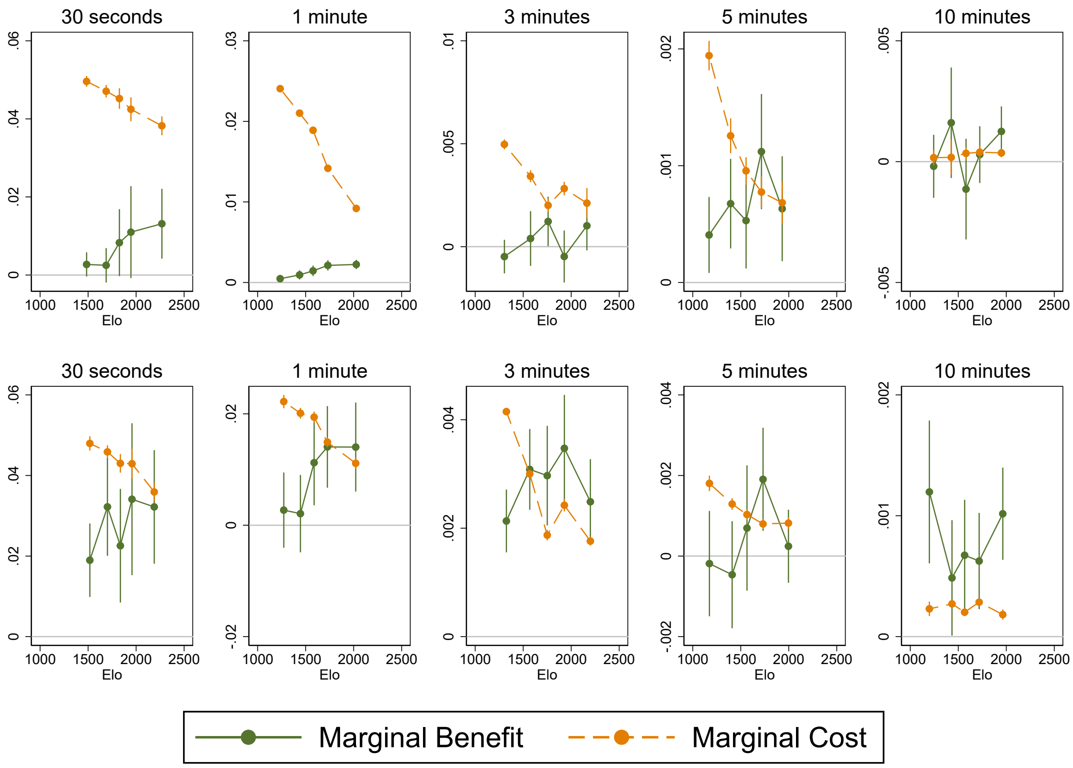


Figure C.3: **Heterogeneity in Marginal Benefit and Marginal Cost by Elo rating. Robustness to using 100 bins.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. The number above the panel indicates the seconds in the baseline time control.

Source: Lichess, author's calculations.

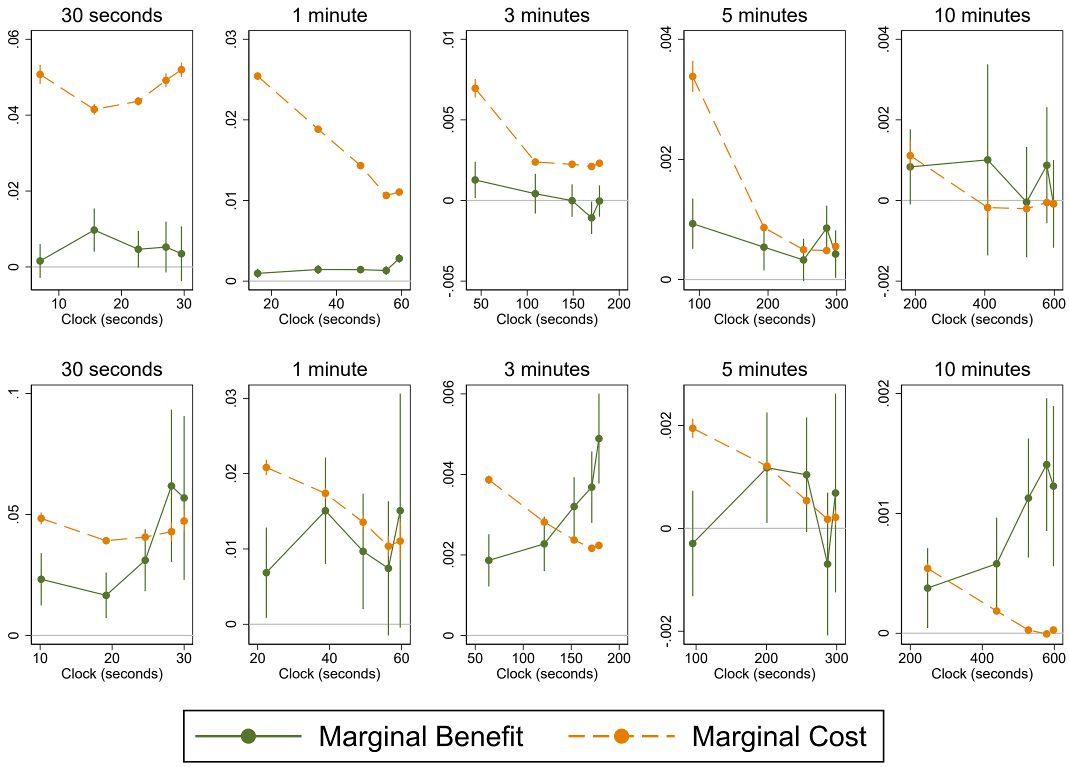


Figure C.4: **Heterogeneity in Marginal Benefit and Marginal Cost by time remaining. Robustness to using 100 bins.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. The number above the panel indicates the seconds in the baseline time control.

Source: Lichess, author's calculations.

Figures C.5 and C.6 show the robustness of Figures 2 and 3 to calculating the value function using LASSO splines instead of the local linear regression method in the main paper. Within each time control, and within five bins of Elo rating, cubic splines are created, with knots at -10, -3, -1, 1, 3, and 10 pawns for the evaluation, and 1/30th of the total time, 1/10th of the total time, 1/5th of the time, and 1/2 the total time for both the clocks. Knots were chosen to reflect the parts of the domain with significant curvature, as can be seen in Figure B.1. All the splines are fully interacted with each other, and the outcome variables are regressed on all the interaction terms, using LASSO to avoid overfitting in the relatively empty parts of the data.

The stata command lassoregress (Townsend, 2018) is used to fit the LASSO regression, with the tuning parameter being chosen through cross-validation.

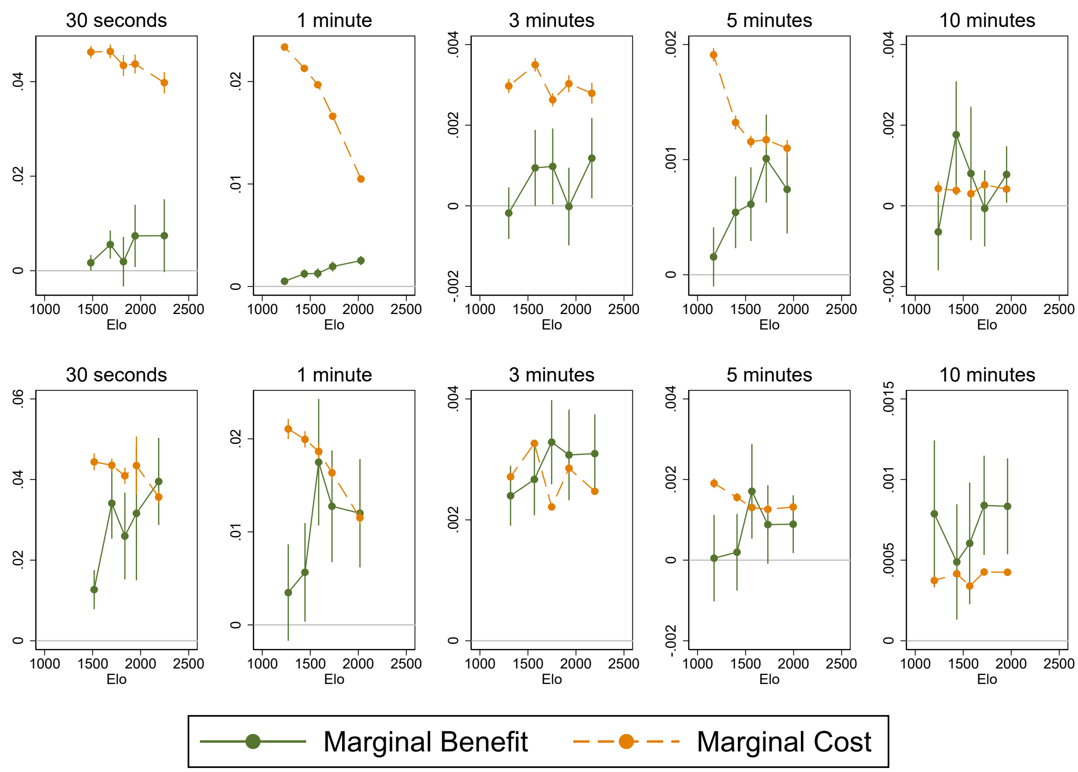


Figure C.5: **Heterogeneity in Marginal Benefit and Marginal Cost by Elo rating. Robustness to using LASSO splines.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. The number above the panel indicates the seconds in the baseline time control.
Source: Lichess, author's calculations.

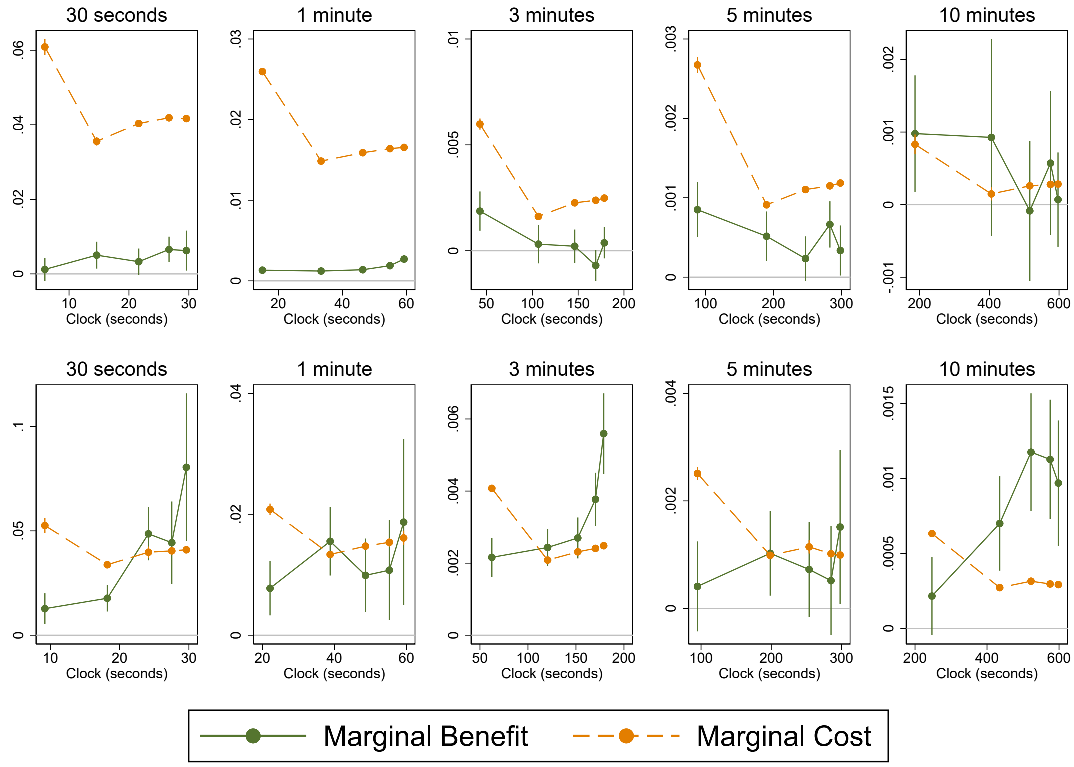


Figure C.6: **Heterogeneity in Marginal Benefit and Marginal Cost by time remaining. Robustness to using LASSO splines.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. The number above the panel indicates the seconds in the baseline time control.
Source: Lichess, author's calculations.

Figures C.7 and C.8 show the robustness of Figures 2 and 3 to additional controls in estimating the marginal benefit of the move. "Check/Capture Controls" drops pairs of moves unless the moves match on whether the opponent's last move was a check and/or a capture. "Number of Pieces Controls" drops pairs of moves unless the moves match on the number of pieces on the board.

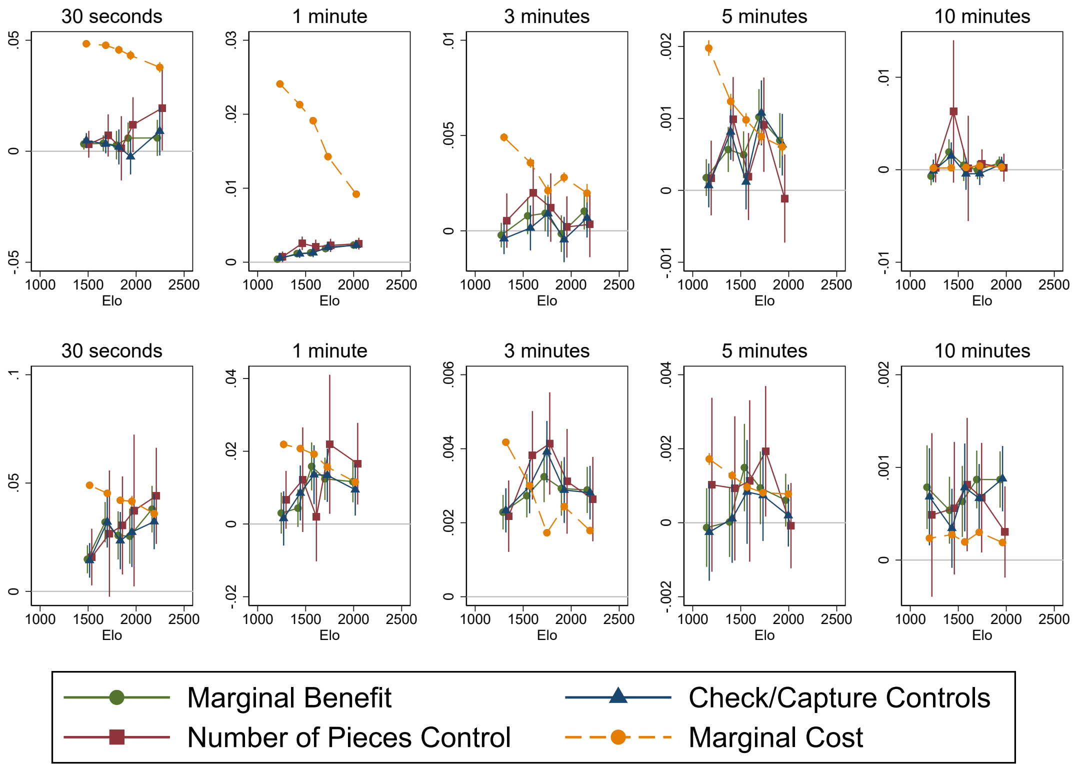


Figure C.7: **Heterogeneity in Marginal Benefit and Marginal Cost by Elo rating. Robustness to additional controls.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. The number above the panel indicates the seconds in the baseline time control.
Source: Lichess, author's calculations.

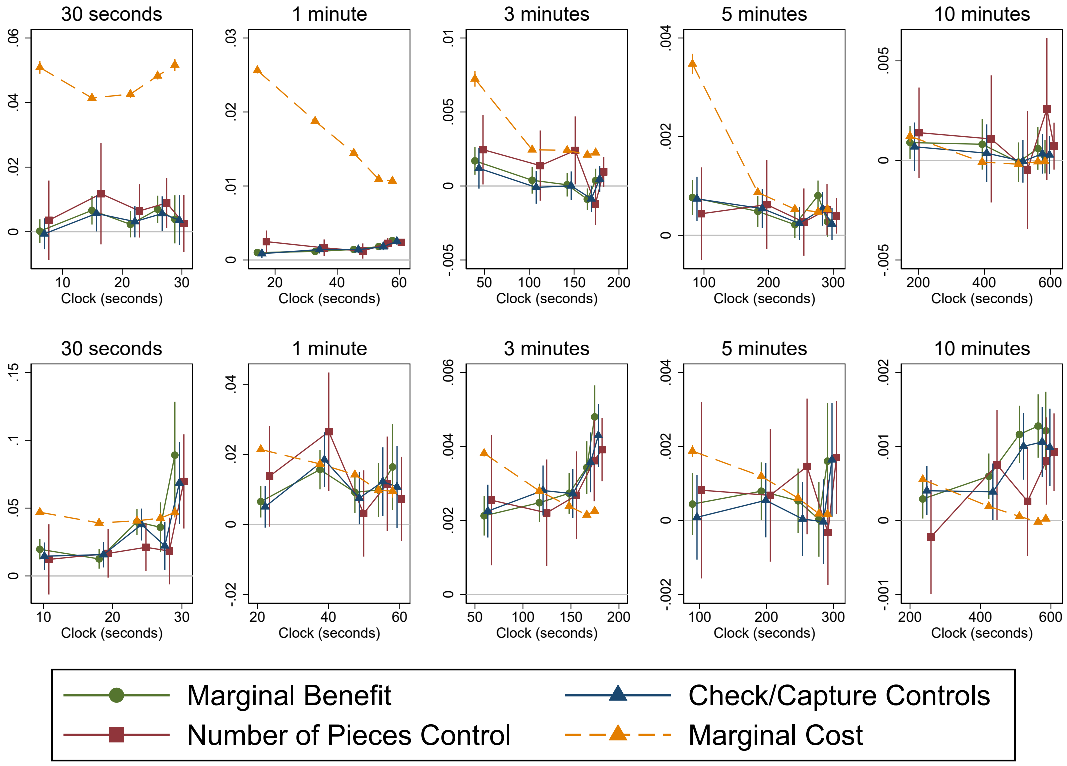


Figure C.8: **Heterogeneity in Marginal Benefit and Marginal Cost by time remaining. Robustness to additional controls.** The rational inattention prediction is that the marginal benefit should fall below the marginal cost in the top panels and vice versa in the bottom panels. The number above the panel indicates the seconds in the baseline time control.

Source: Lichess, author's calculations.

Table C.7 compares a few proxies for the difficulty of the position across matched moves. Only the results for the match to a faster time control are shown. For example, column (1) is a comparison of moves in 30 second time control to 15 second time control. Difficulty is compared in three dimensions: whether a capture was just made, whether the king is in check, and how many pieces are left on the board.

Table C.7: Balance table for looking at the differences in the likelihood that the previous move included a piece capture or check, or for the number of pieces on the board

Captures

	(1) 30 sec	(2) 1 min	(3) 3 min	(4) 5 min	(5) 10 min	(6) 15 min
Lower Time Control	0.00768	0.00124	-0.0000178	0.00453*	-0.000601	0.00181
	(0.00437)	(0.00391)	(0.00170)	(0.00226)	(0.00166)	(0.00348)
Constant	0.230***	0.237***	0.233***	0.216***	0.221***	0.208***
	(0.00219)	(0.00195)	(0.000851)	(0.00113)	(0.000828)	(0.00174)
Observations	33578	43518	225144	122336	229578	49668

Checks

	(1) 30 sec	(2) 1 min	(3) 3 min	(4) 5 min	(5) 10 min	(6) 15 min
Lower Time Control	0.00560*	0.000689	-0.00654***	-0.00495***	-0.000174	0.000966
	(0.00221)	(0.00195)	(0.000877)	(0.00118)	(0.000871)	(0.00190)
Constant	0.0438***	0.0496***	0.0555***	0.0530***	0.0510***	0.0517***
	(0.00110)	(0.000977)	(0.000438)	(0.000592)	(0.000436)	(0.000952)
Observations	33578	43518	225144	122336	229578	49668

Pieces on the Board

	(1) 30 sec	(2) 1 min	(3) 3 min	(4) 5 min	(5) 10 min	(6) 15 min
Lower Time Control	-0.451***	-0.0907***	-0.176***	-0.0928***	-0.0511***	-0.00193
	(0.0210)	(0.0194)	(0.00864)	(0.0119)	(0.00835)	(0.0180)
Constant	27.14***	26.59***	26.16***	25.80***	25.77***	25.78***
	(0.0105)	(0.00971)	(0.00432)	(0.00593)	(0.00418)	(0.00902)
Observations	33578	43518	225144	122336	229578	49668

Standard errors clustered by matched pair

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$