Do Sports Bettors Need Consumer Protection? Evidence from a Field Experiment

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Abstract

Corrective policy in sports betting markets is motivated by concerns that demand may be distorted by behavioral bias. We conduct a field experiment with frequent sports bettors to measure the impact of two biases, overoptimism about financial returns and self-control problems, on the demand for sports betting. We find widespread overoptimism about financial returns. The average participant predicts that they will break even, but in fact loses 7.5 cents for every dollar wagered. We also find evidence of significant self-control problems, though these are smaller than overoptimism. We estimate a model of biased betting and use it to evaluate several corrective policies. Our estimates imply that the surplus-maximizing corrective excise tax on sports betting is twice as large as prevailing tax rates. We estimate substantial heterogeneity in bias across bettors, which implies that targeted interventions that directly eliminate bias could improve on a tax. However, eliminating bias is challenging: we show that two bias-correction interventions favored by the gambling industry are not effective.

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1 Introduction

Until recently, sports betting was mostly illegal in the United States. The legal environment changed in 2018 when the Supreme Court overturned federal restrictions and made states the primary regulators of sports betting. Since then, legal sports betting activity has expanded rapidly. In 2023, 38 states had legalized sports betting in some form, and Americans wagered $121 billion on sports (American Gaming Association, 2024).

Proponents of sports betting legalization argue that consumption reflects the entertainment value of betting. Under this interpretation, deregulation improved consumer welfare by allowing the consumption of a new good. However, some critics argue that sports betting is at least partially driven by behavioral biases. One concern is that bettors may be overoptimistic about financial returns. Because the financial returns to sports betting depend on bettors' skill at picking winners, it is plausible that overoptimism could be worse for sports betting than for other kinds of gambling, like lotteries. A second potential bias is that bettors may have self-control problems – that is, they may bet more in the moment than they would like to have bet if they had chosen in advance. Self-control problems may be particularly difficult to resist for modern sports bettors, since most bets are placed on mobile devices rather than at brick-and-mortar sportsbooks. Motivated by these concerns, policymakers are considering a variety of interventions to protect consumers (Lipton and Draper, 2023).

In this paper, we present experimental evidence that behavioral bias affects sports betting consumption, and we combine this evidence with a model of biased consumption to evaluate various policy interventions. We report on results from a field experiment with a sample of high-volume sports bettors, using techniques from behavioral economics to estimate overoptimism and self-control problems. We first present reduced-form evidence on bias. Then, we use that evidence to estimate a structural model of biased betting demand. Using the estimated model, we evaluate the welfare effects of corrective policies such as taxes and interventions to directly reduce bias. Motivated by these counterfactuals, we conclude by providing additional experimental evidence on the effects of bias-correcting interventions as implemented by sportsbooks in practice.

We begin by presenting a model of sports betting consumption that defines the biases of interest: overoptimism about financial returns and self-control problems. The model allows sports betting to be driven by a true preference to gamble as well as by these biases. Biases cause some costs of betting to not be internalized in consumption choices, which implies that corrective policies can improve welfare. In particular, money-metric uninternalized costs determine the optimal corrective tax since the socially optimal level of betting arises when a tax causes consumers to internalize all net costs of betting (Diamond, 1973; Allcott and Taubinsky, 2015).

To study our model empirically, we conducted a field experiment with linked survey and observational consumption data for a sample of high-volume sports bettors. We focus on high-volume sports bettors because they are the population whom any policy interventions would most impact. Compared to a representative sample of weekly sports bettors, our sample was more educated and exhibited less bias according to qualitative survey measures, suggesting that our bias estimates are conservative for the population. All participants in the study took three surveys over two months. They were also required to sync their sportsbook accounts to our study via an online portal, allowing us to observe their consumption directly.

To measure overoptimism about financial returns, we elicited predictions of future financial returns in each survey. We compare average predicted returns to average realized net returns and interpret the difference as a measure of average overoptimism.

We find evidence of substantial overoptimism: the average participant predicted that they would break even, but in fact lost 7.5¢ for every dollar wagered. This result implies that the average participant internalized *none* of the expected financial costs of betting. We then estimate a distribution of overoptimism at the individual level. We use empirical Bayes methods to deal with the inherent noise in sports betting outcomes (Chen, 2024). We find substantial heterogeneity: 10% of participants are underoptimistic about financial returns, and another 10% are overoptimistic by more than 20¢/dollar. Finally, we show that overoptimism is larger for bettors who wager on multi-leg bets, or *parlays*, which validates concerns that these complex bets are particularly confusing for consumers.

To measure self-control problems, we elicited participants' valuations of the Bet Less Bonus, which was an experimental incentive to reduce future sports betting. Intuitively, agents who percieve that self-control problems will cause overconsumption in the future will value the incentive to bet less (Carrera et al., 2022). Separately, to estimate whether participants are sophisticated or naive about their self-control problems, we elicited predictions of future consumption and compare them to the truth (Augenblick and Rabin, 2019). We combine our estimate of perceived self-control problems with our estimate of naivete to estimate total self-control problems.

We find evidence of modest perceived self-control problems: participants were willing to pay 12% more for the Bet Less Bonus than they would have been if they had no self-control problems. When translated into dollar units, though, perceived self-control problems are much smaller than overoptimism. We also find no eveidence of naivete. Therefore, we treat our estimate of perceived self-control problems as a measure of true self-control problems.

With these bias estimates in hand, we estimate a specialized version of our model and conduct counterfactual simulations to evaluate corrective policies. We use multiple distinct strategies to estimate the price-sensitivity of demand, including a preferred approach that uses the predicted effects of price changes. With our preferred estimates, the implied optimal corrective tax on sports betting is 5.17¢ per dollar, about twice as large as prevailing tax rates. This tax would reduce betting by 31% relative to the status quo. Because of heterogeneous bias, a uniform tax provides only about half the welfare gains of a first-best policy, which motivates our investigation of targeted bias-correction interventions.

The two bias-correction interventions in our experiment mirrored real-world interventions proposed by sportsbooks. To address overoptimism about financial returns, some sportsbooks have recently displayed past winnings and losses more transparently on apps and websites. In our experiment, participants in the *history transparency treatment* viewed summary information about their past net returns. To address self-control problems, sportsbooks typically include an option for bettors to voluntarily set binding limits on their own future betting activity. In our experiment, participants in the *limits treatment* were prompted to make an active choice about whether they would like to use this tool to control their future consumption.

We find that the history transparency treatment did not reduce overoptimism, and the limits treatment only partially addressed self-control problems. The history transparency treatment did not reduce predictions of future returns on average because the average participant was not overoptimistic about their historical winnings – they were only overoptimistic about future winnings. This result is surprising given the prominence of selective memory accounts of overoptimism (Bénabou and Tirole, 2002); it is more consistent with models where agents remember the content of past signals, but misinterpret them (Thaler, 2024). In the limits treatment, some participants chose to set binding limits, partially mitigating overconsumption from self-control problems. However, many people who perceived themselves to have self-control problems chose not to restrict their future behavior. We provide suggestive evidence that uncertainty about the future demand for betting and the resultant demand for flexibility could drive the observed low takeup (Laibson, 2015). Overall, these findings suggest that the bias-correction interventions currently implemented by sportsbooks are not a panacea. To increase welfare in our model, a regulator would either need to create more effective "soft" bias-correcting interventions, or instead rely on "hard" interventions (such as taxes) to reduce overconsumption.

Our primary contribution to the literature is to provide novel estimates of bias in sports betting consumption and to integrate these estimates into a tractable economic model for policy analysis. A large body of work in psychiatry and medical literatures has established that some gamblers experience self-control problems (Potenza et al., 2019). A smaller literature has specifically documented overoptimism among sports bettors (Chegere et al., 2022; Donkor et al., 2023). We are the first to measure these biases in a way that can be incorporated into an economic model, which allows for policy evaluation.[1] Two complementary recent papers, Baker et al. (2024) and Hollenbeck et al. (2024), show that sports betting legalization caused adverse financial outcomes for households. While these results allow us to quantify some of the costs of sports betting, they do not engage with the question of whether consumers internalize those costs in their consumption choices.[2]

[1] Lockwood et al. (2021) conducts a welfare analysis of state-run lotteries. Interestingly, they find that lottery bettors do not overestimate the expected returns to lottery tickets on average, while we find that overestimation of financial returns is pervasive among sports bettors. This fact highlights that different kinds of gambling activity likely warrant different policy interventions.

[2] There is a broader reduced-form literature in economics on the impacts of legal gambling. See for example Guryan and Kearney (2010), Guryan and Kearney (2008), Muggleton et al. (2021), Evans and Topoleski (2002), Kearney (2005), Akee et al. (2015), and Gerstein et al. (1999). Much of this literature has focused on lottery and casino betting. Other

We connect to four other literatures in economics. First, we contribute to the literature on biased beliefs in gambling.[3] Our overoptimism measure can be interpreted as a reduced-form measure of the biased beliefs that arise from the various models proposed in this literature. Second, we contribute to a literature on belief measurement in the presence of elicitation noise (Gillen et al., 2019). We illustrate how to construct individual-specific estimates of latent beliefs using panel data with multiple noisy belief measurements. Third, we build on recent work that highlights the importance of targeting for the welfare effects of nudges and measures targeting in lab experiments (Ambuehl et al., 2022b,a; Allcott et al., 2022a; List et al., 2023). Our analysis of bias-correction interventions highlights the importance of targeting in a field context. Fourth, we contribute to the behavioral economics literature on commitment devices. We provide sugestive evidence that the demand for flexibility reduced the effectiveness of a real-world commitment device, consistent with theory (Laibson, 2015) and evidence from experimental commitment devices (Carrera et al., 2022).

Sections 2-9 present the background on sports betting in the United States, stylized model of mobile sports betting, experimental design, descriptive facts, reduced-form experimental results, structural model and estimation strategy, model-based policy analysis, and conclusion, respectively.

2 Context on Sports Betting in the United States

Modern sports betting differs from other popular kinds of gambling in ways that make concerns about overoptimism and self-control problems particularly salient. In the United States, policymakers can address these concerns with a variety of instruments, including both taxes and more specialized behavioral interventions that attempt to directly correct bias.

2.1 How Modern Sports Betting Works

In the United States, the vast majority of sports betting activity takes place on mobile devices. As of October 2024, 30 states and the District of Columbia allowed sportsbooks (private firms that offer sports bets) to accept wagers via cell phone apps and websites.[4] Mobile platforms accounted for 94% of sports betting revenues in 2023 (American Gaming Association, 2024). The prominence of mobile platforms is a departure from traditional forms of sports betting, which usually occurred at brick-and-mortar sportsbooks or casinos.

Mobile platforms could exacerbate self-control problems in gambling consumption. It is well-

papers specifically studying the rollout of legal sports betting in the U.S. include: Taylor et al. (2024); Couture et al. (2024); Matsuzawa and Arnesen (2024); Humphreys and Ruseski (2024).

[3] See, for example; Terrell (1994) and Suetens et al. (2016) on the gambler's fallacy; Snowberg and Wolfers (2010) on favorite-longshot bias; Donkor et al. (2023) on motivated reasoning; and Lockwood et al. (2021) on probability weighting.

[4] Source: https://www.americangaming.org/research/state-gaming-map-mobile/

known that some gamblers report feeling urges to gamble that are challenging to resist.[5] Combining gambling with smartphones could make it harder to overcome these urges. A public relations chair from Gamblers Anonymous articulated the concern as follows: "They have access to it 24/7 in the palm of their hands. The temptation is always there. You can stay away from casinos and racetracks but you can't stop using your phone" (Vice, 2022).

The expected financial returns to sports betting depend on the odds set by sportsbooks and on a bettor's skill at picking advantageous bets. The *hold rate*, or the share of total dollars wagered that are not paid out as winnings, is one measure of the average financial cost of betting. The average hold rate across U.S. sportsbooks was 9% (American Gaming Association, 2024). In principle, skilled bettors can overcome the hold rate and earn positive returns by identifying wagers with mispriced odds and exploiting those opportunities. In practice, it is challenging to consistently identify such pricing errors, because sportsbooks use sophisticated methods to set odds. Professional sports bettors often emphasize that amateurs underestimate how challenging it is to consistently beat the house.[6]

The relevance of skill in sports betting creates scope for biased beliefs. In games where skill is not relevant, such as lotteries, there is evidence that consumers are sophisticated about negative expected returns (Lockwood et al., 2021). In such games, there are usually few plausible reasons for a consumer to believe that one combination of numbers is a better deal than any other.[7] The psychology of sports betting may be different. Sports fans regularly consume media where commentators provide opinions and arguments about how teams and players will perform.[8] Such consumers could overestimate the extent to which they can predict sports outcomes and, therefore, overestimate the financial returns to betting.

Overoptimism may be particularly relevant for an increasingly prominent kind of bet called *parlays*. Parlays are bets where multiple outcomes must occur jointly for the bet to pay off. Sportsbooks have aggressively promoted parlays, in part because hold rates for parlays are much larger than for regular bets, anecdotally ranging from 20% to 30% (Greenberg, 2022). Given these large financial costs, they are surprisingly popular among consumers: in 2023, they accounted for 28% of dollars wagered in Illinois, the only state that reports this data. Critics worry that people do not understand how costly parlays are, possibly because it is challenging to aggregate independent probabilities of the component outcomes into the probability that the parlay pays off. The concern

[5]See Feeney (2023) for survey evidence. The psychiatric literature classifies gambling disorders in the same diagnostic category as substance use disorders related to alcohol and other drugs, in part because of evidence the underlying psychological and neurological pathways driving these urges are similar across these contexts (American Psychiatric Association, 2013; Potenza et al., 2019; Goudriaan et al., 2019)

[6]The legendary professional sports bettor Billy Walters writes in his memoir: "For the average bettor, [breaking even is] like trying to swim the English Channel at night, doing the backstroke, without a wetsuit. Surrounded by sharks." (Tamny, 2023)

[7]Though see Terrell (1994) for an example of a case where lottery tickers were in fact mispriced and Guryan and Kearney (2008) for evidence consistent with biased beliefs.

[8]This sports commentary is increasingly integrated with the sports betting industry: FanDuel sportsbook has launched its own TV station, FanDuel TV, and ESPN recently launched a sports betting platform, ESPN BET.

is also consistent with the view that compound lotteries are more complex than simple lotteries (Enke and Shubatt, 2023).

2.2 Existing Sports Betting Regulations in the U.S.

From 1992 to 2018, sports betting was regulated at the federal level under the Professional and Amateur Sports Protection Act (PAPSA). PAPSA made sports betting mostly illegal, except for limited licensed operations in Nevada, Delaware, Oregon, and Montana. In 2018, the Supreme Court overturned PAPSA in *Murphy v. NCAA*. The decision essentially ended federal regulation of sports betting, as Congress did not replace PAPSA with a new law.[9] Since *Murphy*, consumption has grown rapidly as more states allowed sportsbooks to operate legally: national dollars wagered grew at an annualized rate of 62% per year in the period from 2018 to 2023.[10] At the time of writing, there are active debates about whether to legalize sports betting in several states where it is still prohibited, including California and Texas.

Sports betting is directly taxed at the federal and state levels. The federal tax on gambling is a 0.25% excise tax on all dollars wagered. Separately, every state with legal sports betting taxes it in some way. Taxes are generally levied on sportsbook revenues, but states differ in the rate of taxation and in the extent to which sportsbooks can deduct expenses from their revenues. To harmonize state taxes with the federal excise tax, Figure 1 reports the share of dollars wagered that ended up as state revenues in 2023 across states. At the high end, states like New York taxed sportsbook revenue at 50%, which means they received 4.5% of all dollars wagered (since the average hold rate is 9%). Other states, like Nevada and Arizona, received less than 1% of dollars wagered. The final row shows that on average, sports betting in the U.S. was taxed at a rate of about 2¢ per dollar wagered.

Sports betting taxes are rapidly evolving. Ohio and Illinois both doubled their sports betting taxes in 2024, and other states are considering similar changes. On the other hand, the American Gaming Association (the trade group for the gambling industry in the United States) argues that the federal excise tax on sports betting should be repealed.[11] A primary goal of this paper is to provide empirical evidence and a theoretical framework that can inform debates about the optimal sports betting tax.

Aside from taxes, regulators have considered non-price interventions to ensure that users gamble responsibly. These interventions have taken many forms. We focus here on two prominent interventions that are designed to correct our biases of interest. To correct overoptimism, some have called for sportsbooks to provide users with transparent data on their past wins and losses. For example, in Australia, sportsbooks are required to email customers "meaningful statements

[9] It is still legal for the federal government to make policy; Congress has just chosen not to do so. *Murphy v. NCAA* is explicit that the federal government can pre-empt states. Justice Samuel Alito's majority opinion states (emphasis ours): "*Congress can regulate sports gambling directly*, but if it elects not to do so, each State is free to act on its own."

[10] https://www.sportsbookreview.com/news/us-betting-revenue-tracker/.

[11] https://www.americangaming.org/policies/hot-issue-sports-betting/

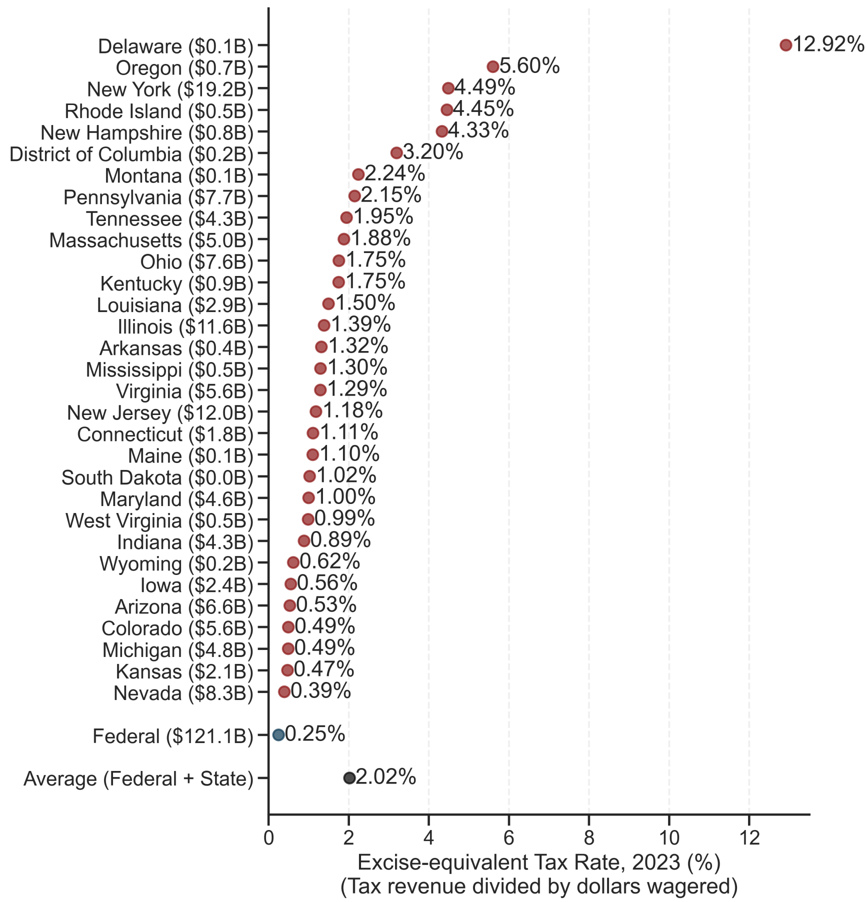

Figure 1: Summary of U.S. sports betting taxes in 2023

Notes: The figure summarizes sports betting activity and taxes in 2023. Labels report dollars wagered or "handle" in each jurisdiction for 2023. Points illustrate the share of dollars wagerd that states retain as revenues, or "excise-equivalent" taxes. For state taxes, the excise-equivalent tax is typically the product of a tax levied on sports betting revenues and the hold rate. Appendix Table B.1 includes more detailed statistics for each jurisdiction. At the bottom, we also report the size of the federal excise tax on gambling and the combined overall excise-equivalent rate. Data on state-level wagers and revenues was compiled by Legal Sports Report https://www.legalsportsreport.com/sports-betting/revenue/ (accessed September 19, 2024).

on their wagering activity" every month (Commonwealth of Australia, 2022).[12] To correct self-control problems, some states require sportsbooks to let users set binding limits on their wagering activity. As of 2022, more than two thirds of U.S. jurisdictions with legal sports betting required operators to provide a mechanism for users to self-limit.[13]

These bias-correction interventions have obvious upsides. They are light-touch, cheap to implement, and potentially well-targeted at heterogeneous bias (Thaler and Sunstein, 2008). However, skeptics have argued that in practice, they do not have material impacts and distract from

[12]No such requirement exists in the United States, but there are some examples at the state level. For example, sportsbook operators in Michigan are required to provide a "readily accessible" information page where users can obtain information on their "game history." (Michigan Gaming Control Board, 2020)

[13]https://www.americangaming.org/resources/responsible-gaming-regulations-and-statutes-guide

the need for larger-scale interventions (Rose-Berman, 2024; Chater and Loewenstein, 2022). Our study provides experimental evidence on the extent to which history transparency and voluntary limit tools accomplish their stated bias-correction goals.

3 A Model of Biased Sports Betting

In this section, we present a simple model of sports betting consumption where overoptimism about financial returns and self-control problems can cause overconsumption. The extent to which bias causes consumers to overvalue sports betting in money-metric units is a key statistic for optimal corrective policy (Diamond, 1973; Allcott and Taubinsky, 2015). Our experiment is designed to measure bias in these units.

3.1 Setup and Bias Definitions

We study a static sports betting consumption decision: an agent i chooses consumption x_i in a given period. We interpret x_i as the choice of how many dollars to wager. We abstract from the dynamics of consumption and from other dimensions of choice (such as choices about which individual bets to place), since these are not the focus of our empirical investigation. When conducting counterfactual simulations, one way to interpret this simplification is that these other dimensions of choice are held fixed.

The agent can derive financial and nonfinancial value from sports betting. The financial value of betting arises because if the agent wins, they can use their winnings to consume a numeraire good. The nonfinancial value of betting is a reduced-form object capturing all reasons for betting other than the agent expecting to consume their winnings. Overoptimism about financial returns and self-control problems cause the agent to choose as if they have higher financial and nonfinancial value respectively.

To model the financial value of betting, we define $a \in [-1, \infty)$ as the *realized net return*. The realized net return is -1 if the agent loses all of his money, 0 if he breaks even, and > 0 if he earns money. The distribution of net returns is governed by a cumulative distribution function F_i. Indexing the returns distribution by i allows expected returns to vary across agents, for example, because some bettors are more skilled than others. After net returns are realized, the agent receives his exogenous endowment y_i plus his total returns $x_i \cdot a$ as numeraire consumption.

We allow for overoptimism by distinguishing between the true return distribution F_i and the perceived return distribution $\tilde{F}_i$. We focus on overoptimism about expected returns, defining $\gamma_i^O = E_{\tilde{F}_i}[a] - E_{F_i}[a]$.

To model the nonfinancial value of betting, we define a reduced-form nonfinancial subutility function $z_i(x_i; \tilde{F}_i)$, which we assume is concave.[14] This function captures reasons for enjoying

[14] Concavity captures the diminishing marginal entertainment value of betting. One interpretation is that betting has time costs as well as financial costs, and the opportunity cost of time rises as the agent places more bets.

betting other than the consumption of future winnings. For example, over 80% of participants in our experiment agreed that "betting makes watching sports games more enjoyable." We allow for heterogeneous nonfinancial utility: since z is indexed by i, different agents may convert dollars wagered into utils at different rates. In this sense, our framework allows for general differences in preferences over betting. We also allow perceived financial returns to directly affect the nonfinancial utility of betting, since $\tilde{F}_i$ enters as an argument.

We incorporate self-control problems by adding a temptation parameter to the model, following Banerjee and Mullainathan (2010). In the moment, the agent chooses as if his nonfinancial utility is $z_i(x; \tilde{F}_i) + \gamma_i^{SC} x$ rather than $z_i(x; \tilde{F}_i)$. The temptation parameter γ_i^{SC} captures the extent to which compulsions cause the agent to bet more than would be normatively optimal for himself, given his understanding of his net returns distribution.

We assume quasilinear utility for ease of exposition and empirical tractability. Normalizing the marginal utility of numeraire consumption to one, we can then define a simple *decision utility function*, which the agent maximizes.

$$
\begin{aligned}
u_i^{decision}(x; \tilde{F}_i) = \quad & \overbrace{y_i + E_{\tilde{F}_i}[a] \cdot x}^{\text{Expected utility from numeraire consumption}} \quad + \quad \overbrace{\gamma_i^{O} \cdot x}^{\text{Effect of overoptimism}} \\
+ \quad & \underbrace{z_i(x; \tilde{F}_i)}_{\text{Nonfinancial utility from betting}} \quad + \quad \underbrace{\gamma_i^{SC} \cdot x}_{\text{Effect of self-control problems}}
\end{aligned}
\tag{1}
$$

We next define the *normative utility function*, which governs the agent's welfare, as the utility function that the agent would maximize if they were unbiased.

$$
u_i^{normative}(x; \tilde{F}_i) = y_i + E_F[a] \cdot x + z_i(x; \tilde{F}_i) \tag{2}
$$

In this setup, biased agents consume as if they overvalue gambling by $\gamma_i^O + \gamma_i^{SC}$ per dollar wagered. Since these parameters are defined in units of numeraire consumption (which we interpret as money) per consumption unit, we refer them as *price-metric biases* (Bernheim and Taubinsky, 2018).

3.2 Graphical illustration and connection to experimental design

We illustrate the bias defintions in a demand curve diagram in Figure 2. We define downward-sloping demand curves as a function of the implicit price of betting (i.e, expected losses) for in-the-moment choices which are subject to temptation ("short-term demand") and choices made in advance which are not ("long-term demand"). An unbiased agent would consume at the point where their long-term demand curve intersects the true price of betting (point A). Adding overoptimism causes the agent to underestimate the price of betting and, therefore, overconsume (point B). The vertical distance between the perceived price and the true price is the overoptimism parameter γ_i^O. Adding self-control problems causes choices to be driven by short-term demand

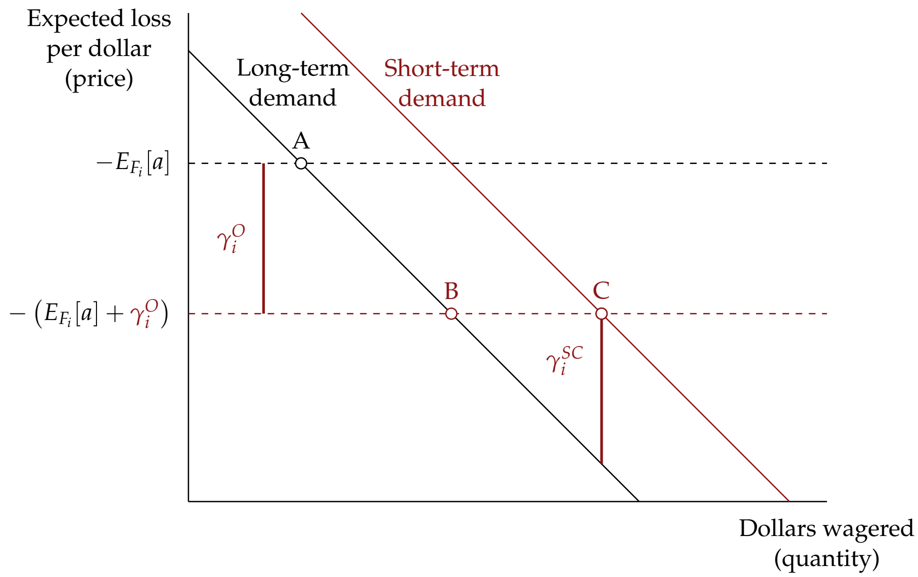

Figure 2: Overconfidence and self-control problems as price-metric biases

Notes: The figure illustrates the definitions of price-metric bias and the effects if bias on consumption. Long-term demand represents choices the agent would make if choosing in advance, and short-term demand represents choices they would make if choosing in the moment. Point A represents unbiased choices, point B represents choices with overoptimism only, and point C represents choices with self-control problems and overoptimism. The linear functional form of demand is only for illustration.

rather than long-term demand (point C). The vertical distance between the short-term demand curve and the long-term demand curve is the self-control problems parameter γ_i^{SC}. Since both biases are defined in price units, they both appear on the graph as vertical distances.

These price-metric biases are crucial objects for policy design (Allcott and Taubinsky, 2015). They represent net costs of betting that are not internalized in consumption decisions. Policy interventions can improve welfare by internalizing these costs. For example, in Figure 2, a corrective tax could induce a price change equal to the sum of the price-metric biases. Doing so would shift consumption to point A, which maximizes normative utility.

Motivated by the policy relevance of price-metric bias, we designed our experiment to measure overoptimism and self-control problems in these units. Our experiment is not designed to isolate the mechanisms driving either bias, and we do not take a stand on any particular microfoundations that could generate either bias. Instead, we view γ_i^O and γ_i^{SC} as reduced-form objects capturing the extent to which overoptimism and self-control problems cause net costs of betting to not be internalized in decisions, whatever the underyling behavioral model of bias.[15]

[15]In the language of Chetty et al. (2009) and Chetty (2015), these parameters are the sufficient statistics for welfare analysis that arise from the underlying behavioral model.

3.3 Discussion

While we allow for general heterogeneity in bias across people, we impose that within person, the bias parameters γ_i^O and γ_i^{SC} are constant. This assumption makes our counterfactual analysis tractable by allowing us to hold γ_i^O and γ_i^{SC} fixed when considering policies that affect consumption. One violation of constant bias is if people are more or less overoptimistic when they are wagering fewer dollars. For readers concerned about such issues, we note that our experimental measures of bias have natural interpretations even when bias can vary with consumption.[16]

Quasilinear utility is another important simplification. In Appendix C.1, we study a more general version of the model with nonlinear numeraire consumption utility. In Section 8.3, we return to evaluate the implications of this simplification in the context of our empirical results.

Our welfare analyses rely on normative assumptions that overoptimism and self-control problems are indeed biases rather than nonstandard preferences. We briefly clarify the nature of these assumptions and justify them below.[17]

We only treat overoptimism as a bias to the extent that it causes misperceptions of financial returns. Other effects of overoptimism are modeled as changes to the normative utility function. Misperceptions of financial returns cause the agent to misunderstand the consequences of their actions: they think betting is less costly than it is. Such misperceptions are objective mistakes and the case for intervening to correct them is strong. Therefore, they do not appear in the normative utility function (2). Of course, overoptimism may also cause the agent to bet more for other reasons – for example, he may find it more fun to gamble when he thinks he is going to win. There is no obvious reason for the planner to treat these other reasons for betting as normatively irrelevant, so we treat these effects of overoptimism as true preferences, not biases.[18] In Appendix C.2, we consider an extension where overoptimism can cause misperceptions of nonfinancial utility. There is an additional subtlety that only applies to welfare analysis of overoptimism-correcting interventions: if agents enjoy being overoptimistic, then correcting overotpimism imposes additional costs.[19] We rely on a stronger assumption to deal with this case, which we state explicitly in Section 8.1.

Considering self-control problems, our normative assumption amounts to the view that long-run preferences are associated with true well-being rather than short-run preferences. This so-called *long-run criterion* is sometimes disputed on conceptual grounds (Bernheim, 2016). However, in our context, it is consistent with neorobiological evidence that problem gamblers are unable

[16]Let $\gamma_i^O(x), \gamma_i^{SC}(x)$ denote overoptimism and self-control problems that can vary with dollars wagered x. Our estimates of overoptimism correspond to estimates of average overoptimism between 0 and the chosen level of consumption, $\left(\int_0^{x_i} \gamma_i^O(x)dx\right)/x_i$. Our estimates of self-control problems correspond to average self-control problems at the chosen level of consumption, $\gamma^{SC}(x_i)$.

[17]See Bernheim and Taubinsky (2018) for a more detailed discussion of normative approaches in behavioral public economics.

[18]Formally, the assumption is that perceptions $\tilde{F}_i$ enter nonfinancial utility identically in the normative and decision utility functions.

[19]An influential literature considers models where biased beliefs are chosen optimally to achieve functional goals, e.g, Bénabou and Tirole (2002).

to appropriately weigh the consequences of their actions when they experience compulsions to gamble (Potenza et al., 2019).

4 Field Experiment

4.1 Overview

Participants in our experiment shared data on their sports betting activity and completed three surveys over a two-month period. Table 1 presents sample sizes over the course of the experiment.

Our intake procedures were designed to recruit a sample of real-world, high-volume sports bettors. We focus on high-volume bettors because they account for the vast majority of sports betting consumption.[20] To reach this population, we conducted a targeted social media ad campaign. Appendix Figure A.1 presents a sample advertisement, and Appendix Table A.1 summarizes our targeting procedures. Upon clicking our ad, participants began an intake survey with an initial eligibility screening module. We excluded participants unless they self-reported betting on sports at least once a week and wagering at least $100 over the last 30 days. 6,155 people satisfied these eligibility criteria, and 2,062 agreed to participate after viewing the introductory materials.

Participants were required to share data on their sports betting activity. Our data collection procedure was designed to ensure that we observed the near-universe of sports wagers placed by our participants. Before we told participants about the data sharing requirements, we asked them whether they used each of six popular sportsbooks: DraftKings, FanDuel, BetMGM, ESPN BET, Caesars, and Hard Rock Bet. These sportsbooks account for the vast majority of legal sports bets in the U.S.[21] After we elicited this list, we asked participants to sync their accounts to the research study via a portal that we developed with an external company, SharpSports. We provide a screenshot of this portal in Appendix Figure A.2.

For participants who complied with syncing requirements, we observe detailed information about every bet placed between Jan 1, 2024 through the end of the study on June 10, 2024. This observational data has several advantages relative to self-reports. Most importantly, given that we are studying misperceptions, we did not want to rely on potentially misreported consumption and returns data as the ground truth. The data also contain detailed bet features for every wager.[22]

Participants received surveys 1, 2, and 3 on April 9, May 10, and June 10, respectively. Throughout this paper, we define period 1 to be the 30-day period from April 10 to May 9 and period 2 to

[20] Forrest and McHale (2024) show, using data from U.K.operators, that the top 5% of bettors accounted for 64% of sportsbook revenues in 2023.

[21] According to Baker et al. (2024), DraftKings and FanDuel alone account for more than 70% of sports betting transactions.

[22] Our experimental paradigm has some practical advantages that extend beyond the present study and could be adapted for future sports betting research. Most previous work that exploits observational data on sports betting relies on proprietary datasets that are provided by sportsbook operators. Our approach does not require a research agreement with a sportsbook. Also, since our approach links survey data with observational sports betting consumption data, it is easily adaptable for future studies – researchers need only change the survey questions.

Phase	Date	Sample Size
Recruitment and intake	March 13 - April 8	545,197 viewed social media ads 12,912 clicked on ads 6,155 satisfied initial eligibility criteria 2,062 consented and provided contact info 666 synced at least one account 555 synced all accounts
Survey 1	April 9	533 completed survey 1
Survey 2	May 10	486 completed surveys 1 and 2
Survey 3	June 10	472 completed surveys 1, 2, and 3, of which 447 provided data for all accounts through June 10 and 444 were also not in the MPL group (analysis sample)

Table 1: Experiment timeline and sample sizes

Notes: The table illustrates the number of unique participants who completed each step of our study.

be the 30-day period from May 11 to June 9, so that period t is always the period following survey t. We define pre-periods 0, -1, -2, and -3 similarly as 30-day periods counting backwards from April 8.

All randomized treatments were implemented in survey 1, so we undertook procedures to minimize attrition among the 533 participants who completed survey 1 (Appendix A.1). Overall, 516 (97%) completed at least one follow-up survey and provided follow-up data for at least one account, and 447 (84%) completed both surveys 2 and 3 while providing complete betting data. To define our final analysis sample of 444 participants, we exclude three participants who were randomized into a small treatment condition that ensured the incentivization of multiple price list elicitations. We discuss selection into the sample and differential attrition in Section 5.3.

We pre-registered our experiment at the AEA RCT registry (AEARCTR-0013310). We included a pre-analysis plan which specified the analysis sample, variable definitions, and regression specifications for estimation of randomized treatment effects. We discuss differences from the pre-registered analysis and present all pre-registered estimates in Appendix D.

4.2 Measuring Overoptimism About Financial Returns

In the first module of every survey, we asked:

> *How much do you expect to gain or lose for every $100 that you wager on [synced apps] over the next thirty days?*

We compare these predictions to true net returns to measure overoptimism γ^O. In surveys 1 and 2, we randomized whether these predictions were incentivized.

To ensure that responses coincided with the target beliefs, we guided participants through a structured elicitation procedure. For example, the procedure required participants to confirm their understanding that "breaking even" corresponds to winning $0 per $100. We also invited respondents to voluntarily revise their responses if they predicted expected net returns outside the range $[-0.4, 0.25]$. As we pre-registered, we truncate all net return predictions to lie within this range.

To enrich our understanding of perceptions, we asked analogous questions about past returns. In surveys 1, 2, and 3, participants were asked to recall their net winnings per $100 wagered in the period from Jan 1 to April 7, $t = 1$, and $t = 2$, respectively. In survey 1, we also asked participants to estimate the average net returns of American sports bettors in 2023. These questions were incentivized for all participants.

4.3 Measuring Self-control Problems

We designed the study to separately estimate perceptions of self-control problems and naivete about about self-control problems. We define perceived self-control problems $\tilde{\gamma}_i^{SC}$ as the agent's prediction about how much their future self overvalues overvalues sports betting. We allow the agent to incorrectly underestimate his own self-control problems, defining naivete $\tilde{\gamma}^{SC} - \gamma^{SC}$ to be the difference between perceived and true self-control problems. Adding these two objects yields γ^{SC}, which is the parameter that governs distortions in sports betting consumption.

Our design follows a recent literature in behavioral economics that uses similar methods to estimate models of time-inconsistency with partial sophistication (Acland and Levy, 2015; Chaloupka et al., 2019; Augenblick and Rabin, 2019; Carrera et al., 2022; Allcott et al., 2022c,b). This approach has a number of advantages relative to alternative popular designs (DellaVigna and Malmendier, 2006; Read and van Leeuwen, 1998). We discuss these advantages in Appendix B.2.

Measuring Perceived Self-control Problems To measure perceived self-control problems, we created an incentive called the Bet Less Bonus. In survey 1, we introduced it to all participants with the following text:

> *You may have the opportunity to earn money by betting less on sports over the next 30 days!*
>
> *If you are selected for the Bet Less Bonus, you will receive a $6 payment for every $10 that you reduce your average daily betting, up to a maximum bonus of $[M].*
>
> *You'll only get paid if you wager less than $[B] per day, which is slightly more than how much you've been wagering recently.*

The benchmark B and maximum payment M were replaced with personalized values based on participants' past betting consumption.[23] For bettors who wagered less than $B·30 in the relevant

[23] The benchmark B was the average daily wagers on baseline supported accounts in 2024, rounded up to the nearest $10. The maximum payment M is $\max\{B/10 \cdot 6, 90\}$. This says that we capped bonus payments at $90.

period, the bonus corresponded to a 2¢ increase in the price of wagering $1.[24] The payment arrived at the end of survey 3.

Valuations of the Bet Less Bonus identify perceived self-control problems because if someone predicts that their future self will bet too much, they will place higher value on an incentive to bet less in the future. To elicit valuations of the Bonus, we used a multiple price list (MPL). Participants made a series of binary choices between receiving the Bet Less Bonus and rceiveing a fixed payment of varying size.[25] We incentivized the MPL choices by informing participants that they may be randomly selected to have their choices "count," in which case one of the MPL options would determine their payment. In practice, 0.5% of participants were randomized to have the MPL choices count. For the other 99.5% of participants, we randomly assigned whether they would receive the Bonus. The Bonus's treatment effect on consumption allows us to measure price-sensitivity. Participants were assigned to the treatment group with probability 0.3475 and the control group with probability 0.6475. All randomized treatments were independently cross-randomized, stratified by period-0 net returns and period-0 dollars wagered.

Since the reasoning that causes perceived self-control problems to increase bonus valuations is somewhat subtle, we took several steps to ensure that participants understood the decision. One simple but important choice was about framing: we called the incentive the "Bet Less Bonus" rather than something more neutral to clarify that the incentive would directionally reduce future consumption. We also asked participants to predict how the Bonus would affect their consumption using an interactive screen that transparently mapped consumption reductions into Bonus payouts. Finally, following Allcott et al. (2022c), we included the following text to bring relevant considerations to mind:

> *How might you decide?*
>
> *You told us that if you are selected for Bet Less Bonus, you expect to wager $[Prediction]. Thus, you expect that you would earn a Bet Less Bonus of $[ExpectedBonus].*
>
> *You might prefer $[ExpectedBonus] instead of the Bet Less Bonus if you don't want any pressure to bet less.*
>
> *You might prefer the Bet Less Bonus instead of $[ExpectedBonus] if you want to give yourself extra incentive to bet less.*

Measuring Naivete We measure naivete about self-control problems by comparing predic-

[24] Since the bonus was active for 30 days, a $10 reduction in daily wagers corresponds to a reduction in wagers of $300 across the whole period. Therefore, the $6 payment corresponds to a subsidy of $0.02 per dollar of wager reductions. We framed the bonus as a subsidy for reducing daily betting rather than a subsidy per dollar reduced because participants were inattentive to the latter framing in pilots.

[25] We treat the lowest fixed payment where a participant chose the bonus as an upper bound on their bonus valuation, and the highest fixed payment where they chose the payment as a lower bound. We refined valuation estimates further by asking participants to state the exact fixed payment which would have made them indifferent between a fixed payment and the bonus. When this self-reported indifference point falls within our MPL bounds, we use it as the bonus valuation. When it does not, we use the midpoint of the MPL bounds.

tions of future dollars wagered to the truth. We infer naivete if people systematically underestimate future consumption. Intuitively, people who underestimate their future self-control problems will also underestimate their future consumption, while people who are sophisticated about self-control problems will not underestimate future consumption. As with the predictions of future returns, we used a structured elicitation procedure to minimize the influence of misunderstandings, and we randomized whether the prediction was incentivized.[26]

4.4 Bias-correction Interventions

We created experimental treatments to evaluate two interventions that sportsbooks have implemented with the express goal of reducing bias. These interventions are history transparency to address overoptimism and voluntary wager limits to address self-control problems.

Addressing Overoptimism: History Transparency In the *history transparency treatment*, participants receive information about their past net returns. The treatment mimics interventions like DraftKings's *My Stat Sheet*, which provides similar information about historical activity at a single sportsbook.[27] Similar interventions that provide information about past performance have been studied in various lab (Möbius et al., 2022) and field (Carrera et al., 2022) settings.

We randomized 50% of participants to be treated. They viewed the following text:

> *You said you [won/lost] $[recollection] for every $100 that you wagered.*
>
> *In fact, you [won/lost] $[realization] for every $100 that you wagered.*
>
> *This calculation used data from [number] bets on [synced accounts] in 2024.*

After viewing the information, participants answered an open-ended question about whether their recollection was close to the truth. Then, participants were allowed (but not forced) to update their predictions about the future.

We study whether the intervention reduced bias on average, and whether the intervention's treatment effects were correlated with pre-intervention bias.

Addressing Self-control Problems: Voluntary Wager Limits In the *limits treatment*, participants were prompted to make an active choice about how to use sportsbook weekly wager limit tools. These tools allow users to cap the amount that they are allowed to wager in a given calendar week. These caps are binding, at least in the short term: if a user attempts to remove the cap, the change will not take effect until after a cooling-off period. Every sportsbook in our study has some version of a weekly limit tool.

[26] To make sure people did not neglect changes in the sports schedule over time, we began by asking participants qualitative open-ended questions about which sports they might bet on over the next 30 days vs. the past 30 days. Then, to make sure participants understood the scale of their own past betting, we told them their own dollars wagered over the past 30 days. We next asked whether they expected to bet more, less, or about the same over the next 30 days compared to the past 30 days. Finally, we elicited a numerical prediction of future dollars wagered.

[27] The express goal of *My Stat Sheet* is to "help customers evaluate their play and make informed choices." DraftKings press release: https://draftkings.gcs-web.com/news-releases/news-release-details/draftkings-launches-my-stat-sheet-new-tool-promote-responsible

We framed limit tools as a way for participants to control their own sports betting activity. In survey 1, we elicited a stated "ideal" weekly wager volume for all participants with the following question:

> *Ideally, in the next 30 days, how much would you wager per week on [synced accounts]?*

Then, the 50% of participants in the limits treatment viewed the following text.

> *For this study, you are required to choose a weekly wager limit for your [synced accounts].*
>
> *If you don't want to limit your betting, that's fine! In that case, you can set very high limits, like $9,999,999. Then you wouldn't be restricting your betting at all. The only requirement is: you must type some number into the weekly limit box for each of your synced accounts.*
>
> *We'll give you video instructions about exactly how to do that in couple of pages.*

We confirmed that participants understood these instructions with comprehension questions.

Next, we asked participants to plan the weekly wager limits across their synced accounts. We included a reminder of their previously stated ideal on the planning screen.[28] Then, we provided video instructions on how to set limits for each account in succession. After showing the video instructions, we asked participants which limits they actually chose. These final self-reports are our measure of chosen limits.

We study whether participants chose restrictive limits, and whether restrictiveness correlates with our measures of self-control problems. We interpret these choices as the restrictiveness that people would choose absent hassle costs and informational frictions.[29] Outside of our experiment, most U.S. bettors do not use limits at all.[30] In other contexts, such as Norway (Auer and Griffiths, 2022) and Italy (Calvosa, 2017), players are required to make choices about limit choices upon account creation. Our results are informative about the plausible impacts of such a regulation in the American market.

4.5 Qualitative Measures of Bias and Other Supplemental Variables

We collected several qualitative measures of overoptimism and self-control problems. The *Gambling Literacy Index* (Wood et al., 2017), measures the extent to which gamblers understand that they cannot beat the house. We interpret it as a proxy for overoptimism. The *Problem Gambling Severity Index* (Holtgraves, 2009) is the most prominent screening tool for problem gambling among practitioners. We interpret it as a proxy for self-control problems. Appendix A.2 contains the

[28]The reminder read: *For reference, you told us earlier that in upcoming weeks, you'd ideally wager a total of $[ideal] per week on [synced apps].* If the chosen limits differed from the ideal, we displayed a message indicating this fact.

[29]For example, the screen where a user can set a limit is sometimes hidden behind opaque menus

[30]DraftKings reported to the Massachusetts Gaming Commission that less than 5% of users in that jurisdiction used limits (Massachusetts Gaming Commission, 2023). In our sample, 93% of users had heard of self-imposed limit tools before, but only 7% of participants had used them.

pre-registered questions and definitions for both indices. In addition to these measures from the literature, we asked participants whether they thought they were sports betting "too little," "too much," or "the right amount." This question is another qualitative measure of (perceived) self-control problems. Allcott et al. (2022b) ask a similar question for other goods, which allows for an easily interpretable comparison of sports betting to other contexts.

We also asked participants to predict their responses to hypothetical changes in the price of betting. These questions provide supplemental evidence on price-sensitivity to complement the randomized treatment effect of the Bet Less Bonus. In survey 3, we asked about the following two scenarios:

> *Suppose that all sportsbooks made their odds 2% worse for an extended period of time. How much, if at all, would you reduce your betting?*
>
> *Suppose you learned that you were 2% worse at making money betting than you'd thought. How much, if at all, would you reduce your betting?*

We elicited answers in percent terms. These questions let us measure predicted responses to naturalistic price changes.

Another kind of hypothetical price change was a change in the Bet Less Bonus's payment rate. We asked participants to predict consumption under payment rates of $\{0.01, 0.02, 0.04, 0.083\}$¢ per dollar. We varied the payment rates within participant and on the same decision screen, so these responses tell us about how much the price change from the Bet Less Bonus affected predictions when that price change was salient. These questions also appeared on survey 3.

These prediction questions were not pre-registered. We added them to the survey after we observed the treatment effect of the bonus in survey 1, with the goal of improving our interpretation of that effect and improving our price-sensitivity analysis.

We also asked various other qualitative and open-ended questions, which we will describe as they become relevant.

5 Sample Characteristics

5.1 Betting Activity

Figure 3 summarizes betting activity in the 30 days before the study began. The median participant wagered \$153/week in the pre-study period, confirming that our recruitment procedures successfully targeted high-volume bettors. This wager volume would put the median member of our sample close to 95th percentile of wager volume among all sports bettors (Forrest and McHale, 2024).[31] Even within our sample, bet volume has a long right tail – more than 25% of our sample

[31] We draw this conclusion by compring \$153/week to results in Forrest and McHale (2024), who report on the distribution of dollars wagered on sports over a year-long period in the U.K. from 2018-2019. That paper uses a multi-operator dataset, which makes it comparable with our data. They find that the 95th percentile of wager volume was

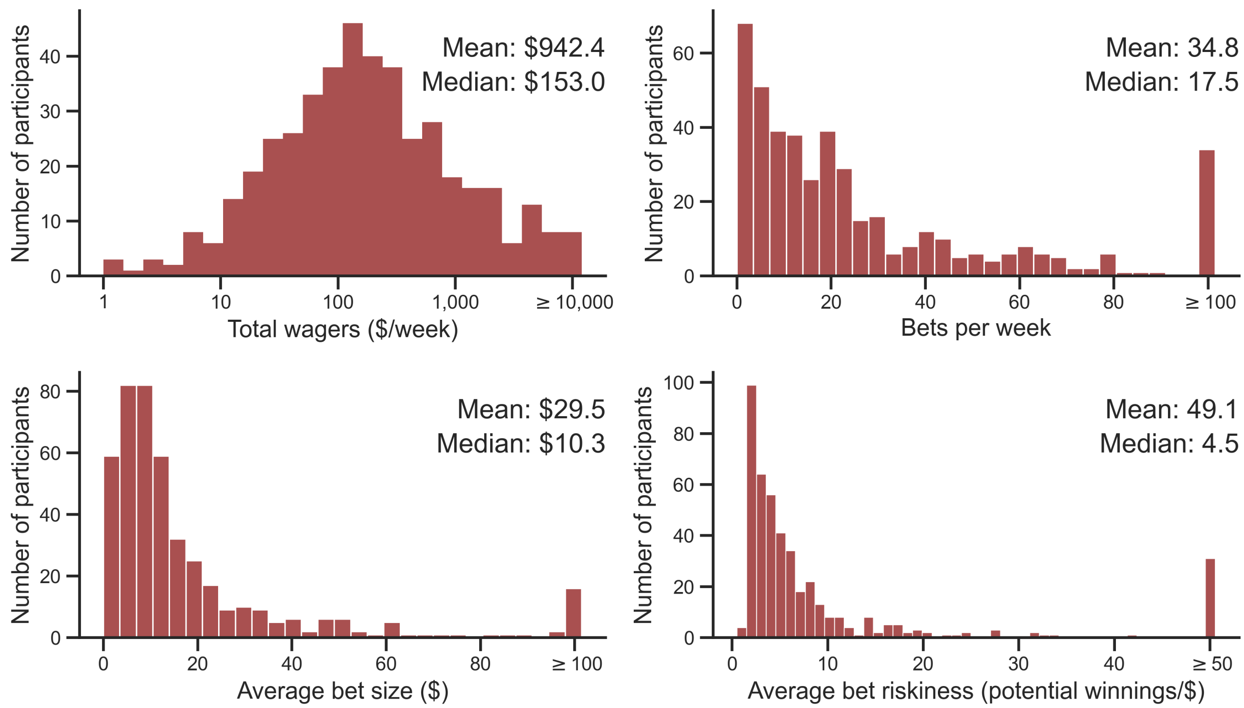

Figure 3: Pre-study betting activity across individuals

Notes: These plots illustrate patterns of betting activity in the analysis sample (N = 444) over the 30-day period prior to survey 1. "Bet riskiness" is the average of decimal odds across all bets, weighted by dollars wagered. People with high bet riskiness wager most of their money on long-shots.

wagered more than $1000 per week. The distributions of number of bets per week (median = 17.4) and average bet size (median = $10.3) are similarly skewed. Finally, we present in the lower right panel the average ratio of potential payouts to stakes across bets, weighted by the size of the bets. Long shot bets are popular: the median person's average bet has a payout to stakes ratio of 4.6.

5.2 Qualitative Evidence of Bias

We present qualitative evidence on overoptimism and self-control problems in Figure 4. The top left panel reports on responses to the question: "Which of the following statements are reasons that you bet on sports?" The most popular selections relate to the nonfinancial value of betting (e.g, 81% said sports betting "makes watching sports more fun"). 39% selected "On average, I win money by betting" as a reason for betting, but only 2% selected "I can't stop myself from betting, even though I wish I could bet less."

The top right panel provides further evidence on perceived self-control problems. 20% of the sample self-report betting on sports too much. For comparison, previous work shows that over

£65/week, or $102/week in 2024 dollars. This number is lower than our median dollars wagered, but the pre-study period was a relatively high-volume betting period, so it's reasonable to say the median would be close to this 95th percentile.

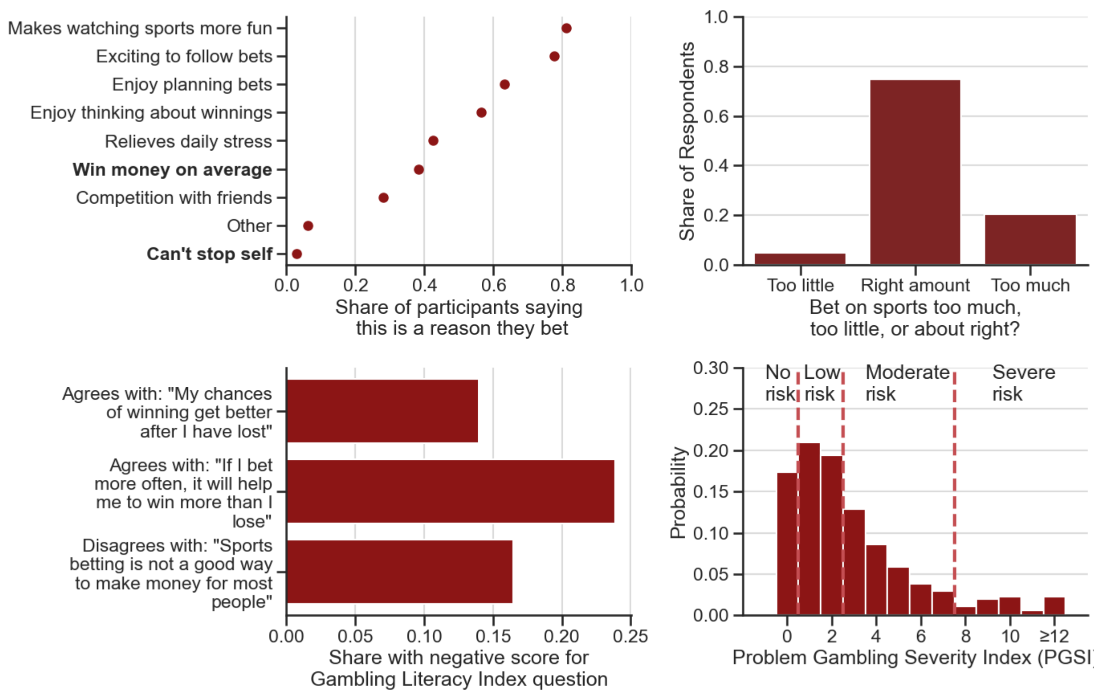

Figure 4: Qualitative evidence of bias

Notes: The figure presents qualitative evidence on our two biases of interest. The top left panel reports on responses to the question: "Which of the following statements are reasons that you bet on sports?" The top right statement repores on responses to the question: "How do you feel about your own sports betting in a typical week?" (options: I am betting on sports [too little/the right amount/too much]). The bottom left panel reports the share of respondents who give answers that receive negative scores for eah of the three components of the Gambling Literacy Index. The bottom right panel shows the distribtution of Problem Gambling Severity index scores. Scores in buckets of $\{0, [1, 2], [3, 7], [7, 21]\}$ are sometimes classified by practitioners as "no risk," "low risk," "moderate risk," and "high risk" for problem gambling, respectively.

Variable	Grubbs and Krauss Census Matched	Weekly Lottery Bettors	Weekly Sports Bettors	Brown, Grasley, and Guido Analysis Sample
N	2806	406	517	444
Demographics				
Age	51.59	55.21	41.47	39.92
White	0.66	0.62	0.59	0.81
Male	0.46	0.53	0.69	0.96
Bachelor's degree or higher	0.34	0.25	0.50	0.82
Graduate degree	0.13	0.08	0.19	0.39
Household income ($000s)	68 (62)	67 (57)	101 (84)	156 (116)
Qualitative bias measures				
Gambling Literacy Index	4.00 (2.30)	3.12 (2.74)	1.53 (3.03)	3.55 (2.05)
Problem Gambling Severity Index	0.99 (2.69)	2.83 (4.21)	6.77 (5.06)	2.89 (2.85)

Table 2: Sample demographics, qualitative measures of bias, and and representativeness

Notes: Columns 1 through 3 present average demographics and qualitative bias measures for subsamples of the Grubbs and Krauss nationally representative study. They show averages for the census-matched sample, weekly lottery bettor sample, and weekly sports bettor samples respectively. Column 4 presents averages for our main analysis sample. Standard deviations are in parentheses.

50% of respondents reported that they used social media too much on a similar question (Allcott et al., 2022b).

The bottom two panels report on the qualitative bias measures that we borrow from the gambling studies literature. The bottom left panel shows responses to the questions that make up the Gambling Literacy Index. For each question, around 20% of participants gave responses that could plausibly be interpreted as factual misunderstandings of how gambling returns work. The bottom right panel illustrates Problem Gambling Severity Index (PGSI) scores. A majority of the sample records a PGSI score of 2 or lower, which is traditionally regarded by practitioners as low or no risk of problem gambling.

5.3 Demographics and Representativeness

We use external data from surveys conducted by Grubbs and Kraus (2023c) (henceforth "GK") to understand the characteristics of sports bettors. GK conduct surveys containing basic demographic questions, the Gambling Literacy Index, and the PGSI. They study nationally representative samples of the general population, self-identified weekly lottery bettors, and self-identified weekly sports bettors.[32] Columns 1, 2, and 3 of Table 2 report average demographics and qualitative bias measures for these three samples respectively. Unlike lottery bettors, sports bettors are younger, have higher education, and earn more than the average American. Sports bettors also have worse biases than lottery bettors according to qualitative measures.

We next compare our analysis sample to the GK weekly sports bettors sample. Our analysis sample was not designed to match the GK weekly sports bettor sample – recall that we intentionally oversampled high-volume bettors – but it is nevertheless a useful comparison group. Our

[32]We provide further details on the GK surveys in Appendix B.3.

analysis sample is whiter, more male, more highly educated and earns more than the GK weekly sports bettors. Most importantly, our sample also has higher scores on the Gambling Literacy Index and lower scores on the PGSI. This result suggests that, if anything, bias estimates for our analysis sample may be conservative estimates of bias for frequent bettors more generally. To illustrate how our results could change for a more general sample, we recompute our main reduced-form results for a weighted version of our analysis sample that matches GK weekly sports bettors on qualitative bias measures and education. We provide details in Appendix B.3.

6 Reduced-form Evidence

In this section, we present empirical evidence that sports bettors exhibit both overoptimism and self-control problems. We also show that sports bettors respond to price changes, which implies that price-based policies can be effective.

6.1 Overoptimism About Financial Returns

We find that participants overestimate the financial returns to betting. On average, participants predicted that they would roughly break even, but the average participant lost 7.5¢ per dollar. We illustrate the distributions of predicted and realized returns in Figure 5, pooling across surveys 1 and 2. We interpret the average difference between predictions and realizations as a transparent estimate of average overoptimism.

The observable feature of betting activity that most strongly predicts overoptimistic prediction is the share of dollars wagered on parlays. Panel A of Figure 6, which plots the heterogeneity of prediction errors with respect to bettor characteristics, shows that bettors with high parlay shares are 18¢ per dollar more overoptimistic than low parlay share bettors. The association arises because even though high parlay share bettors actually earn much less on average, their predictions do not reflect their worse prospects (Appendix Figure B.2).[33] In our sample, the difference is so stark that we cannot reject zero overconfidence for the low parlay share bettors. Overall, these results show that consumer protection concerns about parlays have some legitimacy: compared to other kinds of betting, parlay betting is more likely to be driven by bias rather than preferences.

We also show in Panel A of Figure 6 that low education is the demographic characteristic most strongly associated with overoptimistic prediction. This result suggests that overoptimism is a regressive bias in the sense that it is more prevalent among disadvantaged populations.

We next turn to individual-specific estimates of overoptimism. As we have emphasized, allowing for rich heterogeneity in overoptimism is crucial because we are interested in studying targeted interventions. The empirical challenge is that we do not observe overoptimism directly. Instead, we observe raw prediction errors, which are a combination of underlying overoptimism

[33]The association between high parlay share and misprediction persists even after controlling for other observables (Appendix Table B.2).

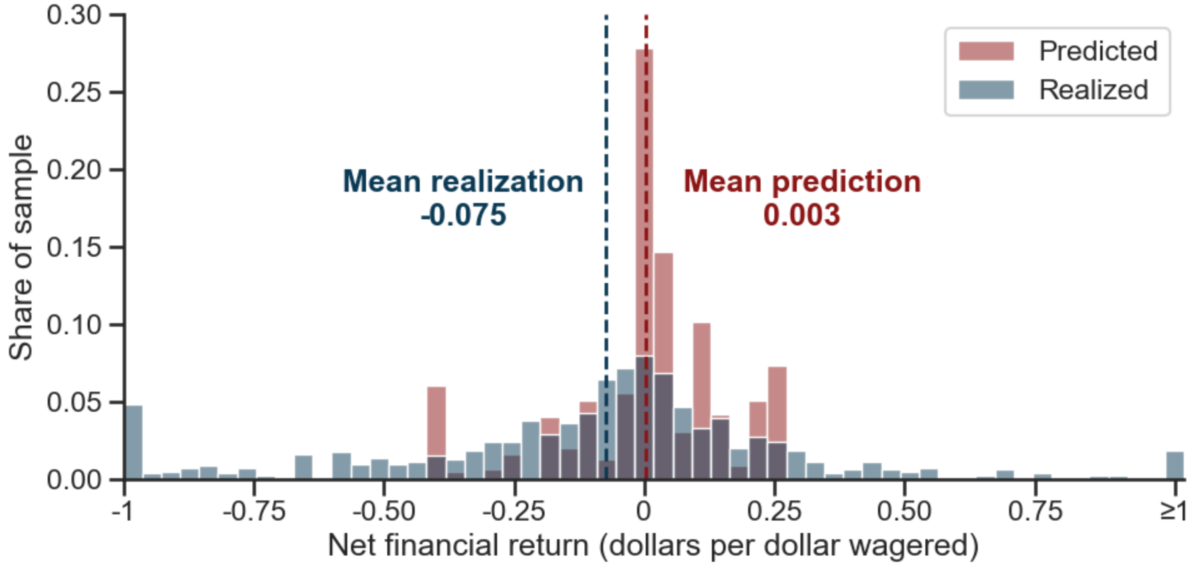

Figure 5: Evidence on overoptimism from predicted and realized returns

Notes: This figure shows the distribution of predicted amd realized winnings, pooling across period 1 (April 9 to May 9) and period 2 (May 11 to June 10). Predictions are elicited in surveys 1 and 2 respectively. We restrict the sample of predictions to participants who placed wagers in the relevant period. Predictions are censored to lie within $[-0.4, 0.25]$. Dotted lines and annotations represent unweighted averages.

and noise. Noise arises from the intrinsic randomness of gambling outcomes and elicitation noise in the prediction question (Kahneman, 1965; Gillen et al., 2019). We apply a shrinkage procedure to recover estimates of the underlying overoptimism parameter for each participant (Chen, 2024). The shrinkage procedure requires us to measure the variance of both sources of noise, which we accomplish using the underlying bet-level microdata and the panel structure of our data for gambling outcome noise and elicitation noise respectively. To measure outcome noise, we assume that the outcomes of particular bets are independent. To measure elicitation noise, we assume that elicitation errors are mean-zero and i.i.d. across surveys. We provide details on our procedure in Appendix B.4.

We estimate a great deal of heterogeneity in overoptimism across individuals. Panel B of Figure 6 shows the distribution of individual-specific estimates. The estimates imply that 9% of participants are *underoptimistic* about their expected returns, and 10% are overoptimistic by more than 20¢/dollar. Optimally, policy would treat these two kinds of bettors differently, suggesting targeted policies could deliver large efficiency gains.

To support our interpretation of prediction errors as evidence of overoptimism, we provide evidence against three alternative explanations. First, people might have misunderstood the nature of our survey question in a way that caused them to report higher predictions. Since we used consistent language across all expected returns questions, this explanation would imply that

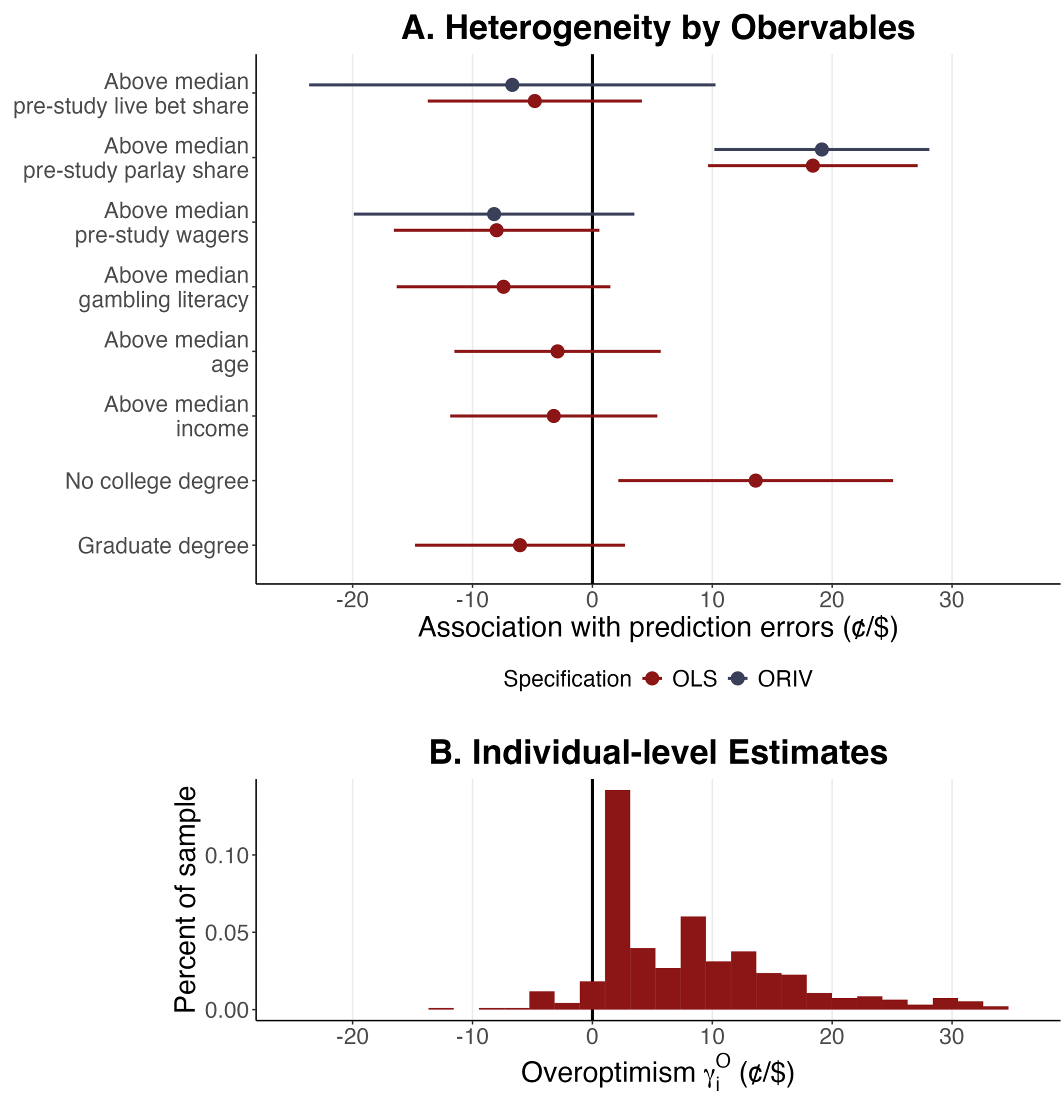

Figure 6: Heterogeneity in overoptimism

Notes: Panel A shows coefficient estimates from univariate regressions of signed prediction errors about net returns (pooled across periods 1 and 2 when possible) on binary explanatory variables. An observation is a member of the analysis sample who placed wagers both in the pre-study period ($t = -1, -2$) and the study period ($t = 1, 2$). Error bars represent 95% confidence intervals. All pre-study betting activity variables are measured as the average of the relevant quantities in periods -1 and -2. The ORIV points show estimates using the Obviously Related Instrumental Variables procedure of Gillen et al. (2019) to correct for measurement error in the pre-study variables. Panel B shows the histogram of individual-specific overoptimism estimates, computed via the shrinkage procedure described in Appendix B.4.

people consistently overestimate all net returns. Instead, we find that overestimation is local to predictions about the future: when we ask people to recall past net returns, they *underestimate* their winnings (Appendix Figure B.1).[34] Second, people might skew their reported predictions upwards out of a desire to protect ego-relevant beliefs. If this were true, we would expect raising the stakes of predictions to decrease predicted winnings (Prior et al., 2015). In contrast, we find that incentivizing predictions did not affect responses (Appendix Figure B.4). Third, we show in Appendix B.1 that rounding in survey responses does not drive our overoptimism estimate.

While our experiment was not designed to study the behavioral mechanisms behind overoptimism, our descriptive patterns provide suggestive evidence supporting some mechanisms from the literature. First, in this sample of highly experienced bettors, overoptimism about future returns persists even though people did not overestimate past returns. This pattern is inconsistent with models of motivated reasoning where agents forget unfavorable signals (Bénabou and Tirole, 2002; Huffman et al., 2022; Sial et al., 2023). Instead, our results are consistent with models where agents selectively interpret past signals (Thaler, 2024), for example by attributing bad news to luck and good news to skill (Ross, 1977; Stipek and Gralinski, 1991). Second, parlay betting is associated with overoptimistic predictions. The main differences between parlays and other kinds of bets are that parlays have multiple legs and longer odds. We find that when we measure the association conditioning on bet odds, the parlay effect remains large and significant while bet odds have no independent effect (Appendix Table B.2). This pattern suggests that overoptimism about parlay wagers is not caused only by the over-weighting of small probabilities (Kahneman and Tversky, 1979). One plausible alternative is parlays are more complex than standard wagers because of their multiple components (Enke and Shubatt, 2023).

6.2 Self-control Problems

As described in Section 4.3, we measure both people's perceptions of their self-control problems and their naivete about their self-control problems. We combine these measures to get an estimate of total self-control problems.

Perceived self-control problems Our measure of perceived self-control problems is the difference between observed valuations of the Bet Less Bonus and counterfactual valuations held by agents who did not perceive themselves to have self-control problems. Intuitively, a participant who wants his future self to bet less will value the Bet Less Bonus more, so we can identify perceived self-control problems from high observed valuations.

This identification strategy requires us to compute counterfactual benchmark valuations of the Bet Less Bonus for participants without perceived self-control problems. To do this, we decompose the Bonus into an unconditional transfer equal to the bonus's maximum value plus an increase in the price of wagering a dollar by 2¢. Therefore, the value of the Bonus to a participant

[34] The finding that people do not overestimate past net returns is consistent with previous work by gambling studies scholars (Braverman et al., 2014).

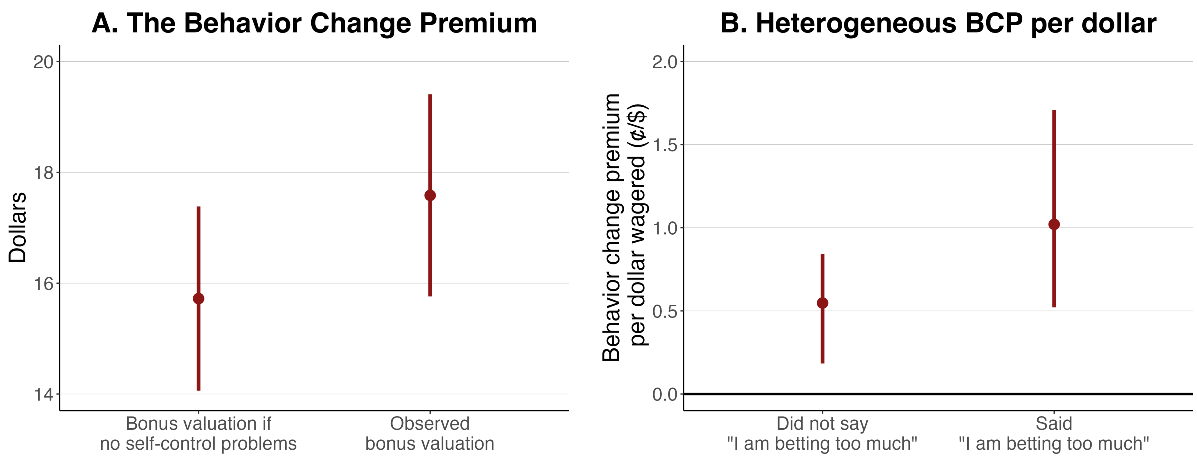

Figure 7: Evidence on perceived self-control problems from Bonus valuations

Notes: Panel A plots the average counterfactual no perceived self-control problems valuations against observed bonus valuations. The difference between the observed valuations and the time-consistent valuations is the behavior change premium. Error bars represent 95% confidence intervals. A paired t-test of the difference between the two variables gives a p-value of 4e-05 (the measures are highly correlated within subject). Panel B plots the average behavior change premia by self-control problem indicators. Error bars represent bootstrapped 95% confidence intervals. The randomization p-value for the difference in estimates is 0.055. For both plots, the sample is the subset of participants whose predicted consumption under both the bonus control and bonus treatment conditions were below the benchmark and above the value at which the price increase from the bet less bonus would no longer bind (N=352, 79% of the analysis sample).

without perceived self-control problems is just the value of the unconditional transfer minus the consumer surplus loss from the price increase. We compute these components using consumption predictions in the treatment and control conditions. On average, participants whithout perceived self-control problems would value the bonus at \$15.72.[35]

Panel A of Figure 7 shows that, on average, participants valued the bonus at \$17.58, which exceeds the no self-control problems benchmark by \$1.86 (12%). We refer to this difference as the *behavior change premium* (or BCP) because it represents the amount of money a participant is willing to pay for the behavior change induced by the Bonus. This result establishes that participants would pay to reduce future consumption, which is evidence of perceived self-control problems.

We next convert the BCP into price-metric units of money per dollar wagered. The average participant predicted that the Bonus would reduce consumption by \$274 over the 30-day period that it was active, and the behavior change premium implies that they would pay \$1.86 for this reduction. Combining the two estimates, we conclude that the average participant was willing to

[35] The average unconditional transfer was \$27.75. We approximate the consumer surplus loss from the price increase by measuring the area under the demand curve between the initial and final price using predicted consumption data to trace the demand curve for each participant. Concretely, given predicted consumption x_i^C in the control condition and treatment consumption x_i^B, we approximate the consumer surplus loss with the trapezoid $\frac{x_i^C + x_i^B}{2} \cdot 0.02$. The average consumer surplus loss was \$12.03. Combining these components implies that without self-control problems, the average participant would value the Bonus at \$15.72.

pay $\frac{1.86}{274} = \$0.007$ for their future self to reduce consumption by a marginal dollar. A behavioral economics literature starting with Carrera et al. (2022) has noted that this *behavior change premium per unit of consumption* is a direct estimate of price-metric perceived self-control problems ($\tilde{\gamma}^{SC}$ in our model).[36] We provide a formal argument in Appendix B.2.[37]

In Panel B of Figure 7, we show that the BCP per dollar wagered is larger for agents who self-report a desire to reduce future betting consumption. The fact that the BCP per dollar wagered correlates with with this qualitative measure of perceived self-control problems across participants validates our use of the BCP per dollar as an estimate of $\tilde{\gamma}^{SC}$.

Naivete about self-control problems Our measure of naivete comes from predictions of future dollars wagered. Intuitively, if people underestimate their future self-control problems and are otherwise unbiased, they will also underestimate their future dollars wagered (Augenblick and Rabin, 2019).[38] Figure 8 shows that participants do not underestimate future consumption. Instead, they slightly *overestimate* future dollars wagered on both survey 1 and survey 2. We interpret this lack of underestimation as evidence against naivete. The result implies that we can use the BCP per dollar measure of perceived self-control problems ($\tilde{\gamma}^{SC}$) as a measure of overall self-control problems (γ^{SC}).

We evaluate two plausible alternative explanations for the lack of underestimation. The first is that participants misperceived the extent to which sports betting would become less enjoyable from March to June.[39] We designed our survey to minimize such misperceptions. We explicitly prompted participants to think about seasonality with an open-ended question. The responses often demonstrate an understanding of how sports seasons affect betting activity.[40] Numerical

[36]Here is the intuition. The present self predicts that the future self will optimize, betting until the value of the marginal dollar wagered is equal to 0 under the (predicted) decision utility function. The present self also predicts that the future self will overvalues all dollars wagered by $\tilde{\gamma}^{SC}$. Therefore, according to the present self's utility function, the value of the marginal dollar wagered in the future is not zero, but $-\tilde{\gamma}^{SC}$. So the present self is willing to pay $\tilde{\gamma}^{SC}$ to avoid consuming that marginal dollar. This willingness to pay is what the behavior change premium per unit of consumption measures.

[37]The argument that the BCP per dollar wagered identifies perceived self-control problems relies on an approximation that participants are risk-neutral over experimental payments, so we conduct an empirical test of this assumption. Such risk preferences matter to the extent that bonus payments are uncertain ex ante. In that case, a risk-averse (-loving) participant would place a lower (higher) valuation on the bonus even if they were time-consistent, and the BCP will underestimate (overestimate) self-control problems. We elicit risk preferences (Appendix A.4) over experimental earnings directly, and find that the average participant is risk-averse (Appendix Figure B.14), so the BCP gives a lower bound on perceived self-control problems. However, correlating bonus valuations and risk aversion rules out that bonus valuations are strongly decreasing in risk aversion (Appendix Figure B.15). We conclude that deviations from risk neutrality had at most a minor impact on our estimates.

[38]In principle, a naive agent could make correct predictions if they had an offsetting biased belief that increased the perceived marginal utility of future consumption (see e.g, Heidhues et al. (2023)). Such an offsetting bias would cause this identification strategy to fail. Importantly, overoptimism in our model is not an example such an offsetting bias – the offesetting bias must drive a wedge between predicted and true future consumption, and we assume that overoptimism appears identically in both the predicted future utility function and the true decision utility function.

[39]Betting activity does fall during the study period for our sample, which is consistent with trends in aggregate betting activity.

[40]For example, we asked: "What types of sporting events, if any, are you likely to bet on in the next 30 days?" In survey 1, one participant said, "None - I mostly only like to bet on NFL football and March Madness basketball so I come and go on and off the sports betting accounts during those times."

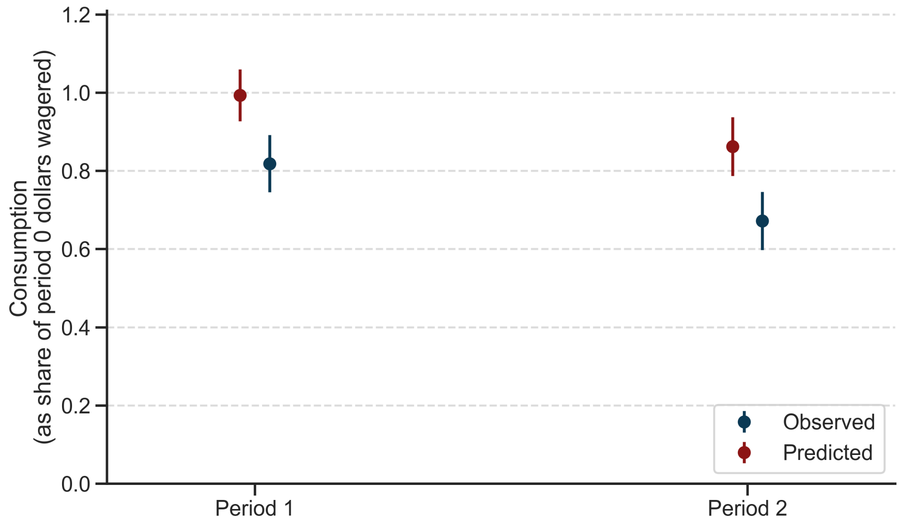

Figure 8: Evidence against naivete from predictions of future dollars wagered

Notes: The figure compares predicted dollars wagered to the truth. We restrict to observations in the bonus control condition. Points represent mean wager volume, as a share of period 0 dollars wagered. Error bars represent 95% confidence intervals. The red series represents predictions, and the blue series represents observations. Both the observed and predicted variables are truncated to lie in $[0, 2]$

predictions also reflect this understanding: predicted changes in consumption are strongly correlated with observed changes in consumption across participants (Appendix Figure B.5). Second, we consider the possibility that participants struggled to comprehend "dollars wagered" as a unit of consumption and that they over-reported consumption due to confusion. Using a qualitative prediction question with natural language and for which such confusions are unlikely to arise, we again find no evidence of naivete (Appendix Figure B.6).

Summary Taken together, our results imply that $E_i[\gamma_i^{SC}] = E_i[\tilde{\gamma}_i^{SC}] = 0.7$¢ per dollar wagered. The first equality, showing that perceived self-control problems and true self-control problems coincide, comes from the lack of underestimation about future consumption. The second equality, quantifying the magnitude of self-control problems, comes from the BCP per dollar.

We highlight that our estimate of average price-metric self-control problems is an order of magnitude smaller than our estimate of average price-metric overoptimism. This result implies that in our sample, the primary motive for corrective policy is overoptimism correction rather than self-control problem correction.

6.3 Price-sensitivity

Price-based corrective policies, such as taxes, can only improve welfare to the extent that consumption responds to prices. We provide two kinds of evidence on own-price demand responses: the randomized effects of the Bet Less Bonus and predicted responses to other price changes.

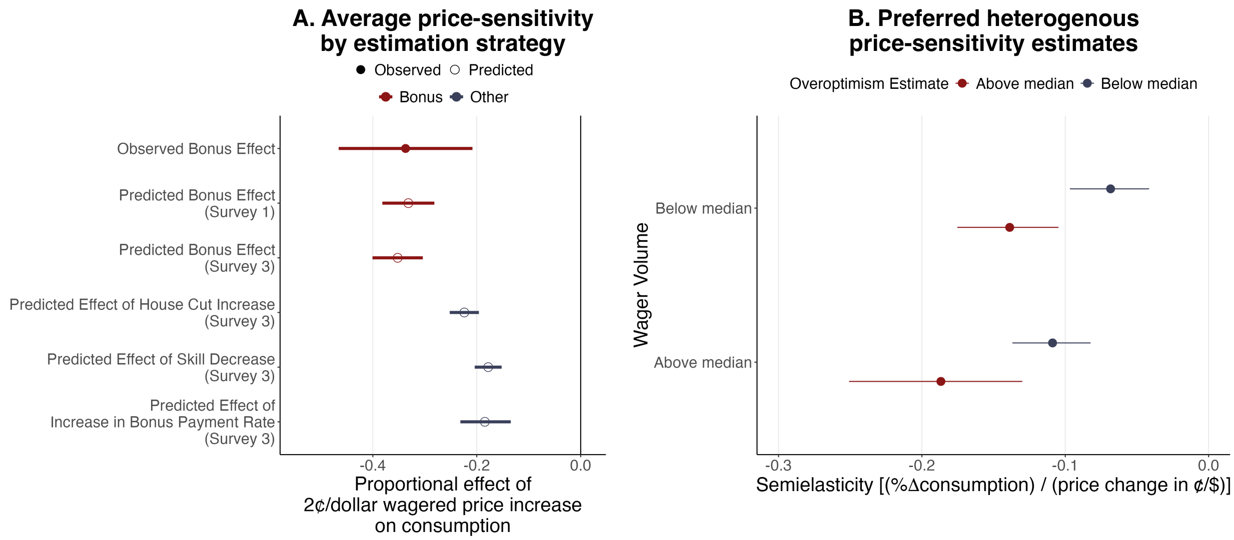

Figure 9: Evidence on price-sensitivity

Notes: Panel A plots several versions of estimated or predicted impacts of 2¢ price increases. The effect is reported as a share of Period 1 consumption. The first row is the estimated Bonus treatment effect (equation B.8). The second and third row are predicted bonus treatment effects, elicited on surveys 1 and 3 respectively. The fourth and fifth row are predicted effects of hypothetical naturalistic price changes. The fourth row is the effect of a hypothetical 2¢/dollar increase in the house cut, and the fifth row is the effect of the participant hypothetically learning that their own expected returns were 2¢/dollar worse than they had previously thought. The effect in the sixth row comes from predicted responses to bonus treatments of various sizes. Panel B plots our preferred price-sensitivity estimates, which come from the predicted effect of a hypothetical change in the house cut. The point on that plot represent average semielasticity estimates, which can be interpreted as the proportional effect of a 1¢ price change on consumption. We provide estimates for four subgroups, splitting by above/below median dollars wagered. Error bars represent 95% confidence intervals.

Our first set of results is the treatment effects of the Bet Less Bonus, which was an experimentally induced 2¢ increase in the price of betting. The first point in Panel A of Figure 9 illustrates the main result: the Bet Less Bonus reduced consumption by 34% for the average participant. We provide details on our regression specification in Appendix B.5 and results in Appendix Table B.6.

One concern is that treated participants simply substituted from legal sports betting to other kinds of gambling. We show using self-reported consumption data that this was not the case (Appendix Figure B.7). The lack of substitution implies that an increase in the price of sports betting would reduce *overall* gambling consumption, which is important to the extent that other gambling consumption is also biased and not corrected by existing taxes (Allcott and Rafkin, 2022; Farhi and Gabaix, 2020). Importantly, though the sports betting industry has argued that taxes on sports betting could induce substitution to illegal alternatives, our point estimate implies that only 4% of reduced sports betting consumption was diverted to the illegal market.[41] These substi-

[41]One of the American Gaming Association's policy objectives is to repeal the federal excise tax on sports betting. Their stated rationale is to discourage substitution to the illegal market: "Currently, this tax serves no dedicated purpose and represents an added operating cost to legal sportsbooks that illegal operators do not pay, further impeding customers' move away from the predatory, illegal market to safe, regulated sports betting channels. Congress can help empower the success of a safe, regulated marketplace, by repealing the federal excise tax that unnecessarily disadvan-

tution results come with two important caveats. First, the estimates capture short-run substitution effects, and long-run substitution effects could be larger. Second, the dependent variable in this analysis is self-reported consumption, which may understate true consumption, particularly for illegal gambling.[42]

Our second set of results considers predicted responses to various hypothetical price changes. Our main finding is that predicted responses to hypothetical price changes were about half as large as the Bonus effect. This pattern holds across three kinds of hypothetical price changes: changes in the house cut, changes in skill, and versions of the Bonus treatment where differences in the payment rate were salient (as described in Section 4.5). Figure 9 illustrates this result by plotting all of our price-sensitivity estimates on the same scale. Predicted effects of hypothetical price changes are consistently smaller than predicted and observed effects of the Bet Less Bonus.

We conduct multiple tests to validate that this pattern is not spurious. To validate our use of prediction data, we show that predicted changes in consumption correlate with observed changes in consumption (Appendix Figure B.5), and this holds across both the Bonus control and treatment conditions (Appendix Table B.3). We also find that the average predicted bonus effects are similar in magnitude to the average observed bonus treatment effect (Figure 9), again suggesting that predictions are meaningful. Finally, we note that the literature on hypothetical choice generally finds that people *overstate* sensitivity to salient changes in conditions, so standard hypothetical response bias cannot explain the direction of our effect (Bernheim et al., 2021).

To decide which of our two estimation strategies to use in our main specification, we turn to qualitative evidence on how people experienced the Bonus treatment. By design, we endowed the Bonus treatment with a frame that made its expected impact on consumption salient (for example, by calling it the "Bet Less Bonus"). As described in Section 4.3, this frame was necessary to implement our measurement of perceived self-control problems since people needed to understand how the bonus affected consumption to report an appropriate willingness to pay. It is reasonable to be concerned that this frame could have induced consumption reductions separate from the effect of the 2¢/$ price increase. We added multiple-choice questions to survey 3 specifically to check for these non-price effects. For a substantial minority of participants, responses suggest that non-price effects were relevant (Appendix Figure B.8). Motivated by these results, we use the predicted response to a 2% change in the house cut as our main estimate of price-sensitivity. We discuss the robustness of our policy conclusions to alternative choices in Section 8.3.

We illustrate our main price-sensitivity estimates in more detail in Panel B of Figure 9. We estimate heterogeneous price-sensitivities by four subgroups of the analysis sample, splitting at the median of pre-study wager volume and of the individual-specific overoptimism estimate. All estimates are reported as semielasticities (proportional effects of a 1¢/dollar price increase). We find that high-volume bettors have larger proportional consumption responses to price changes,

tage legal sports betting operations." https://www.americangaming.org/policies/hot-issue-sports-betting/

[42]Of course, nearly every other study of illegal gambling is also vulnerable to this second issue.

and that high overoptimism is associated with larger responses, conditional on the wager volume subgroup. This second result implies that, all else equal, more biased bettors respond more to price changes, which is good news for the targeting efficiency of price-based policies. Given our modest sample size, the error bars in Figure 9 are reasonably tight. This highlights an important benefit of using hypothetical price responses: they improve our power to detect heterogeneous price-sensitivity by letting us observe choices under different price levels within each subject.

7 Model Estimation

We now use the empirical results from Section 6 to estimate a specialized version of the biased consumption model from Section 3. The main specialization is a constant semielasticity of demand functional form.[43] To impose constant semielasticity, we assume that the functional form of nonfinancial utility is $z_i(x;\tilde{F}) = z_{1i}x\log(x) + z_{2i}x + g_i(\tilde{F})x + h_i(\tilde{F})$.

The demand curve is defined as follows. Let τ_0 denote a status quo price of betting.[44] Let $x_i^*(0)$ denote the normative consumption at this price. We consider consumption as a function of bias and other unit prices τ_i. We show in Appendix C.3 that, under our functional form assumption, demand is characterized by the following equation:

$$\log(x(\tau_i)) = \log\left(\underbrace{x_i^*(0)}_{\text{Normative consumption at status quo}}\right) - \underbrace{\eta_i}_{\text{Semielasticity}} \cdot \left(\underbrace{(\tau_i - \gamma_i^O - \gamma_i^{SC}) - \tau^0}_{\text{As-if price change}}\right) \qquad (3)$$

For estimation in our panel data, we modify equation (3) by imposing structure on log normative consumption: $\log(x^*(0)_{it}) = \xi_i + \delta_t + \varepsilon_{it}$, where ε_{it} are i.i.d. normative demand shocks. The parameters ξ_i ad δ_t represent the individual-specific taste for betting and seasonal trends in the normative utility from gambling respectively. The implicit assumption is that residual idiosyncratic variation in consumption is driven by normative demand shocks ε_{it} rather than bias shocks.

Our estimates of bias parameters and price-sensitivity come directly from the reduced-form experimental results already presented. Concretely, the statistics illustrated in Panel B of Figure 6, Panel B of Figure 7, and Panel B of Figure 9 are our estimates of overoptimism γ_i^O, self-control problems γ_i^{SC}, and semielasticity η_i, respectively.

Given these estimates, we can now estimate the individual and time-specific components of normative consumption. Specifically, we exponentiate both sides of (3), divide by the effect of the as-if price change, and take the expectation of both sides over the normative demand shocks.

[43] This functional form is motivated by the shape of the predicted consumption demand curve (Appendix Figure B.18).

[44] The status quo price τ_0 is a normalization parameter in the following sense. Let $F_A(a)$ be one return distribution, and $F_B(a)$ be another where $F_B(a+t) = F_a(a)$. Then the model with F_A, τ_0 is equivalent to the model with $F_B, \tau_0 + t$. Therefore, the choice of τ_0 does not affect the estimation of any demand parameters.

Doing so yields the following estimating equation for ξ_i and δ_t.

$$E\left[\frac{x_{it}}{\exp\left(\eta_i \cdot \left(\tau^0 + \gamma_i^O + \gamma_i^{SC} - \tau_i\right)\right)}\right] = \exp(\xi_i + \delta_t) \tag{4}$$

The left side of equation (4) represents residualized consumption – observed demand adjusted by the effects of bias and prices, given our estimates. We construct a panel of residualized consumption over the three pre-study periods $t = -2, -1, 0$, using observed consumption and our estimates of γ_i^O, γ_i^{SC}, and η_i. Then, we use a poisson fixed effects regression to estimate ξ_i and δ_t for this panel, normalizing $\delta_{-1} = 0$.

This procedure illustrates our key assumption: all consumption that is not driven by estimated overoptimism and self-control problems must be driven by preferences. An obvious but important implication is that if other unmeasured biases drive consumption, our model will treat those biases as preferences and overstate the utility from betting. More broadly, misspecification would generally cause us to attribute consumption incorrectly to bias versus preferences. For example, we restrict heterogeneity in self-control problems to be across two subgroups.[45] If unobserved heterogeneity in bias drives variance in consumption, our model will incorrectly attribute this variance to heterogeneous preferences.

8 Counterfactual Policy Analysis

8.1 Assumptions and Policies of Interest

To identify policies of interest, we analyze a simple social planner's problem. The planner can set gambling taxes and recycle revenues into lump-sum transfers that are distributed equally. We assume that taxes fully pass through to perceived prices: a tax τ_i causes an increase in the perceived cost of wagering \$1 by τ_i. We assume that all consumers have equal welfare weights, so the planner's objective is to maximize average normative utility subject to a balanced budget constraint:

$$\max_{\tau_i} \sum_i E_i[u_i^{normative}(x_i(\tau_i))] + R(\tau) \tag{5}$$
$$\text{s.t.} \quad R(\tau) = E[\tau_i x_i(\tau_i)]$$

We begin by studying a first-best benchmark: optimal personalized taxes. The optimal personalized tax for agent i is the rate that internalizes both biases: $\tau_i^* = \gamma_i^O + \gamma_i^{SC}$. Under such a scheme, every agent consumes the normatively optimal level of gambling, and social welfare is maximized. We therefore view benefits from this policy as an upper bound for the benefits of corrective policy.

[45] We cannot estimate a full distribution like we did for overoptimism because of the limitations of our self-control problems data: we only measure bonus valuations once, and these bonus valuations contain elicitation noise.

We also study the optimal uniform tax, which is the solution to equation (5) under the constraint that $\tau_i = \tau$ for all i. The optimal uniform tax is a weighted average of individual biases, with weights proportional to the slope of the demand curve (Diamond, 1973; Allcott and Taubinsky, 2015). Specifically, the optimal tax must satisfy $\tau^* = E[w_i \tau_i^*]$, where the demand weights are specified as $w_i = \frac{\eta_i x_i(\tau^*)}{E_i[\eta_i x_i(\tau^*)]}$. With our preferred estimates, the optimal tax rate is 5.17%, more than twice as large as the status quo rate of 2.02% (Figure 1). We note that the optimal rate is lower than sum of the simple unweighted average overoptimism and self-control problems estimates from Section 6 (7.8% + 0.7% = 8.5%). The reason is that high-volume bettors are less overoptimistic on average (Panel A of Figure 6), and high-volume bettors are weighted more heavily in the average, which shades the optimal rate down.

Finally, we evaluate non-tax interventions that directly reduce or mitigate bias. To study bias correction, we consider a stylized intervention that causes agents to choose as if bias $b \in \{M, SC\}$ was equal to $(1 - \omega^b)\gamma^b$, where ω^b parameterizes the stregth of bias reduction. For illustration, we consider $\omega^O = 0.2$ and $\omega^{SC} = 0.2$ independently, corresponding to a 20% reduction in overoptimism and self-control problems respectively. Later, in Section 9, we provide experimental evidence about the empirically feasible magnitude of bias correction.

Discussion We rely on simplifying assumptions to characterize the effects of both policies. For taxes, our assumption that changes in tax rates fully pass through to perceived prices rules out imperfect pass-through from taxes to market prices (for example, because of market power (O'Connell and Smith, 2024)) and imperfect salience of price changes (as in Chetty et al. (2009)). Absent evidence on the supply side and the salience of odds changes in this context, we proceed with this assumption and view our exercise as an informative first step. For bias-correction interventions, there are two key assumptions. The first is that our stylized bias-corrections are perfectly targeted at bias. Our results can therefore be interpreted as a best-case scenario for the benefits of bias correction. We study whether real-world bias-correction interventions are actually well-targeted at bias in Section 9. Second, we assume that the intervention does not change the normative utility function – for example, an intervention that reduces overoptimism does not affect the *nonfinancial* utility of gambling.[46] This assumption amounts to the restriction that $g_i(\tilde{F})$ and $h_i(\tilde{F})$ (which capture the effects of perceptions on the marginal utility of betting and on the level of utility respectively) are both constant. We impose this assumption only when evaluating the welfare effects of the bias-correction interventions, not for taxes.

The planner's problem described above abstracts away from redistributive and efficiency motives for consumption taxation. These simplifications allow us to focus sharply on corrective motives: in our framework, revenues raised from the gambling tax have a net zero impact on social welfare, and the only way for policy to improve welfare is by correcting mistaken consumption.

[46]See Glaeser (2006) for an early discussion of how interventions might impact normative utility, and Butera et al. (2022) for empirical evidence in a different context.

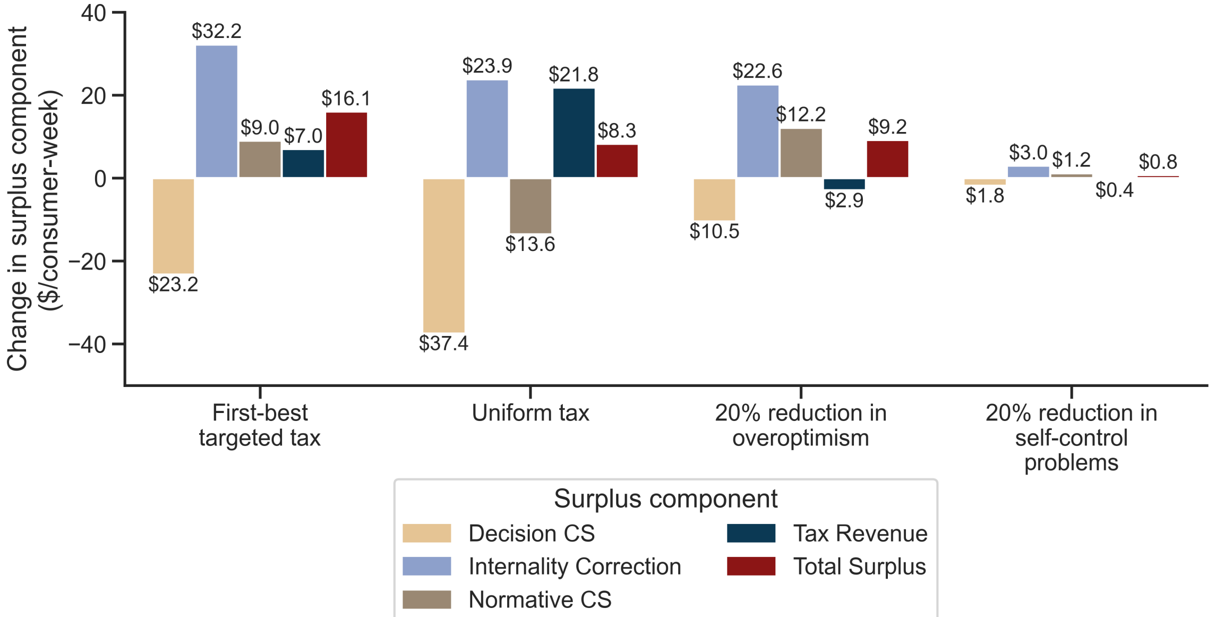

Figure 10: Effects of policies on surplus components

Notes: The plots decompose the welfare effects of the policies described in Section 8. The simulation used normative consumption from period -1 (February 8 - March 9), so welfare calculations apply to that time period. The simulation assumes a status quo tax rate of 2.02%. The total surplus bar is the unweighted sum of Normative CS and Tax Revenue.

8.2 Simulation Results

We disaggregate welfare effects for exposition. Equation (5) shows that the planner's objective function is the sum of (pre-transfer) normative consumer surplus plus government revenues; we present effects on these components separately. We further decompose normative consumer surplus into the *decision consumer surplus*, which is the consumer surplus evaluated according to the decision utility function (1), and the *internality benefits*, which is the reduction in uninternalized costs. In all cases, we report effects in units of dollars per week compared to a status quo policy of a 2.02% tax and using the level of demand from period $t = -1$ (February 8 - March 9).[47] Figure 10 summarizes our results.

Personalized taxes increase welfare by $16.08 per consumer-week. We think of this effect as an upper bound on the potential welfare effects of corrective policy. It is challenging to aggregate these benefits, which are local to our experimental sample, to benefits in the market more broadly. We take a back-of-the-envelope approach, assuming that the policy benefit per dollar wagered for the average experimental participant in this period equals the average benefit per dollar for all U.S. sports bettors in 2023. Under this assumption, our estimates imply an upper bound for welfare gains of $1.37 billion relative to the status quo policy in a year where aggregate demand

[47]February and March are popular times for sports betting in the U.S, so welfare effects per week reported here will be somewhat higher than the average welfare effect per week across the whole year.

was at 2023 levels.[48] As a point of comparison, Allcott et al. (2019) estimate that an optimal sugar-sweetened beverages tax delivers welfare gains between $2.4 billion and $6.8 billion (depending on the specification) relative to a status quo with no taxes.[49]

Of course, personalized taxes are not implementable, so we evaluate a feasible alternative: the optimal uniform tax. The optimal uniform tax increases total surplus by $8.3 per consumer-week, which is only half as large as the first-best surplus gain. The uniform tax falls well short of first best because it cannot differentially reduce the betting of the most biased users while allowing unbiased users to continue betting. Because we estimate substantial heterogeneity in bias (Panel B of Figure 6), this poor targeting leaves significant efficiency gains on the table.

Motivated by the inefficiency of uniform taxes, we turn to bias-correcting interventions. Such interventions are, in theory, exceptionally well-targeted at bias (Camerer et al., 2003; Thaler and Sunstein, 2003), which implies that they could improve on a uniform tax. Indeed, we find that the 20% reduction in overoptimism delivers more total surplus than the optimal uniform tax. The distributional consequences also differ. With the tax, consumers are worse off before accounting for government transfers. With bias-correction, benefits accrue to consumers through internality benefits, while the government loses money because the status quo gambling tax collects less revenue. Correcting self-control problems has similar distributional consequences but is less valuable, since our self-control problem estimates are smaller than our overoptimism estimates.

Overall, these results suggest that effective bias-correcting interventions could deliver major benefits. Of course, in practice, implementation matters – the theoretical benefits illustrated in Figure 10 are only realized if interventions actually reduce biased consumption. Our analysis of the history transparency and voluntary wager limits in Section 9 provides evidence about the effectiveness of two prominent interventions.

8.3 Discussion and Robustness

We turn now to several extensions and robustness checks, summarizing in the interest of brevity.

Alternative motives for taxation The planner's problem in Section 8.1 only allowed for corrective motives. We study a more general planner's problem in Appendix C.4 that explictly allows for revenue-raising and redistributive motives and describe implications for the optimal uniform tax rate. The fact that the average sports bettor is richer than the average recipient of government transfers is a force that increases the optimal sports betting tax for planners with redistributive motives.

Evidence on how beliefs affect nonfinancial utility Our welfare analysis of bias-correction interventions assumes that the interventions do not affect normative utility. One plausible concern is that interventions that correct overoptimism also reduce the nonfinancial value of betting. In

[48] Our estimates imply an average status quo wager volume of $1421 per person-week in the simulation period, so the first-best policy delivers 1.13¢ of total surplus gains for every dollar wagered. Multiplying this number by the $121 billion wagered by Americans in 2023 yields a total surplus gain of $1.37 billion.

[49] The Allcott et al. (2019) calculation includes both externality and internality benefits.

	Specification			Uniform tax		First-best
	(1)	(2)	(3)	(4)	(5)	(6)
	Identification of η	Heterogeneity	Optimal rate (%)	Consumption reduction (%)	Total surplus gains ($/consumer-week)	Total surplus gains ($/consumer-week)
(1)	Change in house cut	Bias and Semielasticity	5.17%	31%	$8.3	$16.1
(2)	Change in house cut	None (Homogeneous)	9.12%	59%	$35.9	$35.9
(3)	Change in house cut	Bias only	4.59%	28%	$5.4	$13.1
(4)	Change in Bonus rate	Bias and Semielasticity	5.15%	28%	$6.9	$14.0
(5)	Change in Bonus rate	None (Homogeneous)	9.12%	52%	$29.0	$29.0
(6)	Change in Bonus rate	Bias only	4.63%	23%	$4.4	$10.9
(7)	Effect of Bet Less Bonus	Bias and Semielasticity	4.97%	37%	$9.7	$19.3
(8)	Effect of Bet Less Bonus	None (Homogeneous)	9.12%	77%	$58.5	$58.5
(9)	Effect of Bet Less Bonus	Bias only	4.63%	42%	$9.4	$20.7

Table 3: Sensitivity of corrective taxation anaysis to alternative estimation strategies

Notes: This table ilustrates how our optimal tax simulations depend on whether we allow for heterogeneous paramters and on our estimation strategy for the semielasticity term η. Columns (1) and (2) describe the specification of interest. Columns (3), (4), and (5) report the implied optimal uniform corrective tax on dollars wagered (setting $\lambda = 1$), the effect of that tax on dollars wagered (as a percent change relative to the consumption under the status quo tax of 2.02%, and the total surplus gains from that tax (again relative to the status quo)). Column (6) shows the total surplus gains from a first-best personalized tax. Column (1) varies the identification strategy for the semielasticity η, using the three approaches outlined in Section 6.3 and visualized in Panel A of Figure 9. When estimating heterogeneous η, we allow for heterogeneity by above/below median in dollars wagered and misperceptions, as in Panel B of Figure 9. For the "Effect of Bet Less Bonus" estimation strategy, we use the predicted bonus effect in survey 1, since this improves power for estimating heterogeneous effects. Column (2) varies our approach to heterogeneity. Rows (1), (4), and (7) estimate heterogeneity in $(\gamma_i^M, \gamma_i^{SC}, \eta_i)$ as in the main paper. Rows (2), (5), and (8) impose homogeneity on all parameters. Rows (3), (6), and (9) impose homogeneity only on the semielasticity estimate η and allow for homogeneous bias $\gamma_i^M, \gamma_i^{SC}$. Row (1) is the specification that we use in the body of the paper.

Appendix C.2, we describe an empirical test for this concern. We reject large effects of beliefs on the marginal nonfinancial utility of betting, validating our welfare assumption.

Robustness to alternative parameter estimates We summarize the implications of alternative estimation strategies for our results in Table 3. We consider alternative approaches to heterogeneity in parameters and alternative estimation strategies for the semielasticity parameter η.[50]

Estimation strategies that impose homogeneous bias greatly affect our results. Comparing row (1) of Table 3 (our preferred results) to row (2) (results that impose homogeneous bias), we see that imposing homogeneous bias both overstates the optimal tax rate and the welfare gains from the tax. With homogeneous bias, we would also mistakenly conclude that a uniform tax was first best. These results demonstrate the empirical importance of carefully attending to heterogeneity in bias.

By contrast, our results are stable when we use different price-sensitivity estimates. The optimal tax rate remains in the relatively narrow range of $[4.59\%, 5.17\%]$. Specifications that use steeper demand slopes (such as row (7), which uses the observed effect of the Bet Less Bonus) do have larger consumption responses to taxes and, therefore, larger welfare effects of taxes (Har-

[50]We describe our strategies for semielasticity estimation in Section 6.3. We visualize average estimates for all strategies in Panel A of Figure 9 and heterogeneous estimates Appendix Figure B.17.

berger, 1964). Quantitatively, though, these differences are small.[51]

Robustness to nonlinear utility of numeraire consumption Our quasilinear utility form implies that agents are risk-neutral over numeraire consumption. We relax this assumption in Appendix C.1. In a model with nonlinear numeraire consumption utility, the formula for the overall price-metric bias (and therefore for the optimal tax) now includes additional terms that arise because overoptimistic agents misperceive their marginal utility of numeraire consumption. Directionally, these extra terms cause higher optimal tax rates for risk-averse and overoptimistic agents, but we argue that they are quantitatively negligible for reasonable parameterizations.

Bans While an outright ban on sports betting is obviously a blunt instrument, it is evidently politically feasible: sports betting is still illegal in many states, including California and Texas. To evaluate the welfare effects of bans, we require additional evidence on the overall perceived net benefits of betting away from the margin. If overall perceived benefits are larger than uninternalized costs, then a ban is welfare-reducing. To measure perceived benefits, we follow Brynjolfsson et al. (2019) and elicit the willingness to accept to completely stop betting on tracked apps for 30 days. We find that on average, the perceived net benefits are larger than the internality from overoptimism (Appendix Figure C.1), which suggests that bans reduce welfare. We provide details on our elicitation and discuss important caveats in Appendix C.5.

9 Experimental Evidence on Bias-correction Interventions

The simulation results in Figure 10 illustrate the potential of bias-correction interventions in theory. However, it is challenging to create interventions that correct bias in practice. In this section, we analyze two specific interventions which sportsbooks claim reduce bias, historical winnings transparency and voluntary wager limits. These interventions do not deliver the welfare gains that are promised by hypothetical bias-correction interventions.

9.1 History Transparency

In the history transparency treatment, we showed people information about their past returns. This information impacted predictions in the expected direction. Panel A of Figure 11 shows that people who received good (bad) news about past returns updated positively (negatively) about future returns. The coefficient on the slope of the best fit line implies that every 1¢ of good news about past returns increased beliefs about future returns by 0.089¢. This result demonstrates that beliefs are at least somewhat malleable, which implies it is possible to correct overoptimism by sending people the right signal.

Unfortunatley, information about past returns is not the right signal: while the history transparency treatment affected beliefs, it did not do so *in a way that reduced bias.* We provide two results

[51] A caveat is that as redistributive or revenue-raising motives become more relevant, price-sensitivity matters more. The optimal tax rate in our more general planner's problem (Appendix C.4) illustrates this point theoretically.

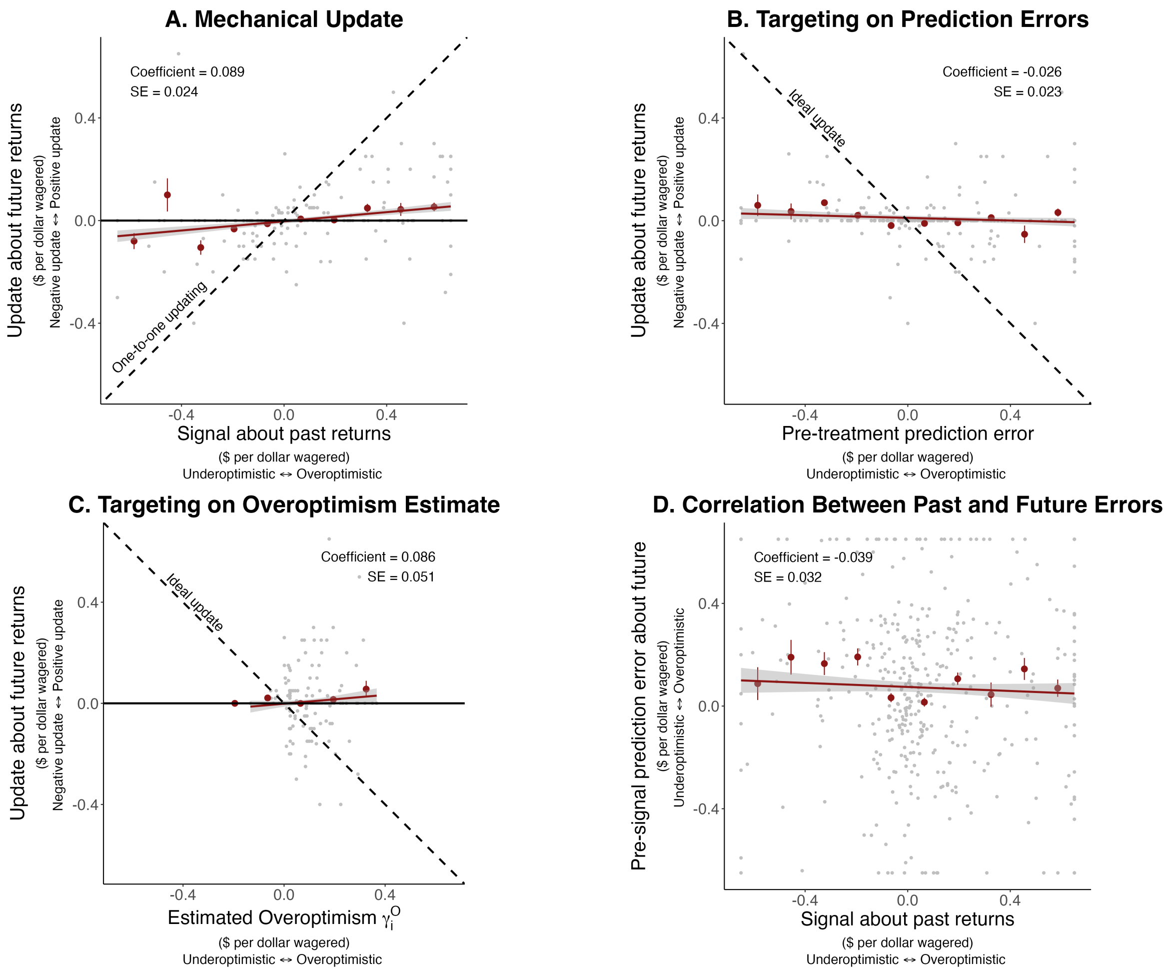

Figure 11: The effects of history transparency on misperceptions

Notes: The figure summarizes the effects of our History Transparency Treatment on beliefs. Panel A shows how updates correlate with the observed signal about past returns. The update variable is equal to the future prediction minus the initial prediction, or zero if the agent chose not to change the prediction. The Signal variable is equal to reported past returns minus the true past returns, truncated to lie within $[-0.65, 0.65]$ (since this is also the domian of the update variable). Panels B and C show how updates depend on two measures of misperceptions, the raw prediction errors in panel B and our shrunk misperception estimates in panel C. If the intervention were well-targeted, participants would update along the dotted line (so that overoptimistic bettors become less overoptimistic). We do not observe this pattern. Panel D shows that the signal about past returns was not correlated with pre-signal prediction errors. Points correspond to treated participant in panels A through C, while pannel D includes a point for both treated and control participants to improve precision. Lines, coefficients, and standard errors all come from univariate regerssions.

that allow us to draw this conclusion.

First, we show that the average treatment effect did not counteract average bias. Instead, the treatment induced *positive* updates on average, though the average update is not distinguishable from zero (Appendix Table B.4). The reason there was no beneficial average treatment effect is related to our evidence that selective memory is likely not the mechansism driving overoptimism. On average, people did not underestimate past returns (Appendix Figure B.1), so they did not receive bad news about the past, and the treatment did not reduce predictions about the future.

Even though the treatment had no average effect, it could still have had well-targeted treatment effects – i.e, it could have reduced predicted future returns more for the overoptimistic bettors. Our second result is that this was not true: treatment effects were not well-targeted at bias. Panels B and C of Figure 11 show that bettors with high overoptimism according to two measures (raw prediction errors and shrunk overoptimism estimates, respectively) do not update more negatively. To understand this result, we again examine the backward-looking errors about past returns. For the treatment effect to be well-targeted, more overoptimistic bettors should receive worse news. Panel D of Figure 11 shows that this is not the case: over-estimation of future returns is not correlated with over-estimation of past returns.

On the whole, these findings suggest that interventions aiming to address forward-looking overoptimism by correcting backward-looking biased beliefs in the sports betting context face fundamental challenges. We view study of alternative approaches to overoptimism-correction as an important area for future research on sports betting.

9.2 Voluntary Wager Limits

The limits treatment prompted participants to make choices about whether to set binding wager limits. Our main result is that some, but not all, participants with perceived self-control problems chose to set binding limits. Therefore, the limit tools only partially addressed self-control problems. To show our result, we plot the reduction in betting consumption that participants chose when setting limits alongside participants' ideal consumption reductions in the left panel of Figure 12. We use two complementary measures of ideal consumption reductions: the stated ideal consumption reduction from a question on survey 1 and the implied consumption reduction from a counterfactual that sets self-control problems γ^{SC} equal to zero in our model. These estimates are quantitatively different, but the consumption reduction implied by limit choices is substantially smaller than either estimate. The average bettor only sets limits that would reduce consumption by 3%.[52] Even using the smaller estimate of ideal consumption reductions, this result implies that voluntary limits tools mitigate less than half of the overconsumption from self-control problems in our sample.

While it is disappointing that voluntary limits only partially address self-control problems, it

[52] Such low take-up is consistent with qualitative evidence that people do not express interest in using limit tools (Appendix Figure B.9).

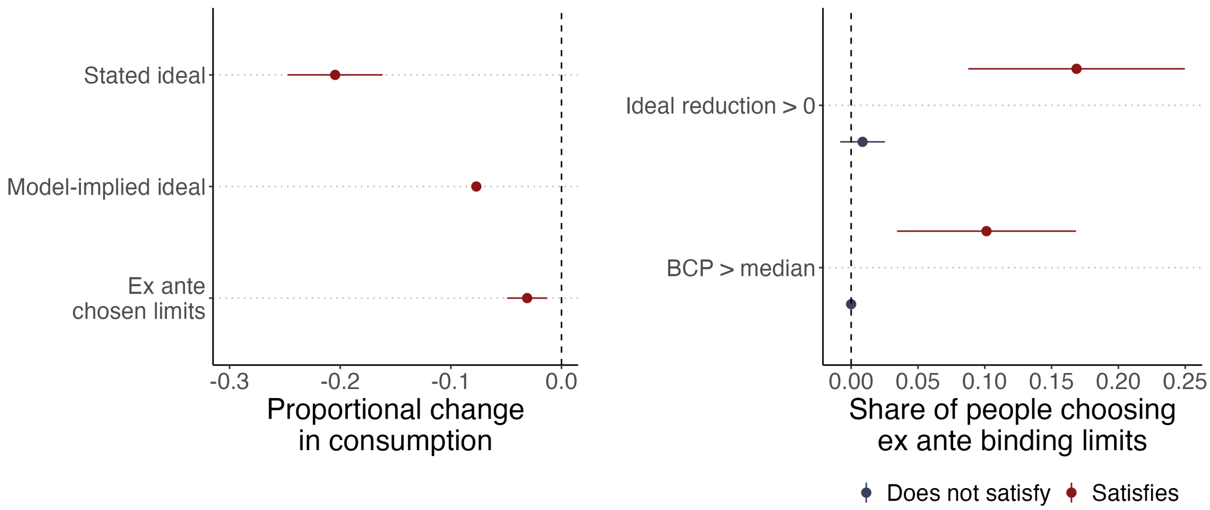

Figure 12: Limit choices

Notes: This figure summarizes limit take-up among the subgroup of bettors in the limits treatment who did not have difficulty finding limit screens for all apps (N=200). The left panel shows three average proportional consumption reductions: the stated ideal proportional reduction, the ideal proportional reduction implied by the model estimates of $E[\tilde{\gamma}_i^{SC}]$ and $E[\eta_i]$, and the proportaional reduction implied by limit choices. The stated ideal proportional reduction and ex ante chosen limits reductions are computed by dividing stated ideal consumption and the sum of all wager limits by predicted dollars wagered, respectively. The model-implied ideal is just $E[\tilde{\gamma}^{SC}] \cdot E[\eta_i] = 0.007 \cdot 11\% = 7.7\%$. The right panel shows how the share of participants setting binding limits varies according to two measures of self-control problems. Ideal reduction > 0 is the subset of people whose stated ideal consumption was less than predicted consumption, and BCP $>$ median is the subset with above-median Behavior Change Premium as defined in Section 6.2.

is not all bad news. Unlike history transparency, voluntary limits are well-targeted at bias. The right panel of Figure 12 shows that people without self-control problems almost never set binding limits, so the limit tools only reduce consumption for biased bettors. Because of this targeting, voluntary limit tools could yield improvements even when taxes are set optimally (Allcott et al., 2022a).

Why do some agents who perceive themselves to have self-control problems not set binding limits? One answer, consistent with the theoretical discussion in Laibson (2015), is that uncertainty about the future demand for betting causes people to avoid making binding commitments even when they want to reduce future consumption for any given demand realization. Qualitative evidence shows that many bettors are uncertain about the ideal amount to bet in the future (Appendix Figure B.10) and that this uncertainty played a role in limit decisions (Appendix Figure B.11). Ultimately, our small sample size and the paucity of self-control problems in the sample make it difficult for us to draw firm conclusions about the determinants of limit take-up. We interpret our results as suggesting that the demand for flexibility could be an important barrier.

10 Discussion

10.1 Considerations for Policy Beyond the Scope of this Analysis

We view this paper as only a first step in the welfare analysis of sports betting regulation. We briefly note some important limitations of the present analysis and qualitatively describe how these might affect optimal policy.

First, our analysis accounts only for the uninternalized costs driven by overoptimism and self-control problems. On top of these, there are reasons to believe that sports betting causes negative externalities. Potential sources of externalities include, but are not limited to: impacts on intimate partner violence (Matsuzawa and Arnesen, 2024; Card and Dahl, 2011), impacts on non-betting sports fans, impacts on match fixing, and psychological impacts on athletes. There also could be other internalities, such as projection bias in predicting the pain of losing (Loewenstein et al., 2003). Our model can in principle easily accommodate a variety of externalities and internalities. Analysts need only compute the damages per dollar wagered (at the margin), and add a new component to the price-metric bias term. To the extent that these concerns are empirically relevant, optimal policy is more restrictive than implied by our estimates.

Second, we do not study the dynamics of sports betting consumption. The psychiatric literature categorizes some forms of problem gambling as a behavioral addiction (American Psychiatric Association, 2013), so at least for some bettors, habit formation is important. Habit formation can exacerbate static self-control problems (Gruber and Köszegi, 2001; Allcott et al., 2022b), which would make optimal policy more restrictive than implied by our estimates.

Third, our analysis abstracts away from the statutory incidence of taxation. In practice, whether a tax is levied federally or by a state likely matters for the tax's local impact on prices. Historically, sportsbooks have set uniform odds across state lines.[53] Under uniform national pricing, an increase in the local sports betting tax would have minimal pass through to local prices. Because of imperfect pass through, we expect consumption responses to state taxes to be muted, which limits their ability to correct overconsumption. If the social planner's goal is to address overconsumption through Pigouvian taxation, it is more straightforward to do so via a federal tax.

Fourth, we do not model the supply side. If firms have market power, their markups act as an offsetting distortion to biases (O'Connell and Smith, 2024). Optimal policy would account for this distortion by being less restrictive. An interesting implication is that, holding status quo tax rates fixed, granting monopoly power to a single sportsbook operator could reduce consumption and bring it closer to the surplus-maximizing point. There is precedent for such a legal structure inter-

[53] The practice of setting uniform prices across space is consistent with the practice of some large U.S. retail chains (DellaVigna and Gentzkow, 2019). The ability to a different jurisdiction might also drive prices more towards uniformity, though substitution is not frictionless – it is both illegal and technically challenging for someone to, say, place bets on a New Jersey mobile sportsbook if they are physically in New York. Recently, one sportsbook, DraftKings, considered passing on the price of the New York tax rate to consumers with an extra fee. They canceled this plan after facing backlash from users. https://www.timesunion.com/business/article/draftkings-cancels-unpopular-surcharge-new-york-19656021.php

nationally for sports betting and in the U.S. for other kinds of gambling.[54] We do note, following our third caveat, that the monopoly may need to be implemented on the federal level to have the desired impact on prices.

Fifth, our study is not designed to study severe problem gambling and the associated negative outcomes of affected bettors, which can include financial distress, associated mental health problems, and suicide. We do not have statistical power to study these issues. To the extent that policymakers weigh these costs, optimal policy is more restrictive than implied by our estimates.

10.2 Conclusion

The optimal regulation of sports betting depends on the extent to which betting activity is driven by preferences versus biases. In a field experiment with a sample of high-volume sports bettors, we document that participants substantially overestimate their own net returns to betting. They also are willing to pay money to reduce their own betting in the future, which is consistent with self-control problems. Combining these estimates with estimates of price-sensitivity, we estimate a model of biased sports betting. Using our preferred estimates, we compute a surplus-maximizing corrective tax of 5.17%. We estimate a great deal of heterogeneity in bias, which implies that the optimal excise tax only delivers 55% of the first-best total surplus. In principle, targeted interventions that reduce bias can allow society to move closer to the first-best. However, when we evaluate two proposed bias-reducing interventions, winnings history transparency and self-imposed limit tools, we find that the former is not well-targeted and the latter only partially reduces self-control problems.

Currently, most of the public discussion around sports betting taxation takes a Ramsey view of taxation, focusing on the benefits of raising revenues for states while implicitly viewing consumption reductions from taxation as costly distortions. By contrast, our paper highlights the Pigouvian perspective. We show that internalities cause unregulated consumption to be above the social optimum, so consumption reductions are socially *beneficial* rather than costly.

Our analysis of bias-correction interventions treatments highlights the importance of further research on platform design and targeted interventions. Uniform taxes involve a tradeoff between reducing the betting of biased agents (which is desirable in our model) and reducing the betting of unbiased agents (which is not). Platform features that directly eliminate bias circumvent this tradeoff, because they reduce mistake-driven betting while allowing unbiased bettors to continue deriving enjoyment from betting. While the two interventions that we studied did not accomplish this goal, the failures were due to the idiosyncratic designs of those interventions and are not necessarily fundamental to the enterprise of bias-correction. Our results on the mechanisms driving bias – for example, that overoptimism is much more prominent among parlay bettors – provide

[54] For example: U.S. states maintain monopolies on lottery betting; Norway has a state-run company (Norsk Tipping) with a monopoly on all kinds of gambling, including sports betting; and the state of Oregon has granted a monopoly on legal sports betting to DraftKings.

insights for the design of future targeted interventions.

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Tables and Figures

Appendices

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A Experimental Design Appendix

Survey instruments are available at mattbrownecon.github.io.

A.1 Timeline and recruitment details

While the vast majority of participants found the study through our advertisements, a few were recruited via a snowball design, in which current participants were asked to referrals friends who were regular mobile sports bettors. Referred participants were always assigned to the same treatment condition as the person who recruited them. We refer to the original participant and the people they recruited as a "referral group," and we always cluster standard errors by referral group. In practice only 8 members of the analysis sample (2%) came from the snowball recruitment procedure.

We undertook several procedures to minimize attrition among the 533 participants who completed survey 1. Participants received text and email reminders to take the surveys, and we delayed most payments until after survey 3 to encourage participants to return. Since we wanted to observe full bet histories, we also emailed survey 3 to all participants who completed survey 1, regardless of whether they completed survey 2. Some participants had technical problems refreshing their account data. We provided extensive support to help participants resolve such issues over email. When a participant was unable to refresh one of their accounts, we still allowed them to complete the survey.

A.2 Index definitions

Problem Gambling Severity Index The Problem Gambling Severity Index (PGSI) is a survey instrument from the psychiatric literature that is designed to provide a summary measure of problem gambling risk in non-clinical contexts (Holtgraves, 2009). Participants report how often they experienced the following nine consequences in the past year (options: Never, Sometimes, Most of the time, Almost always)

- *Have you bet more than you could really afford to lose?*
- *Have you needed to gamble with larger amounts of money to get the same feeling of excitement?*
- *When you gambled, did you go back another day to try to win back the money you had lost?*
- *Have you borrowed money or sold anything to get money to gamble?*
- *Have you felt that you might have a problem with gambling?*
- *Has gambling ever caused you any health problems, including stress or anxiety?*
- *Have people criticized your betting or told you that you had a gambling problem, regardless of whether or not you thought it was true?*

- *Has your gambling caused any financial problems for you or your household?*
- *Have you felt guilty about the way you gamble or what happens when you gamble?*

We asked these questions on survey 1. As pre-registered, we construct the PGSI score by assigning responses of Never, Sometimes, Most of the time, and Almost always scores of 0, 1, 2 and 3, respectively, and summing across the nine questions to compute the PGSI score. Among gambling policymakers, PGSI scores in the ranges of $\{0, [1, 2], [3, 7], [7, 21]\}$ are considered indicators of "no risk," "low risk," "moderate risk," and "high risk" for problem gambling, respectively.

Self-control scale We adapt a positive play behavior scale from Wood et al. (2017). The scale is designed to measure the extent to which a participant engages with sports betting in a healthy manner. We elicit agreement/disagreement with the following items.

- *In the last 30 days, I felt in control of my sports betting behavior.*
- *In the last 30 days, I was honest with my family and/or friends about the amount of money I spent sports betting.*
- *In the last 30 days, I was honest with my family and/or friends about the amount of time I spent sports betting.*
- *In the last 30 days, I only bet on sports with money that I could afford to lose.*
- *In the last 30 days, I only spent time sports betting that I could afford to lose.*

The options are a five-point Likert scale (Strongly disagree (-2), disagree (-1), neither agree nor disagree (0), agree (1), strongly agree (2)). We sum the question scores to create theself-control scale, which ranges from -12 to 12.

Gambling literacy scale We adapt a gambling literacy scale from (Wood et al., 2017). We elicit agreement with the following three items on survey 2.

- *Gambling is not a good way to make money for most people.*
- *My chances of winning get better after I have lost. (reverse coded)*
- *If I gamble more often, it will help me to win more than I lose. (reverse coded)*

The options are a five-point Likert scale (Strongly disagree (-2), disagree (-1), neither agree nor disagree (0), agree (1), strongly agree (2)). Numeric scores written here correspond to the first question where "strongly agree" is "correct;" we reverse the order of the scores for the other questions. We sum the question scores to create a gambling literacy score which ranges from 6 to -6.

Sports betting makes life better Following Allcott et al. (2022), we asked in all surveys: *To what extent do you think sports betting makes your life better or worse?* Participants could respond on a scale from -5 ("makes life much worse") to 5 ("makes life much better"). The question is designed to qualitatively capture the extent to which people derive value from sports betting.

A.3 Overall consumer surplus elicitation

We elicted the WTA to stop betting entirely on tracked apps in the 30 days after survey 3 via a BDM mechanism that was implemented for one participant. We interpret this WTA as the perceived valuation of the ability to place bets on tracked apps for 30 days. This valuation is useful for analysis of policies like total bans on sports betting or uniform caps on sports betting consumption.

Since all of our policy analysis focuses on the period between survey 1, and survey 3, a more appropriate object of interest is really "the total surplus from betting on all apps between in the 30 days after survey 1." We say "all apps" because to the extent that participants stated a low value utilized untracked apps, low valuations may reflect easy cross-app substitution rather than a genuine low WTA. To extrapolate from our incentivized WTAs to the target, we ask a set of hypothetical WTAs: 1) the WTA to stop betting on all apps for 30 days, 2) the WTA to stop betting on tracked apps for 30 days (unincentivized), and 3) the WTA to stop betting on tracked apps for 30 days if the sports schedule had been "similar to the schedule between survey 1 and survey 2 (April 9th to May 9th)." Differences between WTAs to hypothetical questions are informative about the extent to which the incentivized WTA differs from the target.

A.4 Risk preferences elicitation

We elicit a measure of risk preferences over experimental earnings by measuring participants' willingness to pay for a 50% chance to receive \$100. A risk-neutral participant will value this opportunity at \$50 exactly. Risk-averse participants will report lower valuations, and risk-loving participants will report higher valuations.

Our implementation follows an elicitation procedure from Allcott et al. (2022c). We frame the 50% chance as a "coin toss for \$100." Then, we introduce a two-step incentivized MPL. We begin by showing participants binary choice between \$50 for sure and the coin toss. Then, depending on the response to this question, we elicit choices between the coin toss and other fixed payments. For one randomly selected participant, one of these choices is implemented.

A.5 Tables and Figures

Audience Group	Example Targeted Interests	Number of clicks
General Sports Betting (GSB)	*Any of:* Sportsbooks Daily fantasy sports DraftKings FanDuel Barstool Sports	3,752
Sports Fans	*Any of:* Soccer Baseball Basketball ESPN DraftKings FanDuel NBA Premier League	1,634
GSB $\cap$ Sports Fans	$\approx$ *At least one interest in GSB* *AND a separate sports interest*	3,743
GSB $\cup$ Casinos	*Any of GSB or:* Casino Games Lotteries Online gambling	1,514
GSB $\cup$ Beer	*Any of GSB or:* Pabst Blue Ribbon Budweiser Coors Light Miller Light Natural Light	1,478
Bill Simmons	Bill Simmons	968
GSB $\cup$ Day Trading	*Any of GSB or:* Day Trading	396
Total		13,485

Table A.1: Targeted audience groups for Facebook and Instagram Ads

Notes: The table reports audience groups that were targeted with our social media ads. The reported interests are summaries of multiple interest categories as defined by Facebook (for example, "Sportsbooks" includes "Sportsbook (game)" and "Sports betting (gambling)"). The number of unique link clicks are the number of unique users who clicked on an add that was targeted using a particular interest keyword. The total number of unique link clicks in this table is slightly higher than the number of unique link clicks reported in Table 1 because a user who was targeted with ads from two categories and clicked on both ads will be counted in both groups in this table, but only once for Table 1.

Figure A.1: Example advertisement

Notes: We fielded this advertisement and qualitatively similar advertisements from March 13 through April 3 on Facebook and Instagram. Clicking on the ad directed users to our intake survey.

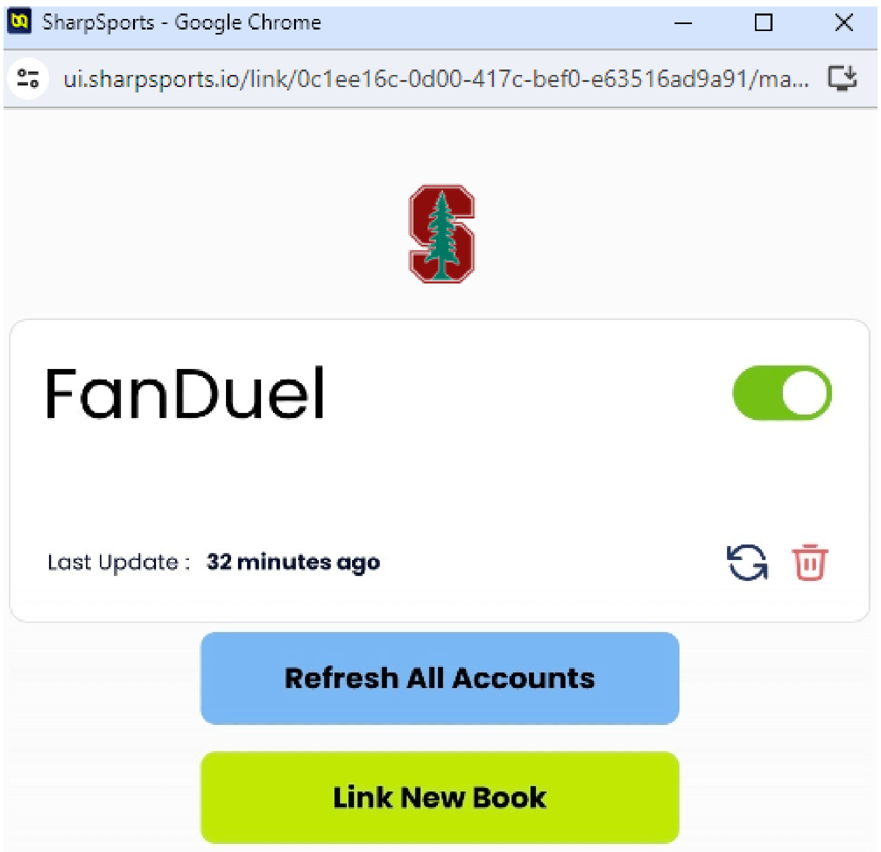

Figure A.2: Example syncing portal

Notes: The image is a screenshot of the SharpSports syncing portal for a participant who only used FanDuel.

B Reduced-form Results Appendix

B.1 The impact of rounding on prediction data

Our predictions data show excess mass at zero and at multiples of 5 cents. This fact suggests that self-reported data may be only a rounded representation of the true underlying belief. In this section, we conduct a test to see how, if at all, this rounding impacts our average misperception estimates.

Let j index "focal" predictions which agents round to. We assume that an agent whose underlying belief is within bandwidth d of a focal prediction reports their true belief with probability p_j and the focal prediction with probability $(1 - p_j)$. To estimate the p_js, we measure the excess mass at each focal prediction, that is, the share of responsdents reporting the focal prediction minus the expected share reporting the prediction if people reported all predictions in the bandwidth with equal probability. Then, we construct a counterfactual un-roudnded distribution that re-allocates excess mass to non-focal predictions, using the assumption that the frequencies of reported non-focal predictions represent a scaled-down version of the frequencies of underlying non-focal beliefs. Allcott et al. (2022c) conduct a similar procedure to test for the impact of rounding on self-reported predicted reborrowing probabilities.

The results of this test show that if anything, rounding causes our predictions data to somewhat understate average misperceptions. We illustrate CDFs of rounding-adjusted belief distributions for various focal beliefs and bandwidths in Figure B.19. The rounding-adjusted beliefs are on average to the right of the unadjusted distribution. This is because zero is by far the largest focal belief, and many more people report predictions slightly above zero than slightly below zero. Our procedure interprets this as evidence that more people round down than round up.

B.2 Estimating self-control problems

Theoretical Argument The following proposition shows that the *behavior change premium per dollar* identifies perceived self-control problems under the assumption that a $0.02 price change is not too large relative to the curvature of betting demand.

Proposition 1. *Let $\tilde{x}(\tau)$ denote an agent's predicted future demand for betting as a function of a unit price τ. Let $V(\Delta; \gamma^{\tilde{S}C})$ denote the valuation of a change in the price of betting by Δ. Assume the agent has perceived self-control problems $\tilde{\gamma}^{SC}$ and terms of order $\Delta^2 \cdot \tilde{x}''(\tau)$ are negligible. Then:*

1. *The difference between the valuation of agent with self-control problems and that of a time-consistent agent is equal to price-metric perceived self-control problems times the predicted demand response: $V(\Delta; \tilde{\gamma}^{SC}) - V(\Delta; 0) = -\tilde{\gamma}^{SC} \cdot \tilde{x}'(\tau) \cdot \Delta$*

We provide a proof in Section E. The logic follows ideas from Appendix A.1 of (Carrera et al., 2022).

The BCP is constructed as an estimate of the excess valuations of a price change over and above a time-consistent agent's valuation – this is exactly $V(\Delta; \tilde{\gamma}^{SC}) - V(\Delta; 0)$. As a consequence, the BCP divided by the predicted demand response $\Delta \cdot x'(\tau)$ (i.e, the BCP per dollar) is a direct estimate of $\tilde{\gamma}^{SC}$.

For intuition, think back to definition of perceived self-control problems $\tilde{\gamma}$ from Section 3 of the main paper. Consider the perceived expected marginal utility of dollars wagered at the chosen consumption level for the long-run self. The long-run self predicts that his future self will optimize according to in-the-moment decision utility, which implies that the marginal decision utility will be zero at the chosen dollars wagered. The agent also predicts that, becasuse of self-control problems, the future self will over value dollars wagered by $\tilde{\gamma}_i^{SC}$. Therefore, from the perspective of the long-run self, the predicted value of wagering the marginal dollar is negative $\tilde{\gamma}_i^{SC}$. An empirical implication of this reasoning is that the agent would pay $\tilde{\gamma}_i^{SC}$ to avoid consuming this marginal dollar. For infintesimal price changes, the behavior change premium per dollar is equal to exactly this willingness to pay. For realistic price changes, the behavior change premium per dollar is an approximation of this willingness to pay.

Advantages of the approach We chose this measurement strategy in part because it is robust when bettors are uncertain about future normative demand. A related but different strategy uses the take-up of binding commitment devices to measure perceived self-control problems.[55] This strategy faces challenges when future demand is uncertain. Under such uncertainty, agents who want to correct self-control problems might still avoid taking up commitment (Laibson, 2015). Intuitively, if an agent does not know their future optimum, they will not want to take up binding commitments that might cause consumption to be suboptimally low. So the commitment device take-up strategy can create false negatives (Strack and Taubinsky, 2021).[56] Such uncertainty is relevant in our context. A majority of bettors agree that "It's hard for me to predict in advance how much I'll want to bet in a given week." (Appendix Figure B.10). Our strategy avoids such issues because it measures the extent to which people would trade future cash payments for future changes in betting consumption, regardless of the level of future normative demand.

Empirical Implementation In the experiment, we use observed bonus valuations v_i^B to learn about the BCP. We model the bonus as a combination of a price change of size $\Delta^B = 0.02$ plus a fixed payment K_i. We also observe predicted consumption in control and in treatment, $(\tilde{x}_i^C, \tilde{x}_i^B)$. For most agents, the price change is always binding (i.e, the agent consumed less than \$150/day or \$1050/week so they will never reach the maximum). Proposition 1 then tells us that the bonus valuation is $-\tilde{\gamma}^{SC} \cdot \tilde{x}'(\tau) \cdot \Delta$ higher than a time-consistent person's bonus valuation would be. Therefore, we can substitute the time-consistent person's bonus valuation and rearrange solving

[55] See, for example DellaVigna and Malmendier 2006; Kaur et al. 2015; Augenblick et al. 2015.

[56] Another prominent strategy for measuring self-control problems is *choice-reversal* designs (Read and van Leeuwen, 1998; Sadoff et al., 2020; Augenblick et al., 2015; Augenblick and Rabin, 2019). These designs show that choices made in-the-moment differ from choices made in advance. While these designs are attractive in some contexts, Strack and Taubinsky (2021) show that choice reversal designs are generally not robust to uncertainty about future demand. They also show that the approach we implement requires weaker assumptions for identification.

for $\tilde{\gamma}$

$$V(\Delta^B;0) - V(\Delta^B;\tilde{\gamma}^{SC}) = -\tilde{\gamma}^{SC} \cdot \tilde{x}'(\tau) \cdot \Delta^B \tag{B.1}$$

$$v_i^B - \left[K_i - \Delta^B \left(\frac{\tilde{x}_i^C + \tilde{x}_i^B}{2}\right)\right] = -\tilde{\gamma}_i^{SC} \tilde{x}'(\tau) \cdot \Delta^B \tag{B.2}$$

$$v_i^B - \left[K_i - \Delta^B \left(\frac{\tilde{x}_i^C + \tilde{x}_i^B}{2}\right)\right] = -\tilde{\gamma}_i^{SC} \left(\tilde{x}_i^B - \tilde{x}_i^C\right) \tag{B.3}$$

$$\implies \tilde{\gamma}_i^{SC} = \frac{\overbrace{\overbrace{v_i^B}^{\text{MPL valuation}} - \overbrace{\left[K_i - \Delta^B \left(\frac{\tilde{x}_i^C + \tilde{x}_i^B}{2}\right)\right]}^{\text{Time-consistent valuation}}}^{\text{Behavior Change Premium}}}{\underbrace{\tilde{x}_i^B - \tilde{x}_i^C}_{\text{Demand response to treatment}}} \tag{B.4}$$

In practice, the survey-reported terms $(v_i^B, \tilde{x}_i^C, \tilde{x}_i^B)$ are measured with error. The BCP from itself (i.e, the numerator of equation (B.4)) is designed to be robust to mean-zero response noise in these parameters (Carrera et al., 2022). Therefore, the significant positive Behavior Change Premium that we illustrated in Panel A Figure 7 is a result showing that people perceive themselves to have self-control problems that is robust to such measurement error. By contrast, since the BCP per dollar (equation (B.4)) involves dividing one term measured with error by another, the measurement error is nonclassical. Therefore, estimating $\tilde{\gamma}_i^{SC}$ separately for each individual and then averaging across individuals need not deliver an estimate of the sample average $E_i[\tilde{\gamma}_i^{SC}]$. Instead, following Allcott et al. (2022c), we replace these values with sample averages $\overline{v}_g^B$, $\overline{x}_g^C$, and $\overline{x}_g^B$ for to compute BCP per dollar estimates within subgroups g. Then, we aggregate these estimates back up into sample-wide estimates by weighting the subgroup-level estimates by the size of the subgroup. Concretely, our estimates use above- and below-median wager volume as the subgroups. When we estimate multiple heterogeneous BCPs per dollar, we adopt this approach within the relevant sample to compute each estimate.

There is one other modification. The approach as written here assumes that the price change applies to the entire the demand curve. In the experiment, the price change only applies to dollars wagered below the benchmark value. We also set a maximum bonus payment of \$90, so for some participants, the price change only applied to the first \$4500 in reduced betting below the benchmark. We do two things to deal with these subtleties. First, we restrict to participants whose predicted consumption in both treatment and control lie within the region where the price change is binding, $[B - 4500, B]$. This is a subsample of 352 participants (79% of the analysis sample). Second, for participants with $B > 4500$, we use $B - 4500$ as the left edge of the trapezoid defining CS loss rather than 0. For these participants, the trapezoid is approximated with $\left(\frac{x_i^C + x_i^B}{2} - (B - 4500)\right) \cdot 0.02$.

B.3 Representativeness and weighted sample analyses

Grubbs and Kraus (2023c) (henceforth GK) conducted two YouGov surveys to learn about the population of sports bettors.[57] The first was a sample of 2,806 Americans defined to match the 2019 ACS on demographics. The second was a sample of 1,557 self-reported sports bettors, weighted to match the demographics of sports bettors in the first sample.

We construct two subsamples of the GK data. *GK weekly sports bettors* includes all participants from either survey who report sports betting weekly or more frequently (N = 406). *GK weekly lottery bettors* includes all participants who reported buying lottery tickets weekly or more from the first survey only (N = 517). We report means of demographic characteristics and of qualitative bias measures for the U.S. population, each GK sample, and our analysis sample in Table 2. We also report means for our re-weighed sample. We construct initial weights using entropy balancing (Hainmueller, 2012) to match the GK sports bettors on the education dummies and qualitative bias measures, but censor weights at $[-1/10, 10]$ to preserve precision.

Table B.5 shows the characteristics of our weighted sample. The sample nearly matches the GK sample on targeted characteristics, as expected. It also becomes directionally more representative on income.

We recreate our reduced form results on overoptimism, perceived self-control problems, naivete about self-control problems, and price-sensitivity in Figures B.22, B.23, B.24, and B.25 respectively. Qualitatively, bias estimates get larger. The raw prediction error in the reweighted sample (Figure B.22) is about 50% larger than the raw prediction error in the main sample (Figure 5). The behavior change premium is also larger in the reweighted sample than in the reweighted sample, though it is hard to detect a difference given that both estimates are small. As in the main sample, we find no evidence of naivete. Our price-sensitivity estimates are also qualitatively similar, but less precise.

Overall, these results suggest that our bias estimates are a conservative measures of bias in the population. Taken seriously, they indicate that optimal policy should be even more restrictive than our estimates imply. Of course, this exercise has its limitations. In particular, it does not engage with the full range of reasons that our sample is nonrepresentative of sports bettors. It also does not extrapolate to the population of infrequent sports bettors or of non-bettors who may begin sports betting in the future. Nevertheless, we view this directional conclusion as reasonable.

B.4 Details on shrinkage procedure and results

We estimate a distribution of latent overoptimism from noisy data on prediction errors with a shrinkage procedure. To do this, we define the overoptimism estimand γ_i^O as the difference between perceived expected net returns $E_{\tilde{F}}[a] = r_i^{Perc}$ and true expected net returns $E_F[a] = r_i$, both

[57] The authors and collaborators have published several other descriptive studies using this data, including Grubbs and Kraus (2023b, 2022, 2023a); Connolly et al. (2024)

of which are time-invariant. In periods $t = 1, 2$, we observe noisy signals of true expected returns and of perceptions. Realized net returns $\hat{r}_{it}$ are noisy measures of true expected returns because of the randomness of sports outcomes, and predicted net returns $\hat{r}^{Perc}_{it}$ are noisy measures of perceived skill because of the usual reasons for error in survey elicitations (Kahneman, 1965; Gillen et al., 2019; Haaland et al., 2023). We plot the distribution of these raw prediction errors in Figure B.20. They are quite dispersed, but this is partly because of noise – the variance of raw prediction errors $\hat{\gamma}^{O}_{it} = \hat{r}^{Perc}_{it} - \hat{r}_{it}$ overstates the variance of latent overoptimism γ^{O}_{i}.

To obtain our overoptimism estimates, we shrink raw prediction errors towards the population mean. The inputs to our shrinkage procedure are the prediction errors and their variances. To compute the variances, we assume that predicted net returns and realized net returns are the sum of the underlying objects and mean-zero noise.

$$\hat{r}_{it} = r_i + \nu^{Realization}_{it} \qquad E[\nu^{Realization}_{it}] = 0; \quad V[\nu^{Realization}_{it}] = \sigma^2_{Realization,it} \tag{B.5}$$

$$\hat{r}^{Perc}_{it} = r^{Perc}_{i} + \nu^{Response}_{it} \qquad E[\nu^{Response}_{it}] = 0; \quad V[\nu^{Response}_{it}] = \sigma^2_{Response} \tag{B.6}$$

We assume that prediction and realization shocks are independent, $E[\nu^{Realization}\nu^{Response}_{it}] = 0$.

The variance of responses $\sigma^2_{Response}$ is identified from the variance of changes in responses across surveys.[58] We estimate that response errors have a standard deviation of $\hat{\sigma}_{Response} = 12.3$¢.

Observation-specific realization variances $\sigma^2_{Realization,it}$ are computed directly from observed wager histories. For a bettor who places K bets in a period, we model realized returns as a weighted sum of K independent Bernoulli trials where the weights are proportional to dollars wagered in the bet and the trial success probability is given by the bet odds. Specifically, we approximate the probability of winning bet k with $\frac{1}{q_{itk}}$ where q_{itk} is the bet's payout per dollar for winners. Then, the variance of net returns per dollar is

$$\sigma^2_{Realization,it} = \sum_k s^2_{itk}(q_{itk} - 1) \tag{B.7}$$

where s_{ikt} is the share of dollars allocated to bet k. This variance formula captures two intuitive features of realization noise. First, realization noise is smaller when people disperse their dollars wagered across many bets, because of the law of large numbers. Second, realization noise is smaller when agents place "sure thing" bets rather than long shots. Because bettors vary a great deal along both dimensions, the distribution of $\sigma_{Realization,it}$ is quite dispersed. The median standard deviation is 30.19¢, but the 10th and 90th percentiles are 10.14¢ and 138¢ respectively. We plot the full distribution of standard deviations in Figure B.21.

Since the precision of realizations depends on features of betting behavior and prediction errors vary with respect to these features (Figure 6), there is a subtle bias in standard shrinkage methods. We found that low-volume bettors and parlay bettors are more overoptimistic. These are

[58] Specifically, the variance of changes in responses across surveys is equal to $2\sigma^2_{Response}$.

exactly the bettors who have less precise realizations, and therefore their average prediction errors are more noisy. Traditional shrinkage procedures inappropriately shrink these bettors' prediction errors towards the (lower) mean estimate among the high-volume, non-parlay bettors who have more precise signals. To avoid this issue, we use the CLOSE procedure developed in Chen (2024) to shrink our estimates. This method allows the average parameter of interest to vary flexibly with the precision of raw estimates. Our implementation assumes that the underlying overoptimism γ_i^O is Gaussian conditional on the standard errors of raw estimates. The output is the individual-level prediction error estimates illustrated in Panel B of Figure 6 in the main paper.

B.5 Bonus treatment effects

To estimate the treatment effects of the Bet Less Bonus, we use the following regression specification for participants i and periods $t \in \{1, 2\}$:

$$Y_{it} = \tau_t^B B_i + \beta_t X_i + \delta_t + \varepsilon_{it} \tag{B.8}$$

where Y_{it} is an outcome variable, B_i is a bonus treatment indicator, X_i is a vector of pre-specified individual-level covariates, and δ_t is a period fixed effect.[59] Coefficients $\hat{\tau}_t^B$ are estimates of the contemporaneous and long-run impacts of the Bet Less Bonus for $t = 1$ and $t = 2$ respectively.

When implementing this regression, we make two choices to ensure that our estimates can be interpreted as proportional average treatment effects of the price change. First, we define a normalized dependent variable Y_{it}^{Norm} to be the period-t outcome as a proportion of the period-0 outcome, and then scaled such that the control group always has an average of 1. Treatment effects on Y_{it}^{Norm} are interpreted as average proportional treatment effects relative to the control group (Chen and Roth, 2024). Second, we restrict the sample to the set of participants who could have been exposed to the price change. Recall that the Bet Less Bonus was a payment for reducing consumption below a personalized benchmark; treated participants who wagered more than the benchmark were not exposed to the price change. 6% of treated participants exceeded the benchmark, so we drop them from the sample. By construction, these are the treated participants with the highest normalized consumption outcomes, so to ensure that the treated and control groups are comparable we also drop the 6% of control group participants with the highest normalized consumption outcomes.

The estimates in Table B.6 show that the Bet Less Bonus caused consumption to fall by 34% (Column 1). People mainly reduced consumption by placing fewer bets. The share of people not betting at all doubled (Column 2), and the number of bets placed falls by 29% (Column 3). By contrast, people did not reduce their average dollars wagered per bet (Column 4). We did not observe significant persistent effects of the bonus, but our estimates are imprecise. Therefore, we

[59] The covariates are: a limits treatment indicator, log winsorized baseline wagers, elicited beliefs about period-1 winnings, a measure of information conveyed in the information treatment, white, income terciles, and randomization stratum indicators.

can neither rule out large habit formation nor zero habit formation.

Appendix Figure B.7 displays our results on substitution to other kinds of gambling. The top row represents the 34% treatment effect of the Bet Less Bonus on tracked sports betting. The rows below plot treatment effects for various other categories of gambling other than sports bets on tracked apps. We elicited spending on these other categories via an unincentivized question at the end of each survey, which participants knew would not affect their payments. The second row represents the treatment effect on the sum of spending on the individual categories, while the rows below represent treatment effects on individual categories.

B.6 Tables and Figures

State	Dollars Wagered (millions)	Tax Revenues (millions)	Excise-equivalent Tax (%)
State taxes			
New York	19,197	861.5	4.49
New Jersey	11,972	141.5	1.18
Illinois	11,620	161.0	1.39
Nevada	8,261	32.5	0.39
Pennsylvania	7,683	165.1	2.15
Ohio	7,594	133.3	1.75
Arizona	6,574	34.8	0.53
Virginia	5,588	72.3	1.29
Colorado	5,560	27.4	0.49
Massachusetts	4,989	93.8	1.88
Michigan	4,811	23.6	0.49
Maryland	4,617	46.2	1.00
Indiana	4,338	38.4	0.89
Tennessee	4,292	83.6	1.95
Louisiana	2,905	43.5	1.50
Iowa	2,420	13.4	0.56
Kansas	2,122	10.0	0.47
Connecticut	1,763	19.5	1.11
Kentucky	885	15.5	1.75
New Hampshire	822	35.6	4.33
Oregon	676	37.9	5.60
West Virginia	483	4.8	0.99
Mississippi	474	6.1	1.30
Rhode Island	461	20.5	4.45
Arkansas	405	5.3	1.32
Wyoming	172	1.1	0.62
District of Columbia	170	5.4	3.20
Maine	82	0.9	1.10
Delaware	65	8.4	12.92
Montana	62	1.4	2.24
South Dakota	9	0.1	1.02
Total	**121,075**	**2,144.5**	**1.77**
Federal excise tax			
Total	**121,075**	**302.7**	**0.25**
Federal + State			
Total	**121,075**	**2,447.2**	**2.02**

Table B.1: Summary of sports betting activity and taxes in 2023

Notes: The table summarizes sports betting activity and taxes in 2023. The first column shows the dollars wagered in each state in 2023. The second column shows the total tax revenues for each state in 2023. The third column computes the share of dollars wagered that each state retains as tax revenues, which we refer to as the excise-equivalent tax. At the bottom, we also report the size of the federal excise tax on gambling and the combined overall excise-equivalent rate. Data on state-level wagers and revenues was compiled by Legal Sports Report https://www.legalsportsreport.com/sports-betting/revenue/ (accessed September 19, 2024).

	Misprediction			Prediction	Realization
	(1)	(2)	(3)	(4)	(5)
Constant	0.0231	0.0351	0.0271	0.0284	-0.0078
	(0.0638)	(0.0662)	(0.0643)	(0.0189)	(0.0623)
Above median parlay share	0.1736***	0.1875***	0.1660***	-0.0241*	-0.2018***
	(0.0467)	(0.0510)	(0.0489)	(0.0138)	(0.0456)
No college degree	0.1345**	0.1348**	0.1298*	0.0204	-0.1045
	(0.0678)	(0.0678)	(0.0685)	(0.0201)	(0.0662)
Graduate degree	-0.0065	-0.0097	-0.0078	0.0139	0.0252
	(0.0496)	(0.0499)	(0.0498)	(0.0147)	(0.0485)
Above median live bet share	-0.0730	-0.0700	-0.0747	-0.0102	0.0688
	(0.0452)	(0.0455)	(0.0454)	(0.0134)	(0.0442)
Above median wagers	-0.0227	-0.0258	-0.0206	0.0105	0.0362
	(0.0466)	(0.0468)	(0.0468)	(0.0138)	(0.0455)
Above median age	-0.0319	-0.0336	-0.0291	-0.0100	0.0276
	(0.0458)	(0.0459)	(0.0461)	(0.0135)	(0.0447)
Above median income	0.0213	0.0223	0.0205	-0.0279*	-0.0441
	(0.0483)	(0.0483)	(0.0483)	(0.0143)	(0.0471)
Above median bet riskiness		-0.0344			
		(0.0504)			
Standardized bet riskiness			0.0129		
			(0.0247)		
R^2	0.06334	0.06455	0.06405	0.03116	0.07768
Observations	371	371	371	371	371

Table B.2: Individual-level predictors of overoptimistic predictions

Notes: The table shows how mispredictions, predictions, and realizations vary with demographics and pre-study bet activity data. It reports coefficient estimates and standard errors from multivariate regressions. Coefficients are in units of dollars per dollar wagered. All covariates are binary, except for "standarized bet riskiness," which is the standardized average bet riskiness for wagers placesd in periods $t = 1$ and $t = 2$. An observation is a member of the analysis sample who placed wagers both in the pre-study period ($t = -1, -2$) and in the study period ($t = 1, 2$).

	True consumption (as share of t=0 consumption) (1)	(2)
Constant	0.2263***	0.0506
	(0.0780)	(0.1298)
Control Prediction × Control	0.3173***	0.3480***
	(0.0436)	(0.0781)
Bonus Prediction × Bonus	0.2093***	0.1516***
	(0.0478)	(0.0575)
Control Prediction × Bonus		0.1462*
		(0.0848)
Bonus Prediction × Control		0.0758*
		(0.0444)
R^2	0.11373	0.12591
Observations	442	442

Table B.3: Self-reported proportional consumption changes predict observed consumption changes

Notes: The table shows how self-reported proportional consumption changes predict observed consumption changes. The table reports coefficient estimates and standard errors from multivariate regressions. Coefficients are in units of dollars per dollar wagered. An observation is a member of the analysis sample who placed wagers both in the pre-study period ($t = -1, -2$) and in the study period ($t = 1, 2$).

	Posterior (t=1) - Prior (t=1) (1)	(2)	Prediction (t=2) - Prior (t=1) (3)	Prior (t=1) - Realization (t=1) (4)
Constant	0.0062	-0.0030	-0.0147	0.0745***
	(0.0072)	(0.0075)	(0.0124)	(0.0168)
Signal		0.0894***	0.0649	-0.0393
		(0.0245)	(0.0406)	(0.0553)
Sample	Treated	Treated	Treated	Wager in t1
Observations	231	231	231	396
R^2		0.05493	0.01103	0.00128
Adjusted R^2		0.05080	0.00671	-0.00126

Table B.4: Information treatment effects

Notes: The table summarizes regression results about the impacts of the information treatment, using regression (**??**). For the first two columns, an observation is a participant in the information treatment, and the dependent variable is a measure of updating. In the first case, it is the difference between the survey 1 posterior prediction and the survey 1 prior prediction, while in the second case, it is the difference between the survey 2 prediction and the survey 1 prior. The signal is the difference between true past net returns and recollections of past net returns. The coefficients on the signal teach us about the magnitude of instantaneous and persistent updating respectively. For the third column, an observation is an agent who placed at least one wager in period 1, and the dependent variable is the prediction error. The coefficient on the signal tells us whether signals were correlated with prediction errors. Standard errors in parentheses. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Variable	Grubbs and Krauss: Census Matched	Weekly Lottery	Weekly Sports	Brown, Grasley, and Guido: Unweighted	Weighted
N	2806	406	517	444	444
Demographics					
Age	51.59	55.21	41.47	39.92	38.35
White	0.66	0.62	0.59	0.81	0.75
Male	0.46	0.53	0.69	0.96	0.92
Bachelor's degree or higher	0.34	0.25	0.50	0.82	0.55
Graduate degree	0.13	0.08	0.19	0.39	0.21
Household income ($000s)	68 (62)	67 (57)	101 (84)	156 (116)	111 (95)
Qualitative bias measures					
Gambling Literacy Index	4.00 (2.30)	3.12 (2.74)	1.53 (3.03)	3.55 (2.05)	1.73 (2.30)
Problem Gambling Severity Index	0.99 (2.69)	2.83 (4.21)	6.77 (5.06)	2.89 (2.85)	6.15 (3.97)

Table B.5: Weighted sample characteristics

Notes: The table compares various subsamples of a nationally representative survey conducted by Grubbs and Kraus (2022) with our experimental sample. The first four columns are as in Table 2. Standard deviations are in parentheses. The fifth column displays statistics for our weighted sample. The sample weights are initially calculated to make the analysis sample match the weekly sports bettor sample on education vairables and qualitative bias measures. The weights are then truncated at [1/10, 10] to retain precision.

	Dollars Wagered (1)	Any Wagers (2)	Number of Bets (3)	Average Bet Size (4)
t=1 × Bonus	-0.3370***	-0.1085***	-0.2926***	-0.0630
	(0.0657)	(0.0330)	(0.0760)	(0.0508)
Bonus × t=2	-0.1429	-0.0021	-0.0510	0.0786
	(0.1306)	(0.0324)	(0.0974)	(0.1391)
Sample	Bonus binding	Bonus binding	Bonus binding	Bonus binding & wagered both periods
Normalized Dependent Variable?	Yes	No	Yes	Yes
Control mean	1	0.91	1	1
R^2	0.04521	0.09339	0.04818	0.03237
Observations	828	828	828	686

Table B.6: Bet Less Bonus treatment effects

Notes: We report regression results from specification defined in (B.8). An observation is an individual-period for $t = 1, 2$. As described in Section 6.3, we drop treated participants whose consumption exceeded the bonus benchmark, and we drop the analagous top 6% of participants in the control group. For the third column, we also restrict to participants who placed at least one bet in both periods. The dependent variables are defined as the proportional change between the period-zero outcome and the period t outcome, normalized so that the control group dependent variable mean always equals one in both periods. Standard errors in parentheses. We cluster by referral group as pre-specified. $^+p < 0.1$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

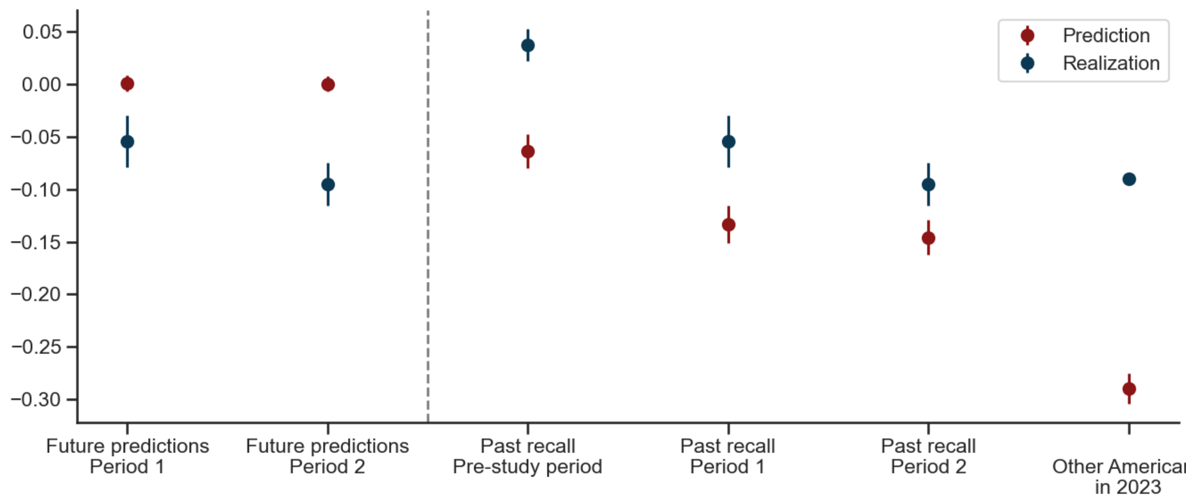

Figure B.1: Mispredicted financial returns in the future and the past

Notes: The figure illustrates mean predicted net returns and realized net returns across participants for various periods. The points to the left of the dotted line represent forward-looking prediction, and the points to the right of the line represent backward-looking recollections. The "Pre-study period" is the period from January 1, 2024 until April 8, 2024 (the day the study began). Future predictions about periods 1 and 2 took place in surveys 1 and 2 respectively. Recollections about the pre-study period, period 1, and period 2 took place during surveys 1, 2, and 3 respectively. The question about other Americans took place in survey 1. The ground truth (-0.09) for that question comes from American Gaming Association (2024). Future predictions and realized returns are truncated to lie in $[-0.25, 0.4]$; returns for questions are not truncated. Error bars represent 95% confidence intervals.

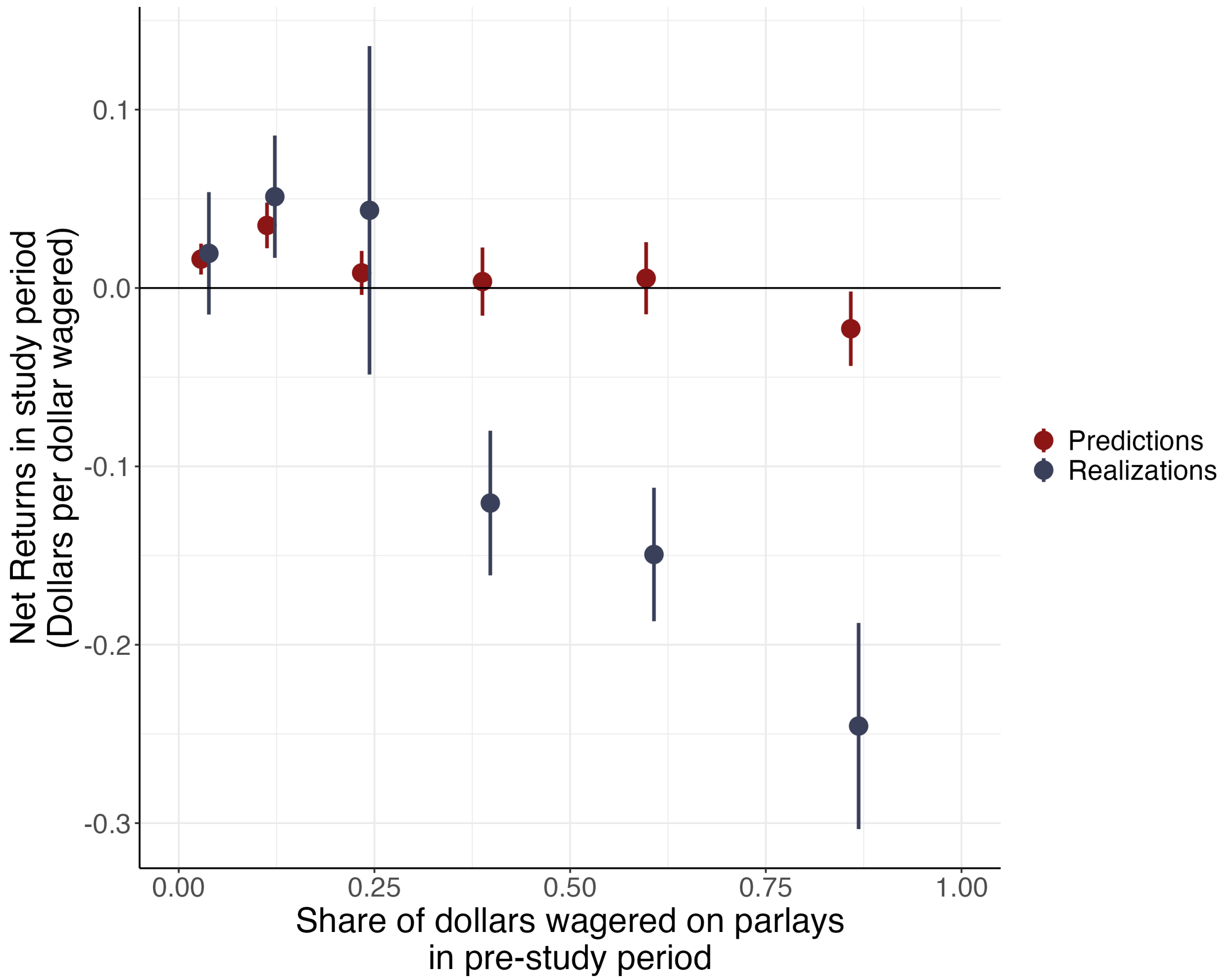

Figure B.2: Predicted and realized net returns by pre-study parlay share

Notes: The figure shows the distribution of predicted and realized net returns vary with the a measure of parlay intensity. This measure, on the x-axis, is the share of dollars wagered on parlays across pre-study periods -1 and -2. Points are means and error bars are 95% confidence intervals.

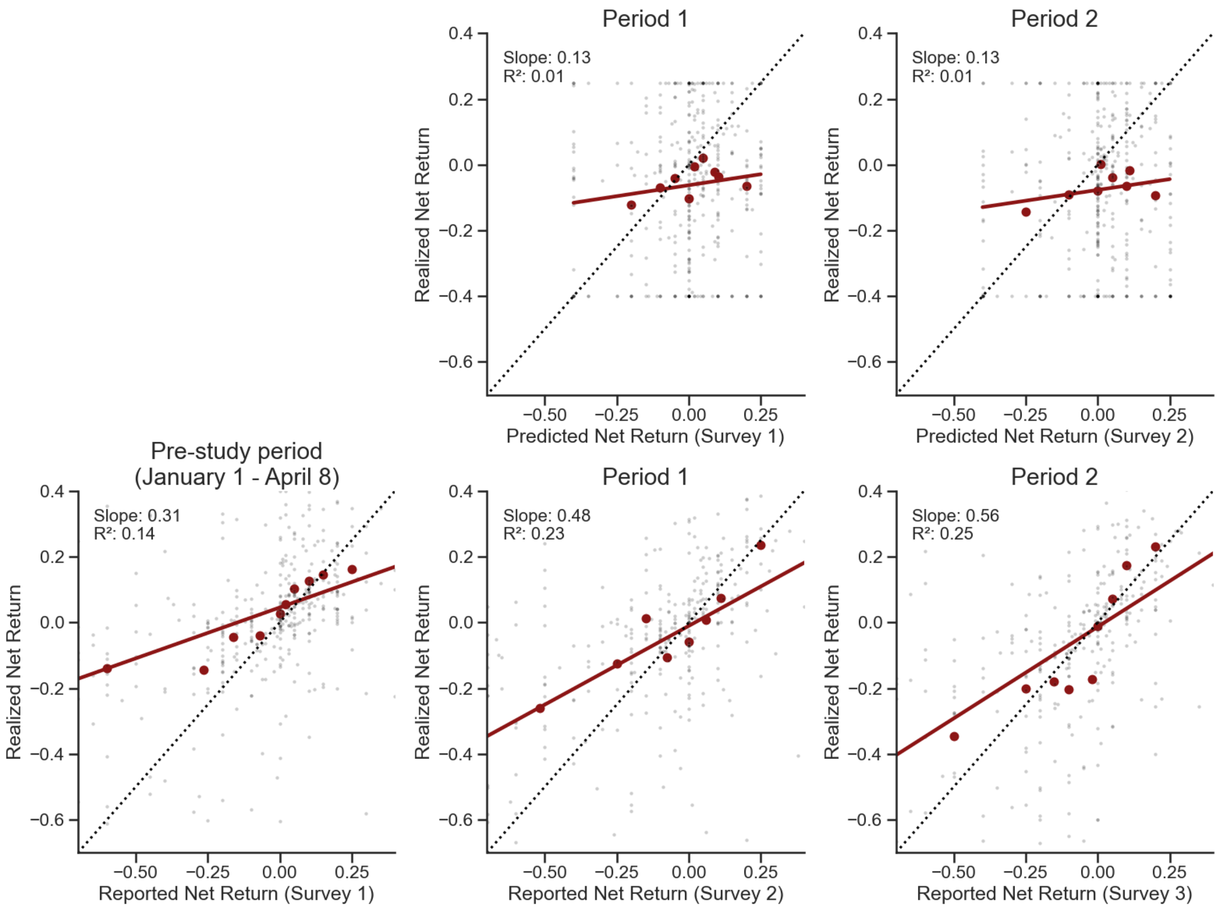

Figure B.3: Correlation between predicted and true financial returns in the future and past

Notes: The figure illustrates the correlation between reported and true financial returns across people within-period. The top row shows the correlation for predictions about the future, the bottom row shows them for recollections about the past. Future predicted and realized returns are truncated to lie in $[-0.25, 0.4]$; past recalled and realized returns are truncated to lie in $[-1, 1]$. The line of best fit is from a univariate regression. We report the slope and R^2.

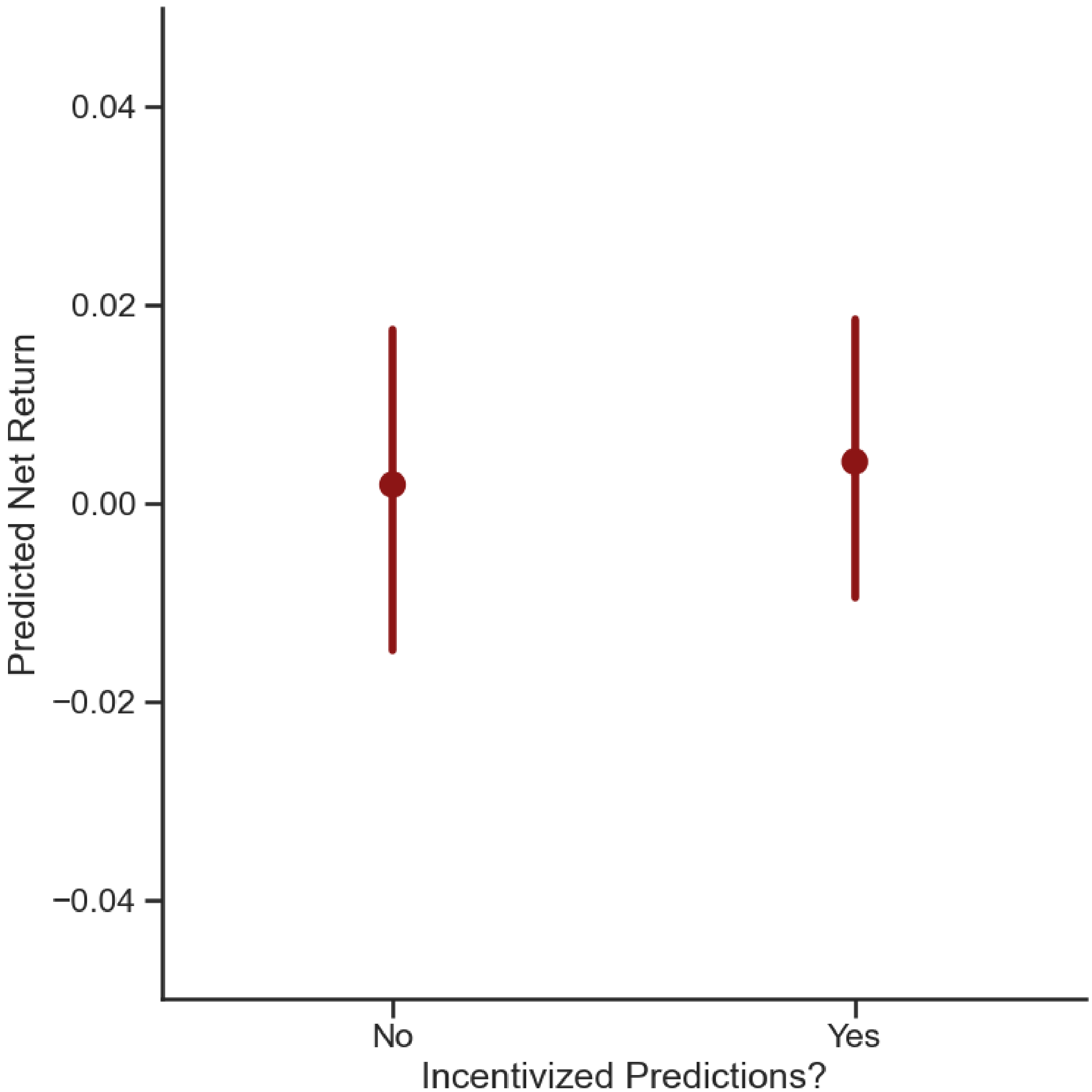

Figure B.4: Impact of incentivization on predictions of financial returns

Notes: The figure plots the means of predicted net returns by whether predictions were incentivized. Error bars represent 95% confidence intervals. Predictions were incentivized for 65% of participants.

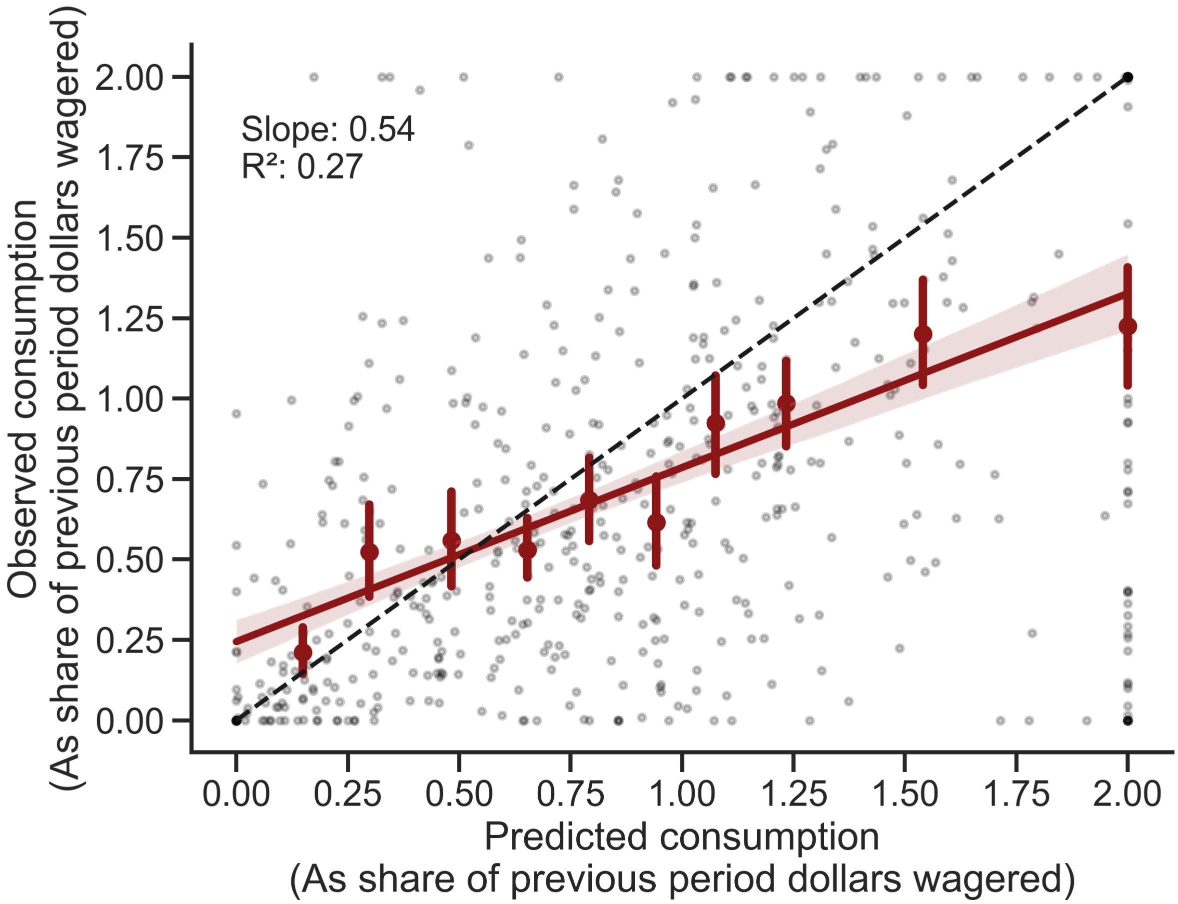

Figure B.5: Correlation between predicted and true future dollars wagered

Notes: The figure plots predicted consumption against observed consumption for the bonus control group, pooling across periods 1 and 2. The units proportional changes relative to consumption in the previous period. Predicted and observed changes are both truncated at 2, which corresponds to doubling dollars wagered.

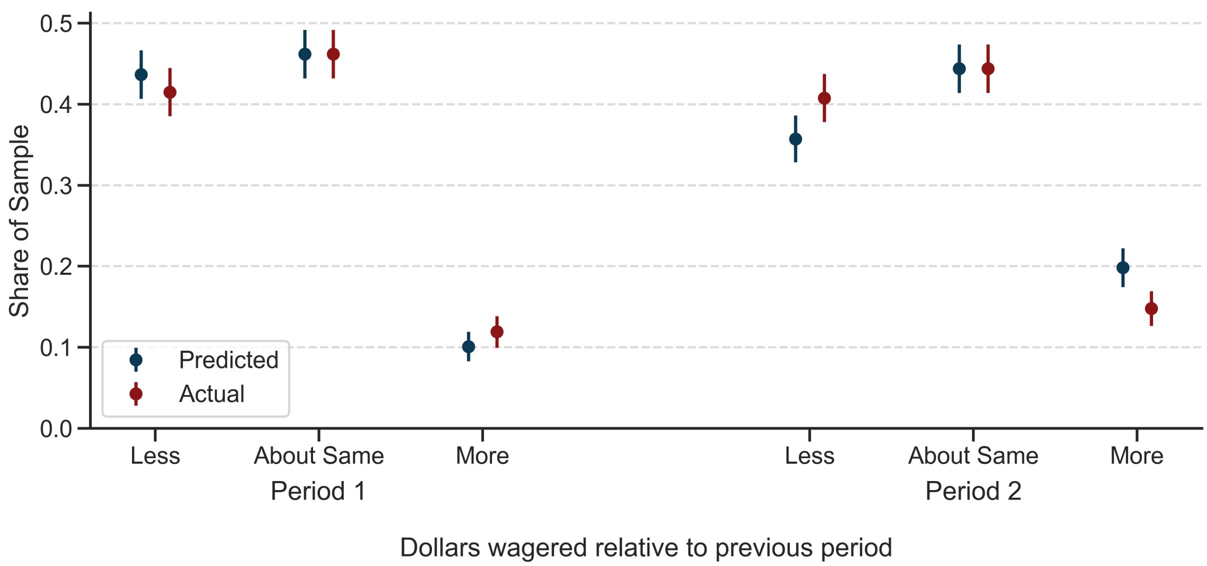

Figure B.6: Evidence against naivete from qualitative predictions

Notes: The blue series is the share of participants in each survey giving the designated response to the following tertiary prediction question: "Overall, in the next thirty days, do you think you will wager more or less than you've been wagering recently?" The red series partitions observed wager volumes into three bins, depending on their relationship to the previous period wager volume, given a bandwidth b. "Less" corresponds to wagering less than $1/b$ times the previous period ($x_t < \frac{1}{b}x_{t-1}$). "More" corresponds to wagering more than b times the previous period ($x_t > bx_{t-1}$). "About the same" corresponds to the cases in between. We choose the bandwidths so that the observed "about the same" share is equal to the survey-measured about "about the same" share. Error bars represent 95% confidence intervals.

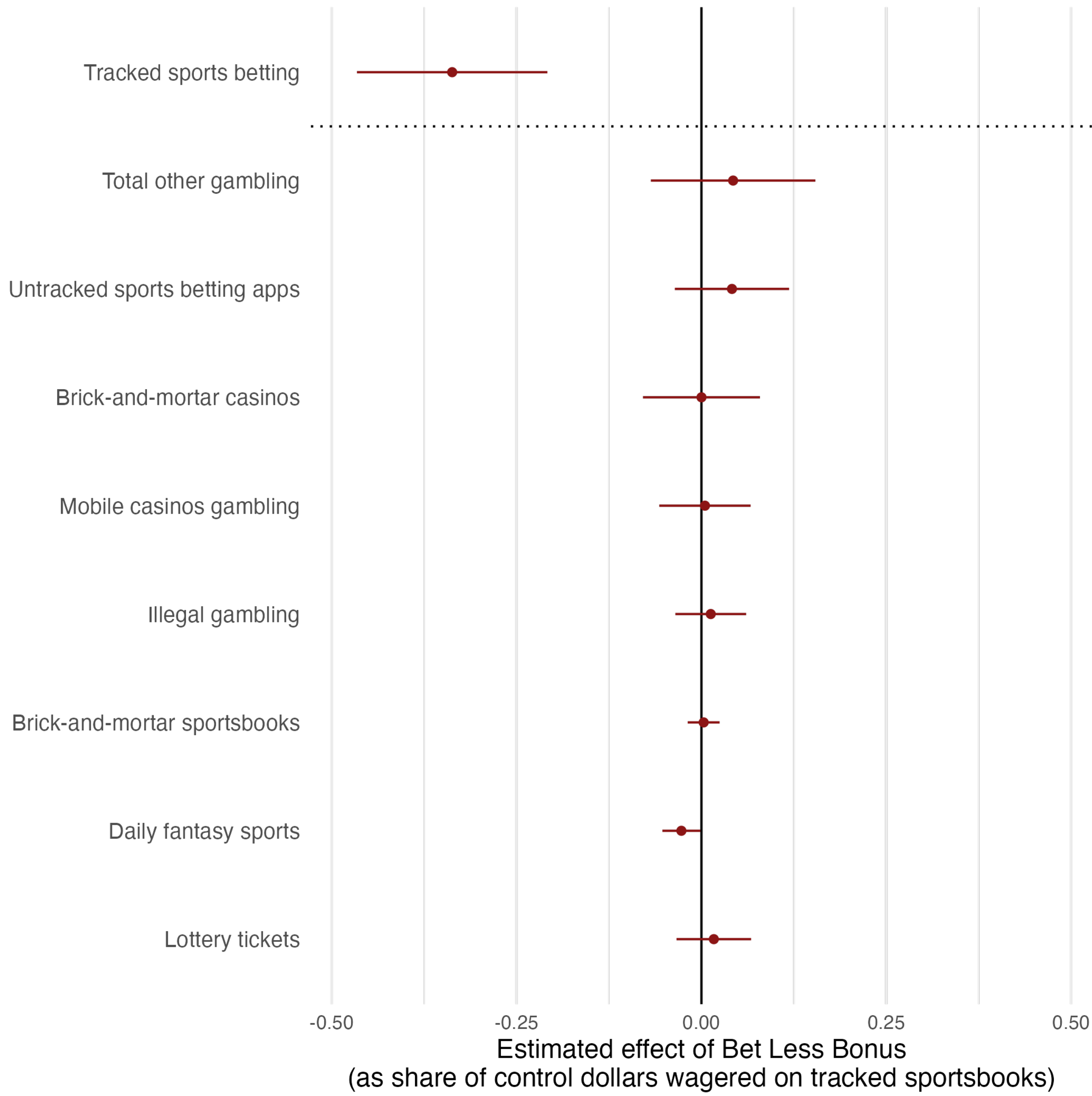

Figure B.7: Effect of Bet Less Bonus on self-reported untracked gambling

Notes: The figure presents coefficients from bonus treatment effect regressions as specified in equation B.8. For tracked sports betting, the dependent variable is normalized as described in that section. For untracked gabmling, the dependent variable is normalized by the same factor that the tracked sports betting variable. Therefore, all coefficients are interpreted on a common scale as changes proportional to the magnitude of tracked sports betting consumption in the control condition. The normalized dependent variable ratios are truncated above at 2 to improve precision. Error bars represent 95% confidence intervals.

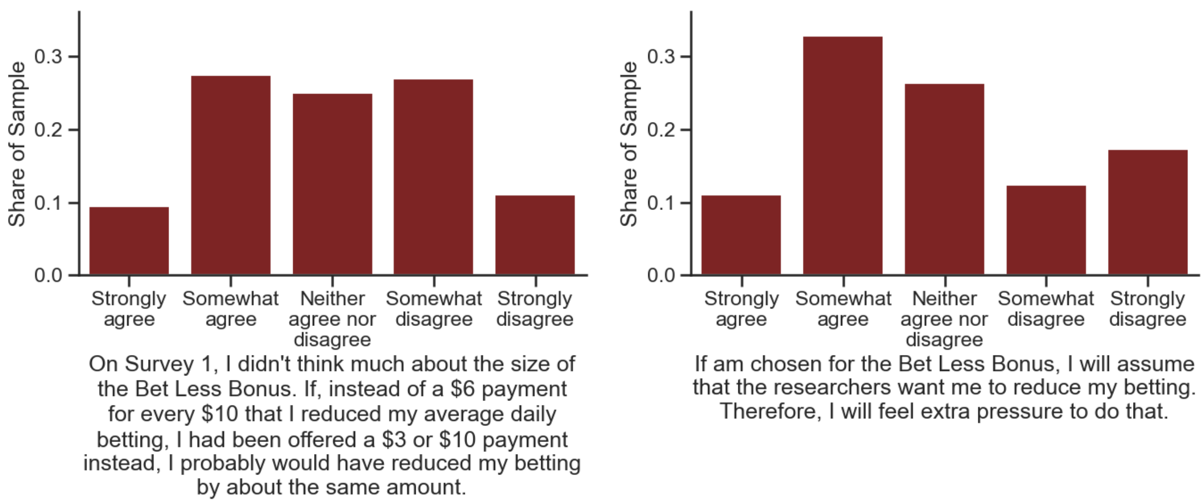

Figure B.8: Qualitative evidence on how participants interpreted the bonus

Notes: The figure shows the shre of people giving each response to the likert scale questions about the Bet Less Bonus. Both questions were asked on survey 3.

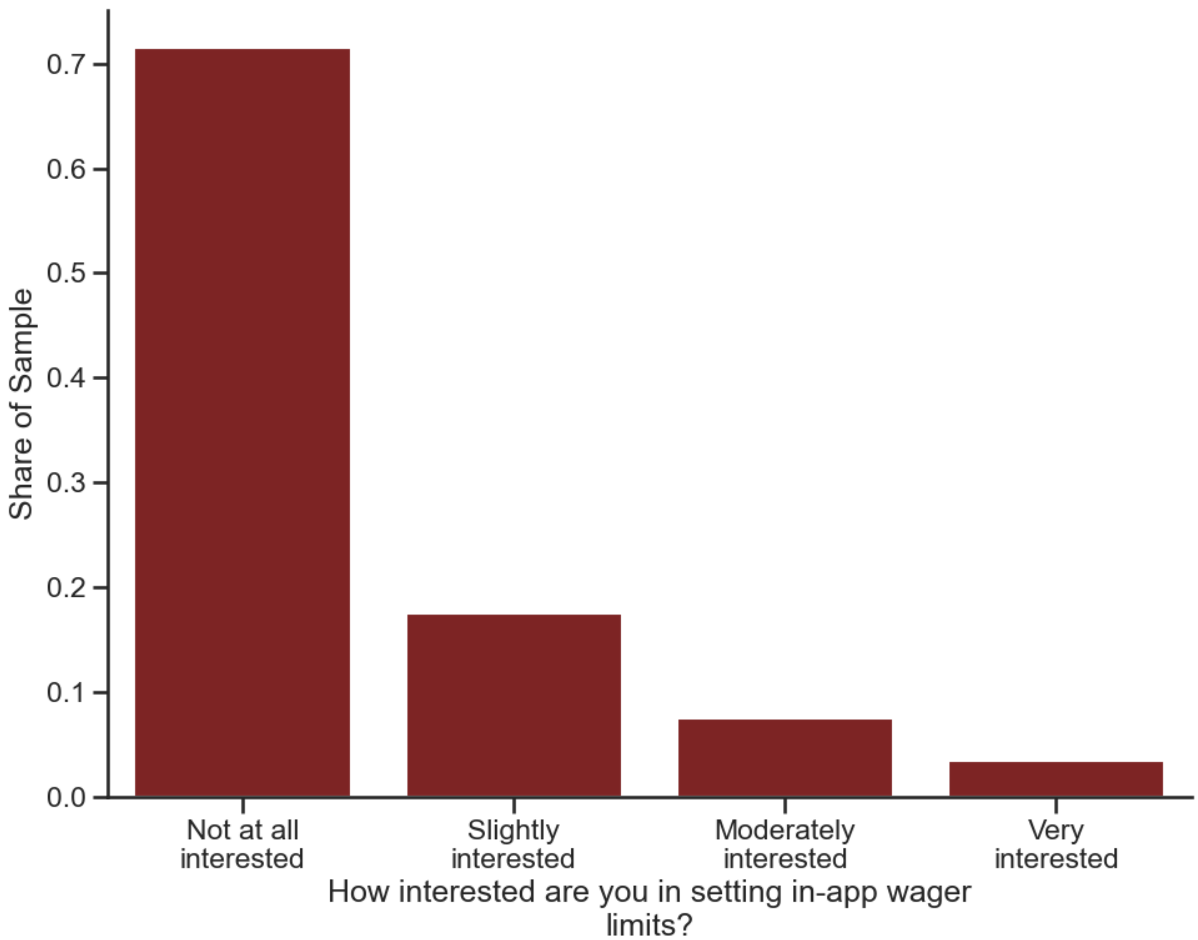

Figure B.9: Qualitative interest in limits

Notes: The figure reports the frequency of responses to the question, "How interested are you in setting in-app wager limits?" We asked this question before participants set their limits. We restrict to the subsample of bettors in the limits treatment who did not have difficulty finding limit screens for all apps (N=200).

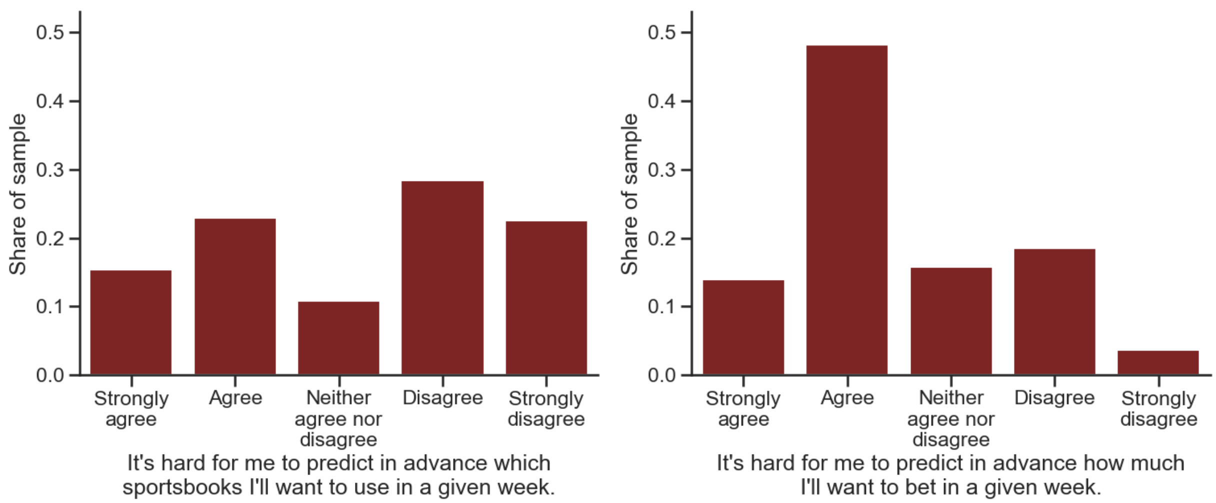

Figure B.10: Qualitative evidence on uncertainty about future demand

Notes: The figure shows the shre of people giving each response to the likert scale questions about uncertainty about the future. Both questions were asked on survey 3.

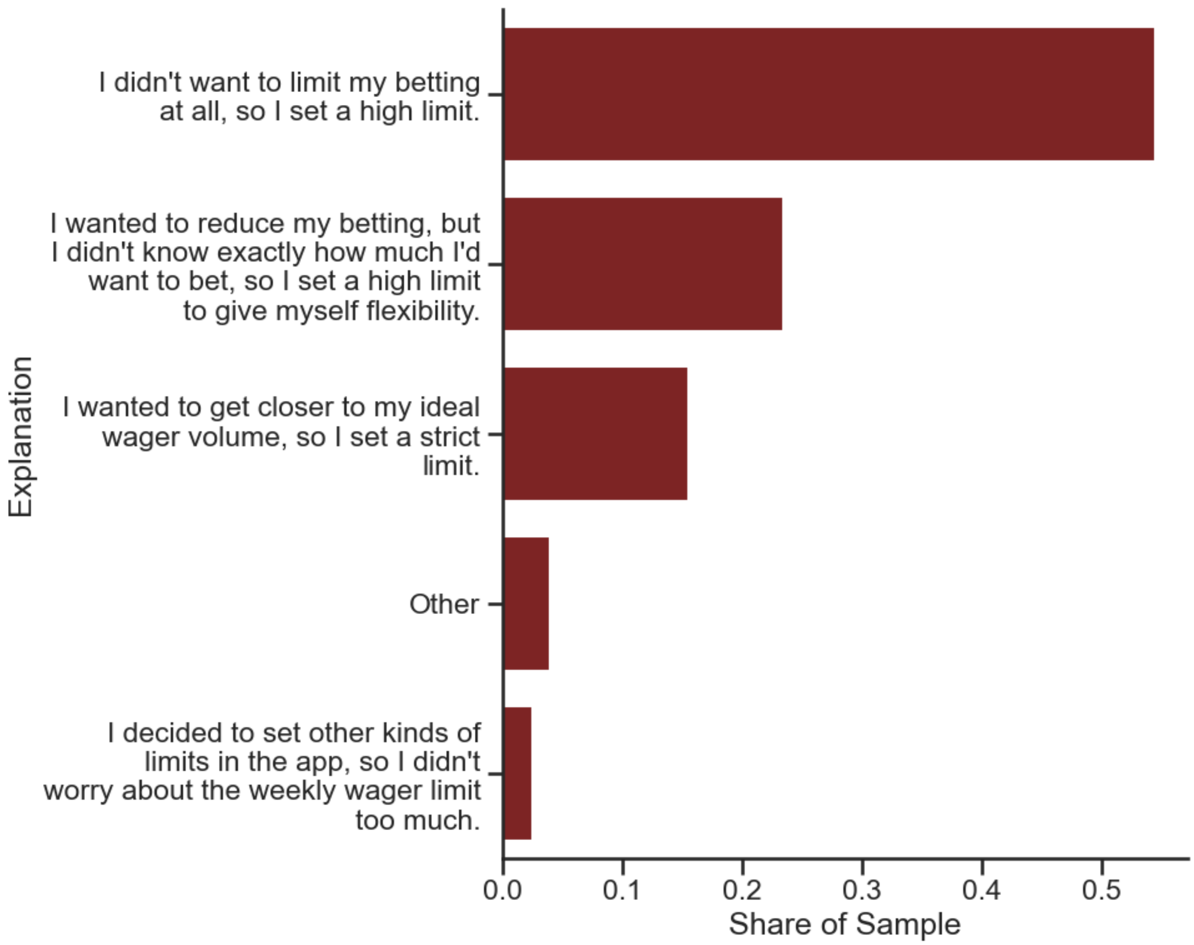

Figure B.11: Qualitative explanations of limit choices

Notes: The figure reports the frequency of resposnes to the question, "Please select the statement that best describes your thinking when choosing the weekly wager limit." We asked this question after participants set their limits. We restrict to the subsample of bettors in the limits treatment who did not have difficulty finding limit screens for all apps (N=200).

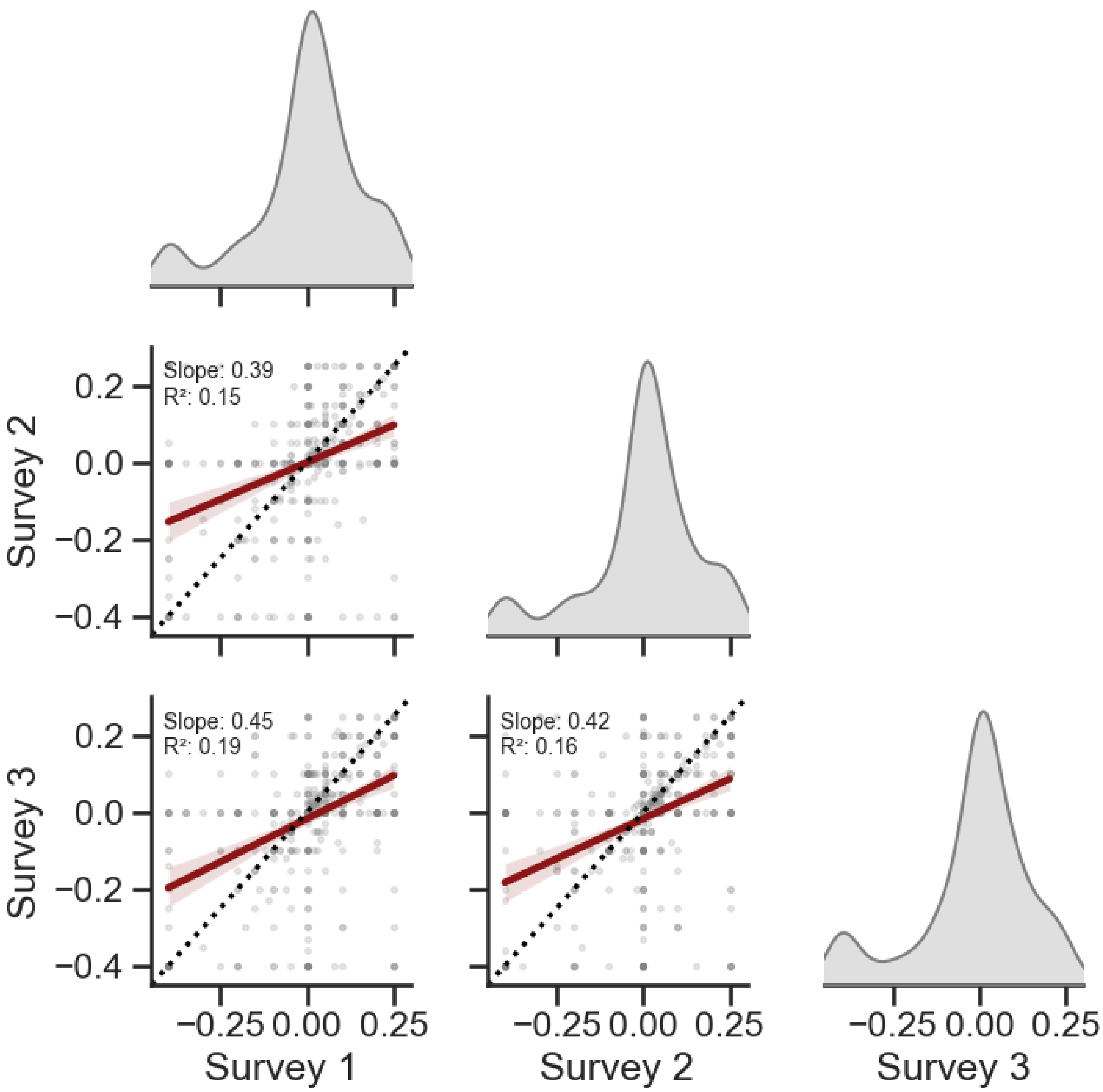

Figure B.12: Correlation of predicted net returns within individual over time

Notes: The figure illustrates the correlation of predicted future net returns across different surveys. A point is a participant in the anlysis sample. Predictions are truncated to lie in $[-0.25, 0.4]$. Diagonal plots are desities. The line of best fit is from a univariate regression. We report the slope and R^2.

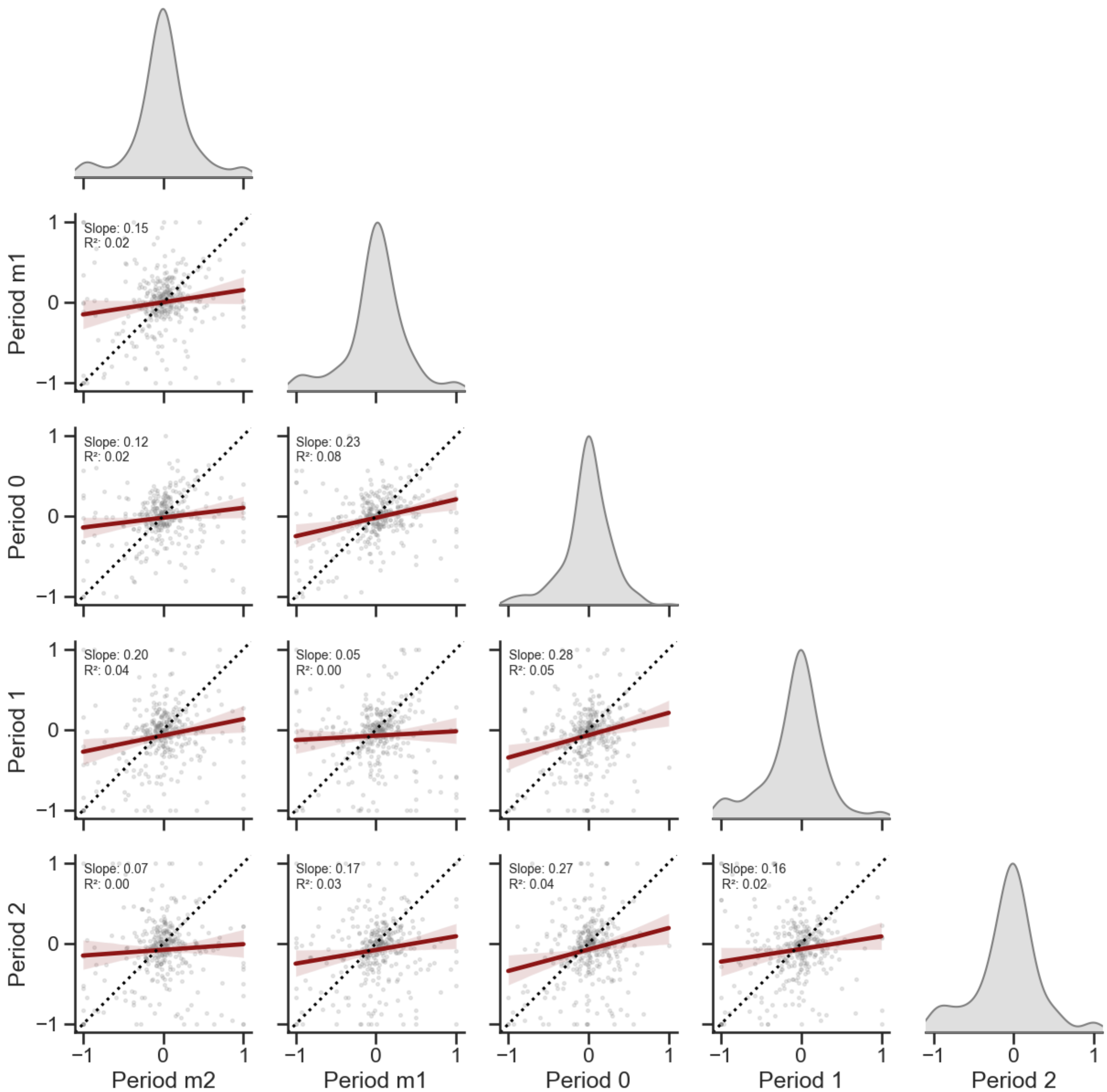

Figure B.13: Correlation of realized returns within individual over time

Notes: The figure illustrates the correlation of realized net returns across periods. A point is a participant in the anlysis sample. Periods 1 and 2 are the 30-day periods during the study; periods 0, -1, and -2 are 30-day periods before the study. Returns are truncated to lie in $[-1, 1]$. Diagonal plots are desities. The line of best fit is from a univariate regression with raw (untruncated) realization data. We report the slope and R^2.

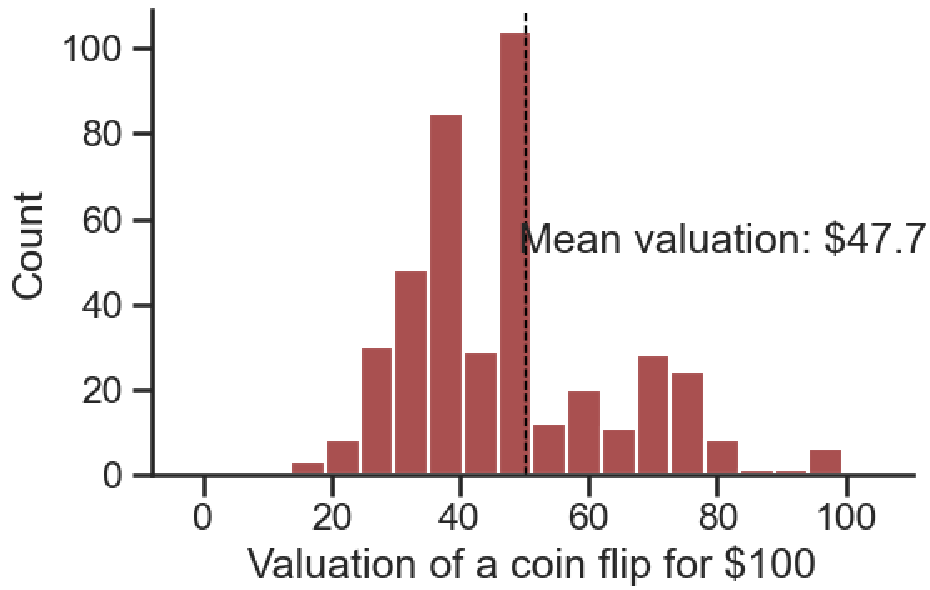

Figure B.14: Valuations of a $100 coin toss

Notes: The figure illustrates the elicited valuations of a 50% chance to win $100. The elicitations were incentivized as described in Section A.4. A $50 valuation would correspond to risk-neutrality over experimental earnings, negative valuations to risk aversion, and positive numbers to risk-loving.

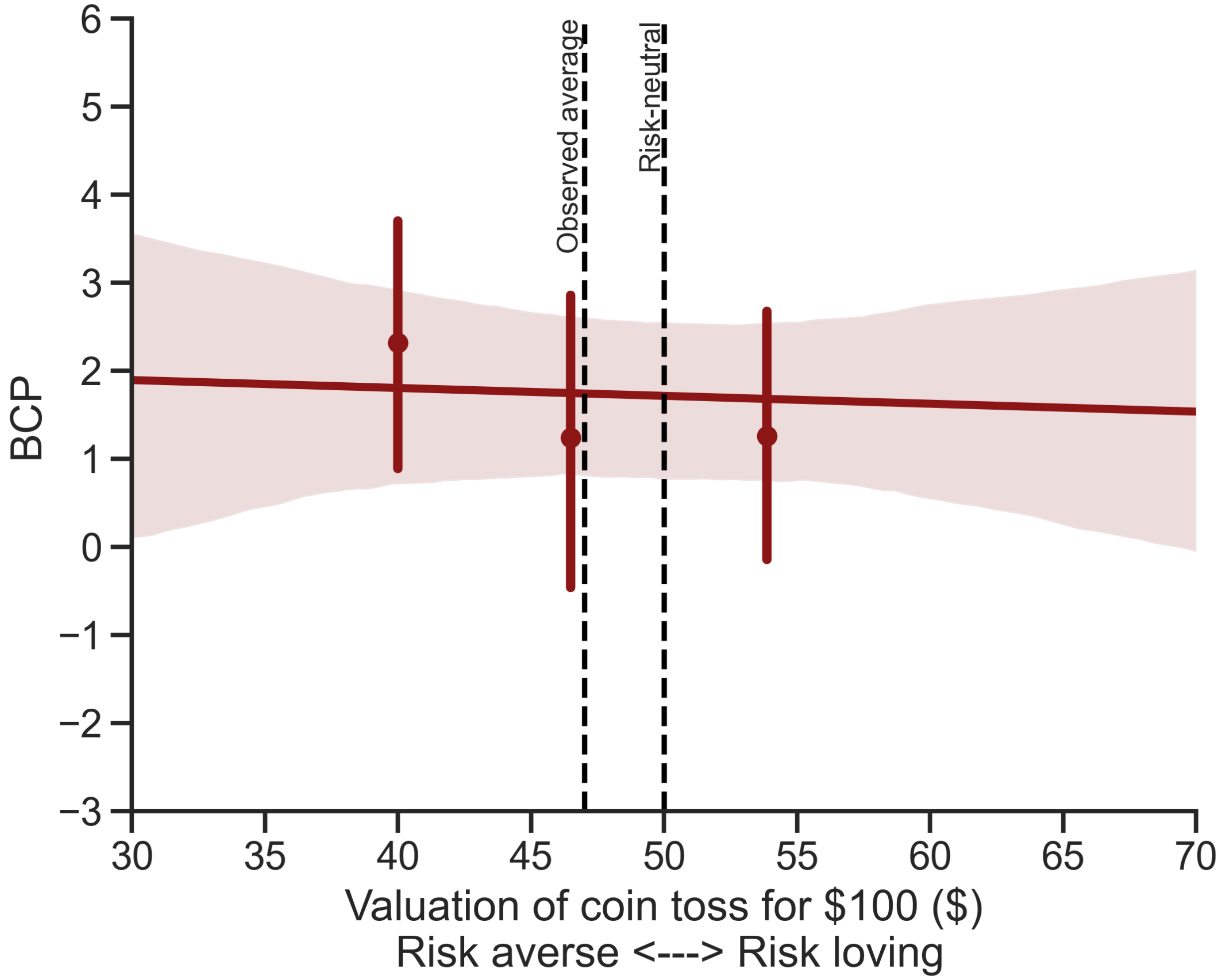

Figure B.15: Correlation between behavior change premia and risk aversion

Notes: The figure shows how the average behavior change premium correlates with our measure of risk aversion over experimental earnings. Our risk aversion measure is the valuation of a 50% chance to win a $100 bonus ("coin toss for $100"). The vertical lines correspond to the average observed valuation and the valuation that would imply risk-neutrality.

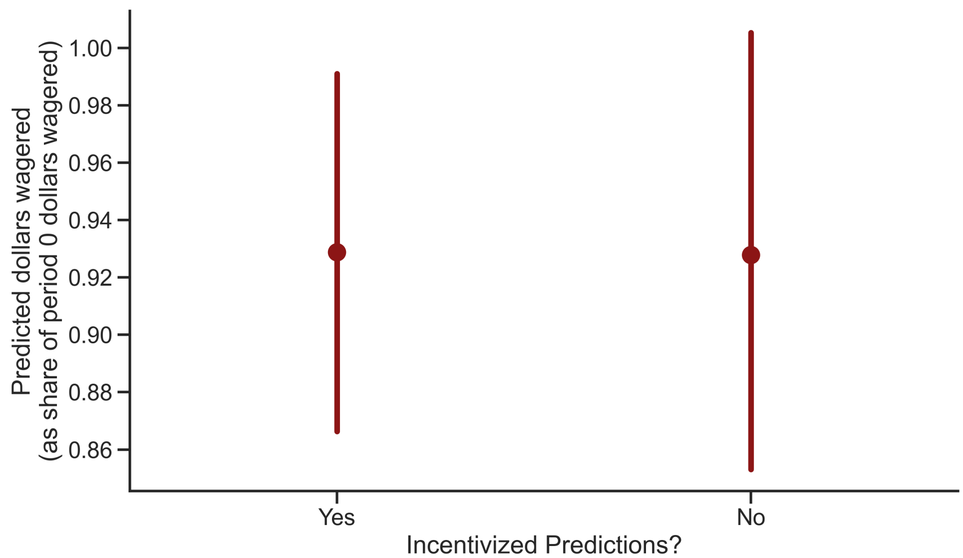

Figure B.16: Impact of incentivization on predictions of dollars wagered

Notes: The figure plots the means of predicted dollars wagered by whether predictions were incentivized. We restrict to the sample of participants in the Bonus control condition as in Figure 8. We predictions across surveys 1 and 2. Error bars represent 95% confidence intervals. Predictions were incentivized for 65% of participants.

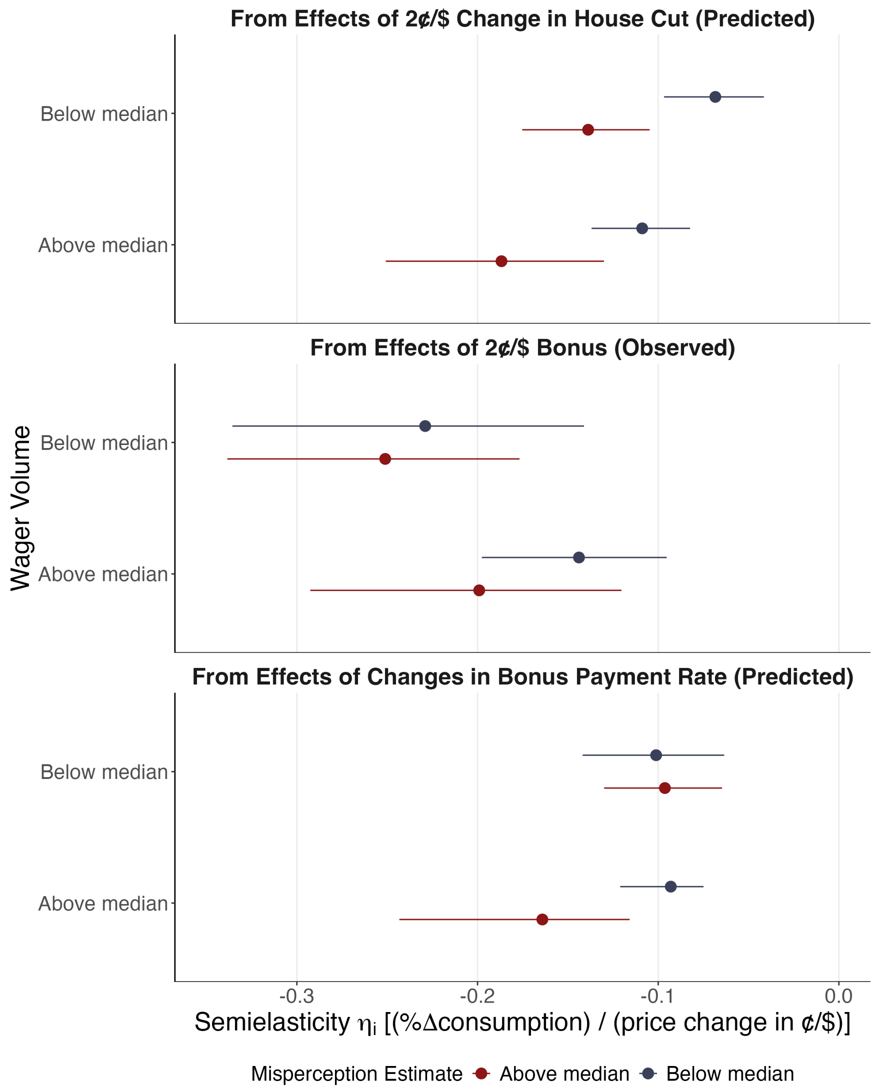

Figure B.17: Alternative Semielasticity Estimates

Notes: The figure shows semielasticity estimates for three alternative identifications strategies. We compute separate semielasticity estimates for four groups in each panel, below/above median wager volume and below/above median (shrunk) misperception estimate. The first panel shows semielasticities estimated from the predicted effect of a hypothetical 2¢ change in the house cut. These are the estimates we use in the main paper (and in Figure B.18). The second panel shows semielasticities estimated from the predicted effects of the Bet Less Bonus on survey 1. The third panel shows semielasticities estimated from a changes in the bonus payment rate. Error bars represent 95% confidence intervals, analytical for the first two panels and bootstrapped for the third.

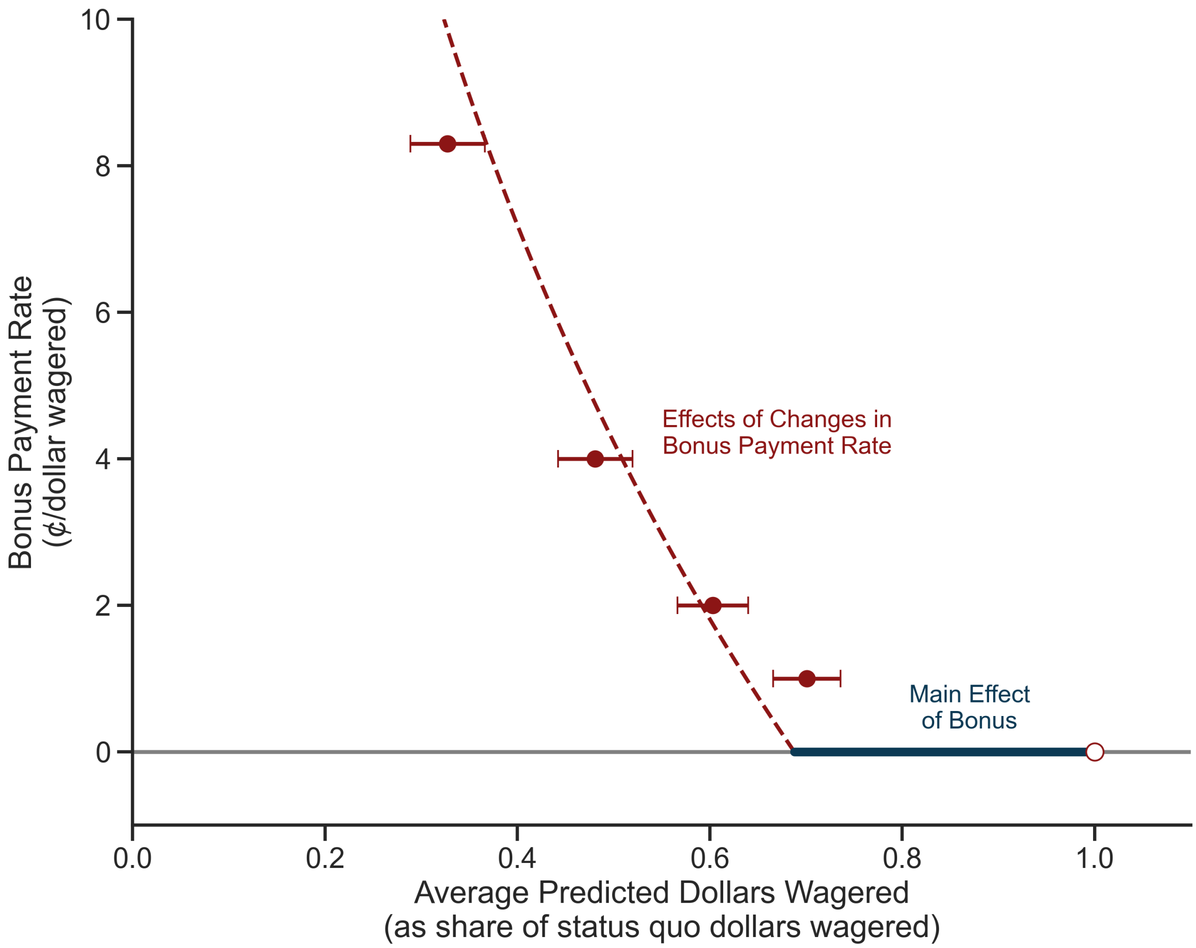

Figure B.18: Effects of varying the Bonus payment rate on predicted consumption

Notes: The figure shows the average predicted consumption as a share of control consumption for Bet Less Bonuses with varying payment rates. Error bars represent 95% confidence intervals. We asked this question to all participants in survey 3. The dotted line is a constant semielasticity fit through the aggreagated shares. We drop participants who predict that they will wager $0 in the control condition, since the proportional effect of a price change on consumption is undefined for them. We truncate predictions from above at 1, to ensure that there are no upward sloping demand curves.

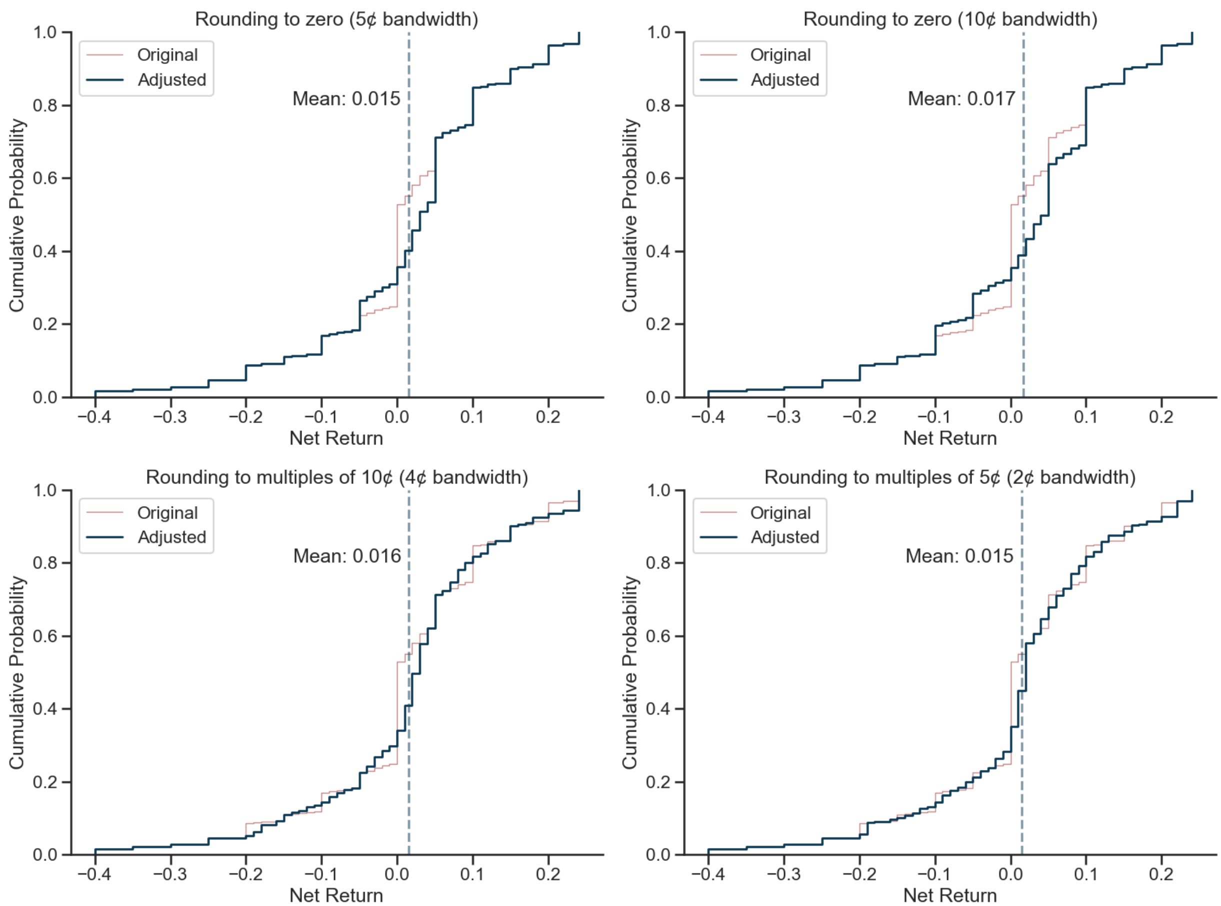

Figure B.19: Rounding-adjusted belief distributions

Notes: These figures present the rounding-adjusted belief distributions as described in Section B.1

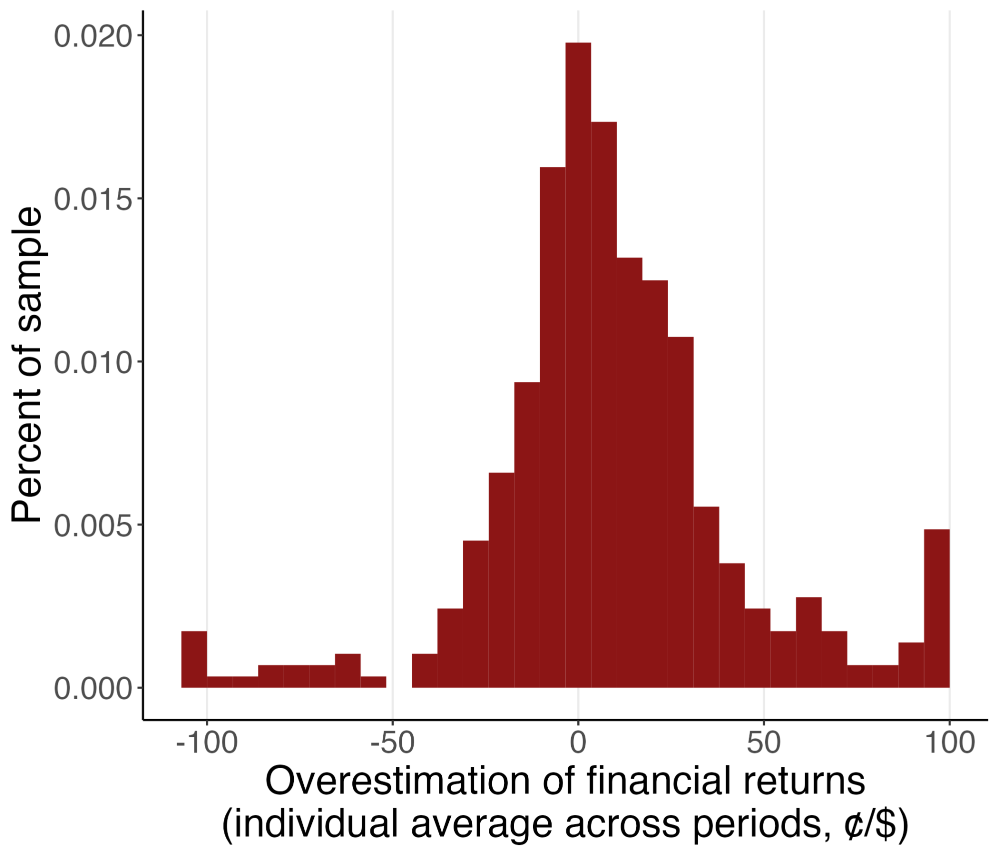

Figure B.20: Distribution of raw prediction errors about future returns

Notes: The figure is a histogram of the difference between predicted net returns and realized net returns. An observation is a participant. In cases where the participant placed at least one wager in periods 1 and 2, we average their prediction error across those periods.

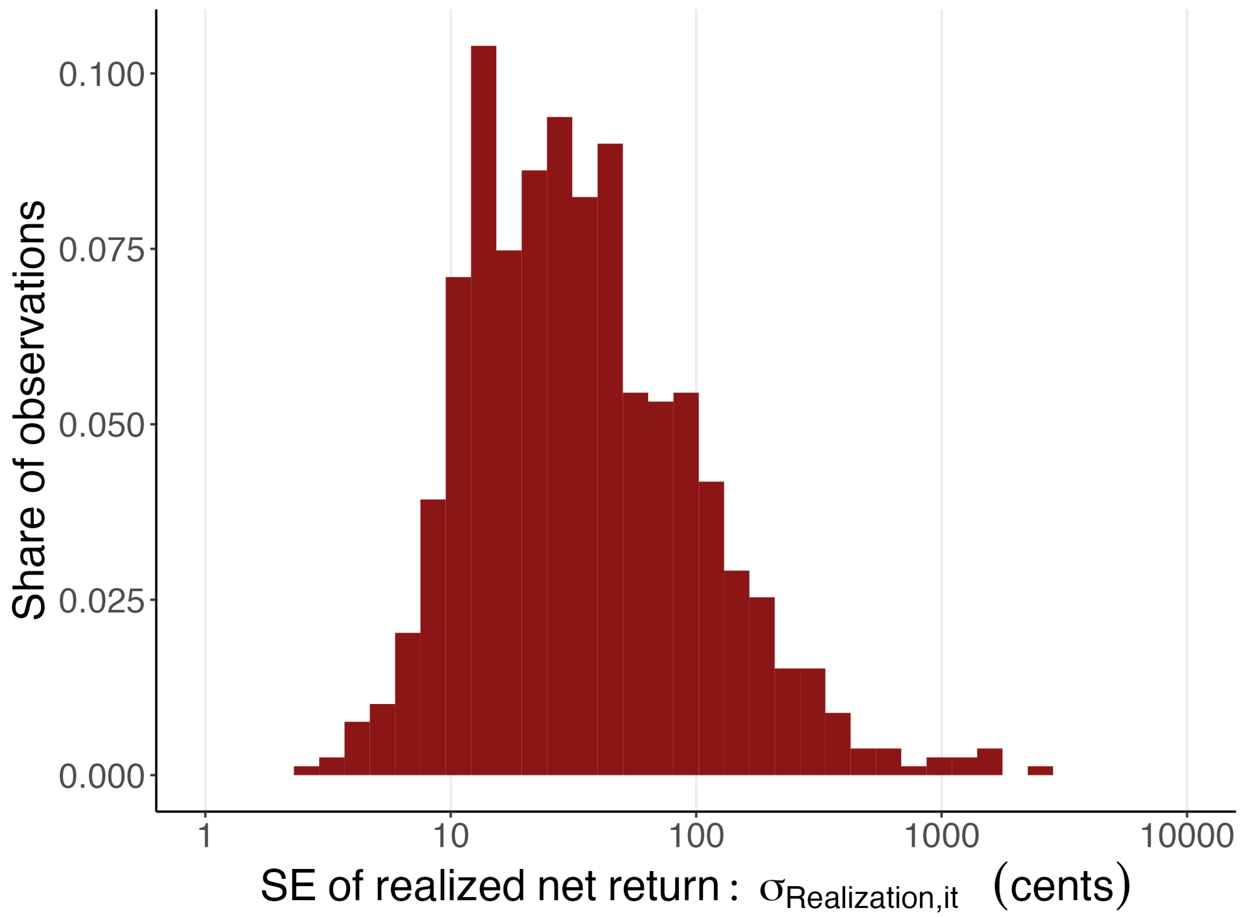

Figure B.21: Distribution of individual-period net return standard errors

Notes: The figure illustrates the distribution of standard errors of net returns, using a sample of all individual-period pairs in the study period where a wager was placed. The standard errors are computed from bet micro-data via the procedure described in Section B.4.

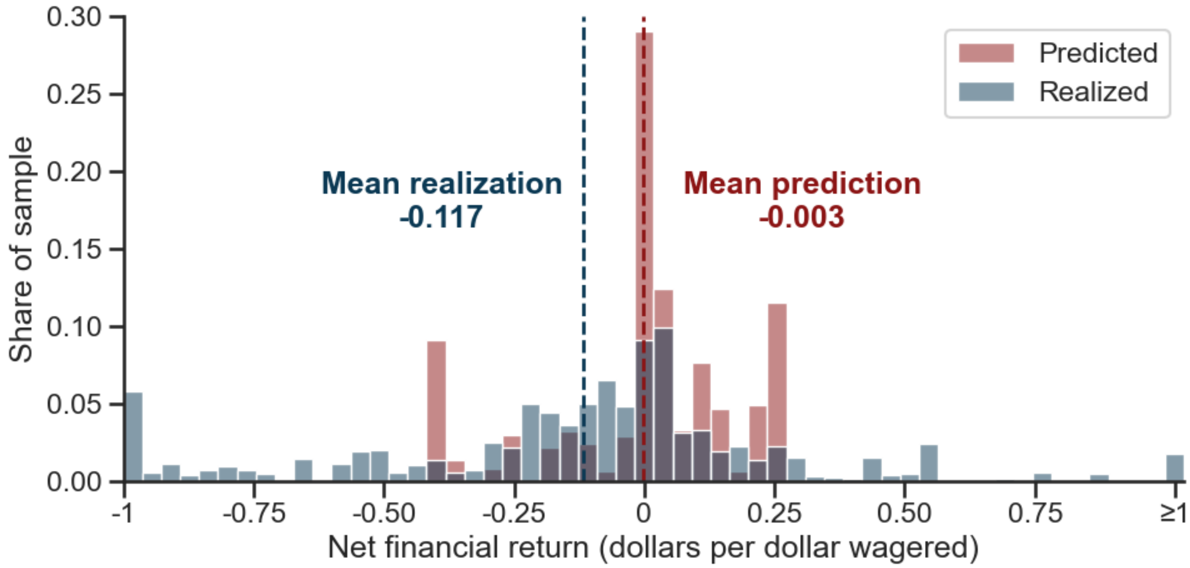

Figure B.22: Comparing predicted financial returns to realized financial returns (weighted sample)

Notes: This figure is the weighted-sample analog of Figure 5. Predictions are censored to lie within $[-0.4, 0.25]$. Dotted lines and annotations represent averages. Weights are initially computed to match a representative sample of weekly sports bettors on education and qualitative bias measures. We then truncate the weights at [1/10, 10] to retain precision.

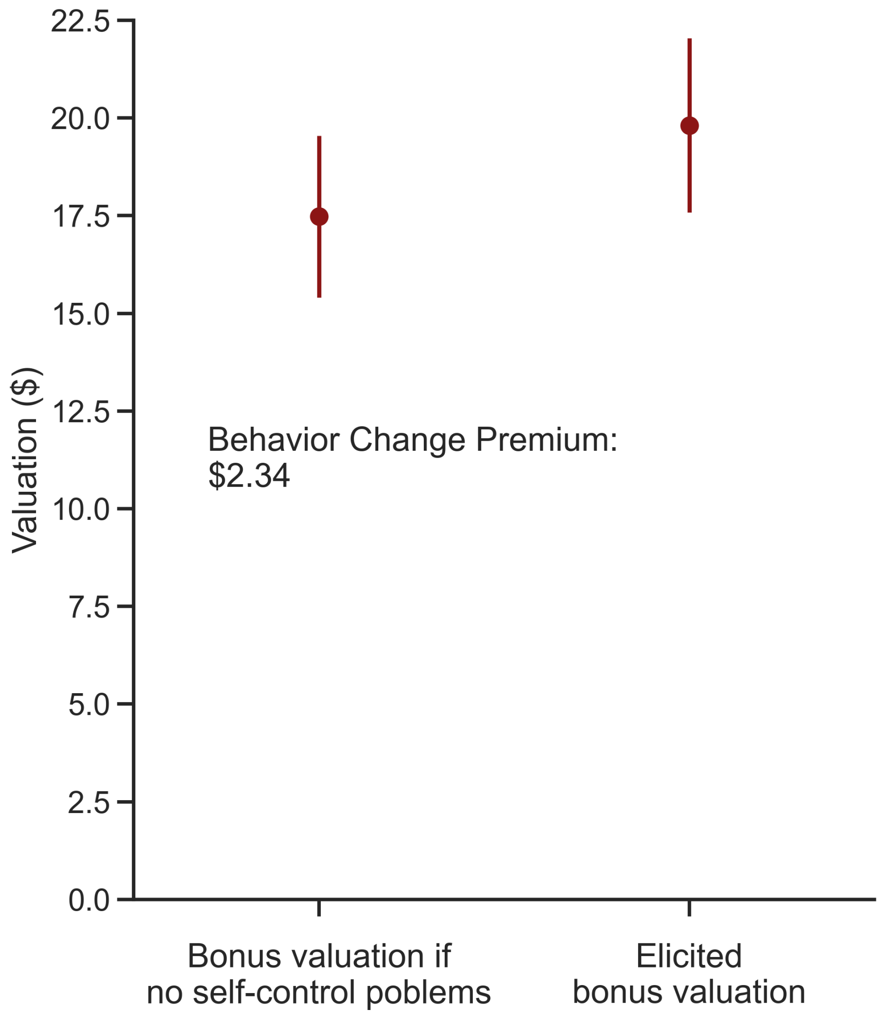

Figure B.23: The Behavior Change Premium (weighted sample)

Notes: This figure is the weighted-sample analog of Panel A of Figure 7. It plots the average valuations that time-consistent participants would have placed on the Bet Less Bonus as well as the average observed valuations. The difference between the observed valuations and the time-consistent valuations is the behavior change premium, which is our measure of perceived self-control problems. Weights are initially computed to match a representative sample of weekly sports bettors on education and qualitative bias measures. We then truncate the weights at [1/10, 10] to retain precision.

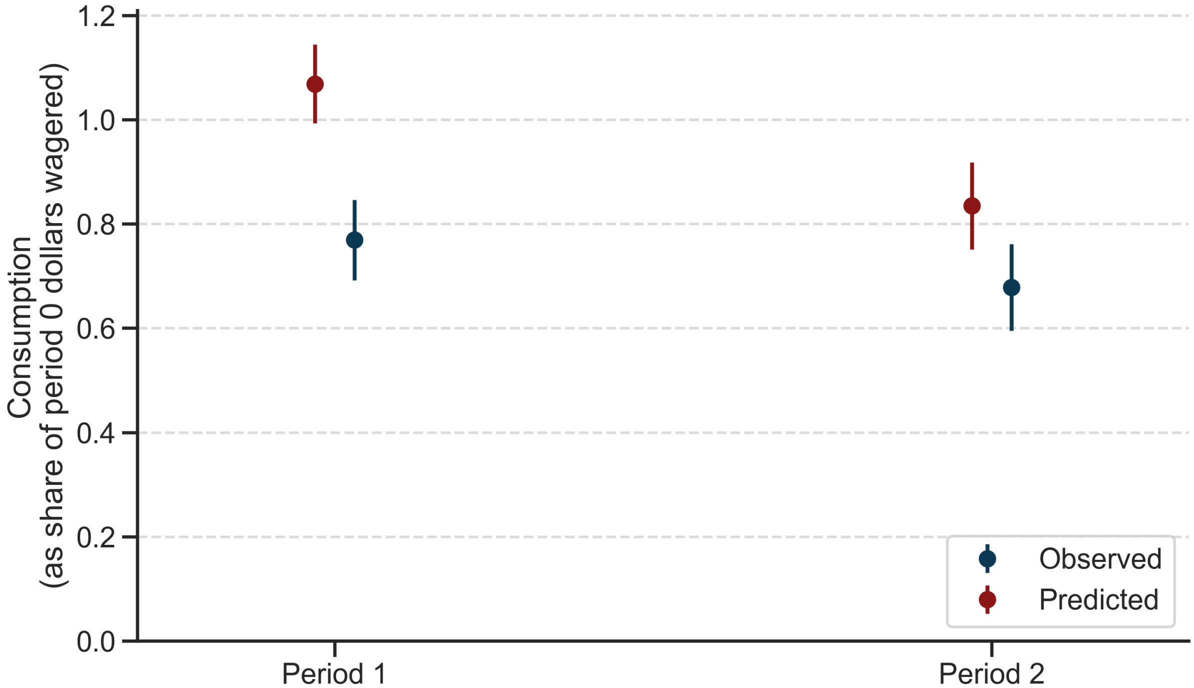

Figure B.24: Evidence against naivete from predictions of future dollars wagered (weighted sample)

Notes: This figure is the weighted-sample analog of Panel A Of Figure 8. We restrict to observations in the bonus control condition. Points represent mean wager volume, as a share of period 0 dollars wagered. Error bars represent 95% confidence intervals. Both the observed and predicted variables are truncated to lie in $[0, 2]$. Weights are initially computed to match a representative sample of weekly sports bettors on education and qualitative bias measures. We then truncate the weights at [1/10, 10] to retain precision.

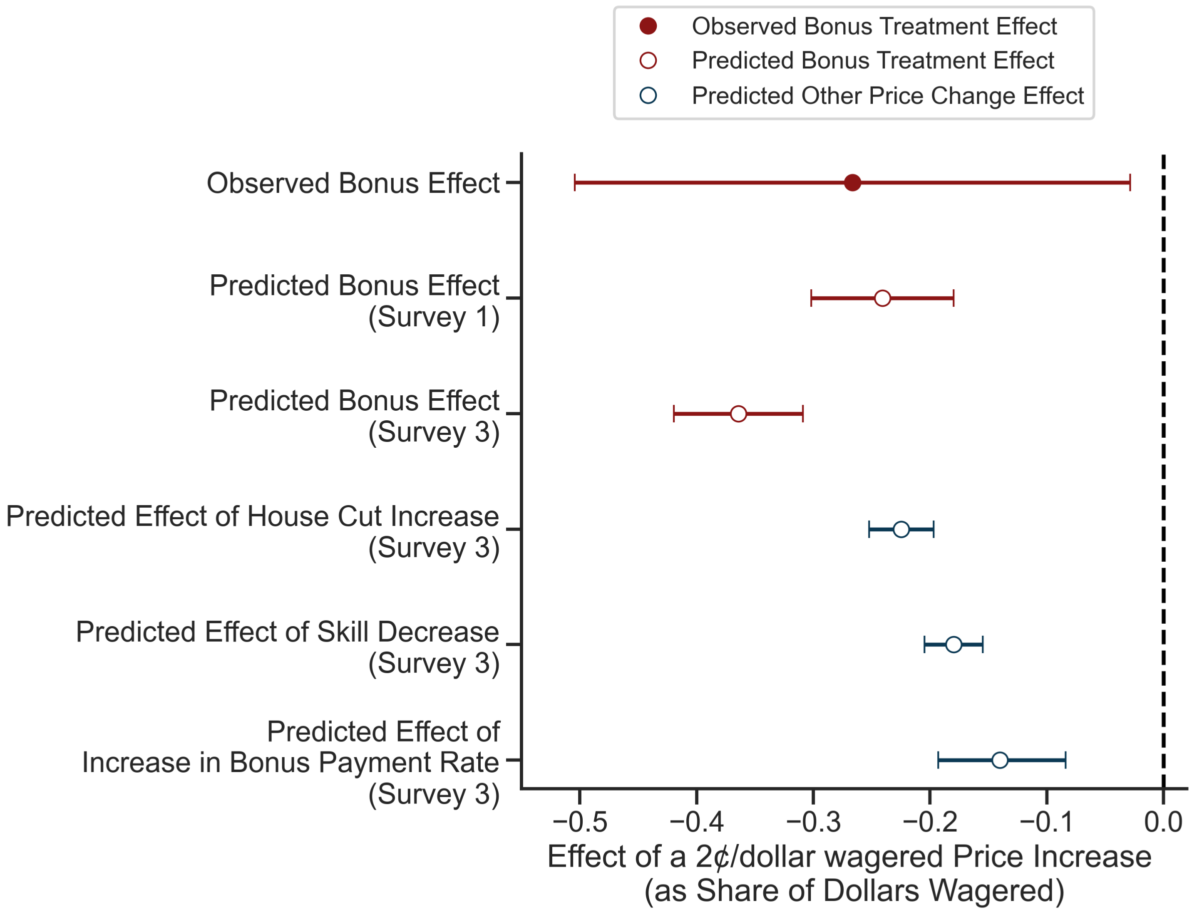

Figure B.25: Predicted effects of different kinds of price changes (weighted sample)

Notes: This figure is the weighted-sample analog of Figure 9. See the notes that Figure and Section 6.3 for details on how each effect is defined. Weights are initially computed to match a representative sample of weekly sports bettors on education and qualitative bias measures. We then truncate the weights at [1/10, 10] to retain precision.

C Model-based Analysis Appendix

C.1 Extension to nonlinear numeraire consumption utility

We consider in this section an extension of our biased betting model that relaxes the quasiinear utility assumption from Section 3. The headline result is that we can still establish an overall price-metric bias, but it is no longer equal to the sum of the bias parameters that we estimate in the model. There are now some adjustment terms, which depend on the curvature of numeraire consumption utility. Ultimately, though, for reasonable parameterizations, there reasons to believe that these adjustment terms are quantitatively negligible.

While we relax the quasilinearity assumption, we make a more restrictive assumption on the form of misperceptions that we can accomodate. Specifically, we restrict attention to misperceptions of expected net returns, defining $\tilde{F}_i(a) = F_i(a - \gamma_i^O)$. The bettor still overestimates expected returns by γ^O, but we shut down any other kind of misperception.[60]

We define a consumption utility function $v()$, which is increasing and twice-differentiable but can be nonlinear and nonconvex. The normative and decision utility functions are as follows:

$$\text{Normative Utility} \qquad u_i^{normative}(x) = E_{F_i}\left[v_i(y_i + a \cdot x_i)\right] + z_i(x_i) \tag{C.1}$$

$$\text{Decision Utility} \qquad u_i^{decision}(x) = E_{\tilde{F}_i}\left[v_i(y_i + a \cdot x_i)\right] + z_i(x_i) + \gamma_i^{SC} \cdot x_i \tag{C.2}$$

The following proposition characterizes price-metric bias in this model.

Proposition 2. *Let $\gamma_i(x)$ denote the difference between the decision and normative marginal utilities of dollars wagered given dollars wagered x, divided by the expected normative marginal utility of consumption: $\gamma_i(x) = \frac{du_i^{decision}(x)/dx - du_i^{normative}(x)/dx}{E_{F_i}[v_i'(y_i + a \cdot x_i)]}$. We refer to γ_i as the price-metric bias. Normalize that expected normative marginal utility of consumption to 1. Then $\gamma_i(x)$ is given by the following equality.*

$$\gamma_i(x) = \gamma_i^O + \gamma_i^{SC} + (E_{\tilde{F}_i}[v_i'] - 1)(E_F[a]) + Cov_{F_i}(v_i', a) - Cov_{\tilde{F}_i}(v_i', a) \tag{C.3}$$

where $E_{\tilde{F}_i}[v_i']$ and $E_{F_i}[v_i']$ are shorthand for $E_{F_i}\left[v_i(y_i + (a + \gamma^O) \cdot x_i(\tau))\right]$ and $E_{F_i}\left[v_i(y_i + a \cdot x_i(\tau))\right]$ respectively, and the same marginal consumption utilities are used in the covariances. These terms are where the dependence on x appears.

Proposition 2 adds extra terms that account for other uninternalized costs. We show that these adjustment terms are quantitatively negligible, which justifies our focus on measuring γ_i^O and γ_i^{SC} in the field experiment. The first adjustment term, $\left(E_{\tilde{F}_i}[v_i'] - E_{F_i}[v_i']\right) E_F[a]$, arises because overoptimism may cause the agent to mispredict the expected marginal utility of consumption.[61]

[60] It is of course possible to express price-metric biases for general misperceptions, but given that our empirical analysis focuses entirely on expected returns, it is not of primary interest to do so for this project.

[61] In the most plausible case where the agent is overoptimistic and loses money on average, this adjustment term is positive. Overoptimism causes the agent to underestimate his future marginal utility of consumption, so he does not predict that the pain of losing from gambling will be as large as it actually is. This error causes overconsumption.

The second term, $Cov_{\tilde{F}_i}(v_i', a) - Cov_{F_i}(v_i', a)$, arises because overoptimism may cause the agent to mispredict the covariance between his marginal utility of consumption and returns.

Because the curvature of the numeraire consumption utility function is likely to be limited in small regions where predicted returns can differ from true exected returns, we expect that both adjustment terms will be negligble compared to the main bias parameters $\gamma_i^O, \gamma_i^{SC}$.

C.2 Perceived winnings and nonfinancial utility: theory and evidence

We report here on extensions relating to how nonfinancial utility depends on beliefs. We show that theoretically, some forms of dependence can exacerbate overoptimism and make price-metric bias larger. Empirically, though, we show using predicted responses to a hypothetical scenario that such considerations are unlikely to matter much in our context.

Theory In our baseline model, we allow nonfinancial utility $z_i(x; \tilde{F})$ can depend only on perceived net returns. In that model, changes in perceptions $\tilde{F}$ can cause the normative marginal utility of betting to change, but the consume fully iternalizes these changes so there are no impacts on bias. However, if normative utility depends directly on *true* perceived returns F (for example, because the real pleasure of betting comes from finding out that you beat your friends at the end of the month), then overoptimism can cause people to misperceive the marginal utility of betting.

To illustrate, we use the functional form of utility specified in , though the intuition is general. The extension allows the marginal utility of consumption to depeond on F as well as $\tilde{F}$. To simplify our points, we assume that the utility depends only on the expected true and perceived returns, not higher-order moments. Putting these modifications together, we write $g_i(E_F[a], E_{\tilde{F}}[a])$ instead of $g_i(\tilde{F})$, and the full normative nonfinancial utility function is:

$$z_i(x; E_F[a], E_{\tilde{F}}[a]) = z_{1i} x \log(x) + z_{2i} x + g_i(E_F[a], E_{\tilde{F}}[a]) x + h_i(E_F[a], E_{\tilde{F}}[a]) \tag{C.4}$$

Misperceptions can cause errors because people choose exclusively according to their perceptions: they choose as if the nonfinancial utility is $z_i(x; E_{\tilde{F}}[a], E_{\tilde{F}}[a])$. Therefore, there is an additional price-metric bias for overoptimistic agents: misperceptions of nonfinancial utility cause them to overvalue a dollar wagered by $\gamma_i^O \cdot \frac{\partial z_i}{\partial E_F[a]}$. In the natural case of overoptimistic agents with nonfinancial utility that is increasing in true returns, this term is positive, so accounting for it would make optimal policy more restrictive.

Evidence The key extra statistic we need to quantify misperceptions of nonfinancial utility is $\frac{\partial z_i}{\partial E_F[a]}$. This term represents the impact of increased true returns on the marginal nonfinancial utility of betting, *holding perceptions constant.* Even in an experiment, it is challenging to measure this term directly from choice data, because by definition we never can vary perceived true returns without varying perceptions. Instead, we study how choices change when we vary perceived returns, holding prices constant. This analysis allows us to measure the total effect of perceived returns on marginal nonfinancial utility $\frac{\partial z_i}{\partial E_F[a]} + \frac{\partial z_i}{\partial \tilde{F}_i}$. Assuming both terms are nonnegative, we can use the estimate to put an upper bound on $\frac{\partial z_i}{\partial E_F[a]}$.

To study the total effect of perceived returns, we asked the following survey question: *You predict that you'll [win/lose] $[R] for every $100 that you wager.*

Suppose two things happened at the same time:

- *You learned that you were 2% WORSE at making money betting than you'd thought.*
- *All sportsbooks suddenly made their odds 2% BETTER for an extended period of time.*

If this happened, your expected winnings wouldn't change: you'd still expect to [win/lose] $[R] for every $100 that you wager.

Would these events change the amount that you wager?

We plot responses to this question in Figure C.2. The modal response, given by more than 80% of participants, is that this change would not affect their consumption. The upper bound of a 95% confidence interval for τ^{offset} is 0.013.

Let τ^{offset} denote the treatment effect of the offsetting price change on predicted consumption, and let τ^{price} denote the treatment effect of a naturally occurring 2¢ price increase. To be conservative, we use the upper bound $\tau^{offset} = 0.013$, and we use $\tau^{price} = 0.223$, which comes from our preferred average semielasticity estimates. The ratio $\tau^{offset}/\tau^{price} = 0.06$ implies that a 0.06¢/$ change in perceived winnings, holding price constant, has the same impact as a 1¢/$ increase in the price. In other words, 0.06 is a measure of the total derivative $\frac{\partial z_i}{\partial E_F[a]} + \frac{\partial z_i}{\partial \tilde{F}_i}$ when utility is money-metric. Multiplying γ^O by 0.06 gives an upper bound on the price-metric bias from misperceptions of nonfinancial utility.

Since we made multiple generous assumptions and still end up with a small adjustment term (i.e, multiplying all γ_i^Os by 1.06) we conclude that accounting for these issues would not much affect the policy conclusions of this paper. Of course, there are many other reasons people could misperceive nonfinancial utility other than overoptimism. On these our papers is silent – we are focused on overoptimism and self-control problems.

C.3 Deriving the constant semielasticity demand curve

We show here how we derive the demand function (3). We rely on the specialization of nonfinancial utility to the following form:

$$z_i(x; \tilde{F}) = z_{1i} x \log(x) + z_{2i} x + g_i(\tilde{F}) x + h_i(\tilde{F}) \tag{C.5}$$

where $z_{1i} < 0$.

For notational convenience, let $\gamma_i = \gamma_i^O + \gamma_i^{SC}$ denote the sum of the biases. We consider two first order conditions for consumption: the normative status quo FOC (given $\tau_i = \tau_0$, $\gamma_i^O = \gamma_i^{SC} = 0$) and the choice FOC. We define $x_i^*(0)$ as the consumption that satisfies the normative status quo

FOC and $x_i^{choice}(\tau)$ as the consumption that satisfies the choice FOC.

$$E_F[a] - \tau_0 + z_i'(x_i^*(0)) = 0 \tag{C.6}$$

$$(E_F[a] + \gamma_i^O - \tau_i) + z_i'(x_i^{choice}(\tau_i)) + \gamma_i^{SC} = 0 \tag{C.7}$$

Taking the difference of these FOCs and using our functional form assumption yields the following expression.

$$0 = z_{1i} \log(x^*(0)) - z_{1i} \log(x^{choice}(\tau_i)) + \tau_i - \gamma_i - \tau_0 \tag{C.8}$$

We can then rearrange the equation to following expression:

$$0 = z_{1i} \log(x^*(0)) - z_{1i} \log(x^{choice}(\tau_i)) + \tau_i - \gamma_i - \tau_0 \tag{C.9}$$

$$z_{1i} \log(x^{choice}(\tau_i)) = z_{1i} \log(x^*(0)) + \tau_i - \gamma_i - \tau_0 \tag{C.10}$$

$$x^{choice}(\tau_i) = \exp\left(\log(x^*(0)) + \frac{1}{z_{1i}} \cdot (\tau_i - \gamma_i - \tau_0)\right) \tag{C.11}$$

which is exactly the form of equation (3). This result shows that parameter estimates from log-linear demand specifications admit a structural interpretation. The slope of log demand in prices is a semielasticity term which arises from the curvature of the nonfinancial utility of betting, as usual. The intercept is the log of normative demand at the reference tax.

C.4 Extension to a more general planner's problem

In this section, we state a planner's problem with a distributional and revenue-raising motive for taxation. We show how these concerns affect the optimal uniform tax on sports betting both theoretically and given our parameter estimates.

As in Section 8.1, the planner can choose a single uniform tax τ on dollars wagered. We make two modifications. First, the planner maximizes a welfare-weighted average of normative utility. We define welfare weights λ_i, normalized so that $E[\lambda_i] = 1$. Second, we assume that revenues from the tax are recycled with a multiplier k that represents the marginal value of public funds. These modifications yield the following planner's problem.

$$\begin{aligned} \max_{\tau} \quad & E_i\left[\lambda_i u_i^{normative}(x_i(\tau))\right] + kR(\tau) \\ \text{s. t.} \quad & R(\tau) = E_i[\tau x_i(\tau)] \end{aligned} \tag{C.12}$$

This problem nests the case analyzed in the main paper (equation 5) when $k = 1$ and $\lambda_i = 1$ for all i.

These modifications allow us to accommodate distributional and revenue-raising motives for sports betting taxation. We are deliberately agnostic about the microfoundations of welfare weights, allowing them to accomodate a broad variety of distributional cosiderations (Saez and Stantcheva, 2016). The interpretation of our k parameter is more subtle. If we think of this planner's problem as a reduced-form representation of a richer model with alternative revenue-raising instruments, the optimal tax system will impose that k is equal to 1. Therefore, the $k \neq 1$ case represents the problem of choosing a sports betting tax when 1) other taxes are not set optimally *ex ante* and 2) other tax instruments do not endogenously adjust to the new sports betting tax. This case may be relevant in practice, given that sports betting taxes are often set through a political process than other taxes and are earmarked to particular sources of spending.[62] The case of $k > 1$, for example, can represent a situation where sports betting tax revenue causally increases spending on programs that have positive externalities.

Let $\gamma_i = \gamma_i^O + \gamma_i^{SC}$ denote the overall price-metric bias for agent i. The following proposition characterizes the optimal tax.

Proposition 3. *The tax rate τ^* that solves the planner's problem (C.12) must satisfy the following equality:*

$$\tau^* = \frac{1}{k} E_i \left[w_i \gamma_i \right] + \frac{1}{k} Cov \left(w_i \gamma_i, \lambda_i \right) - \frac{1}{k} \cdot \frac{1}{-\overline{\eta}} Cov \left(x_i / E_i[x_i], \lambda_i \right) + \frac{k-1}{k} \cdot \frac{1}{-\overline{\eta}} \tag{C.13}$$

where $w_i = x_i' / E_i[x_i']$ denotes a weight proportional to the slope of the betting demand curve and $\overline{\eta} = E_i[x_i'] / E_i[x_i]$ is the aggregate semielasticity of demand.

The first term of equation (C.13) is the average marginal bias as described in Section 8.1 and in Allcott and Taubinsky (2015). The next two terms capture the distributional effect of the tax. The second term includes the covariance between welfare weights and the bias-correcting benefits of the tax, which is similar to the *prorgressivity of bias correction* term in (Allcott et al., 2019). When this covariance is positive, the optimal tax is higher. Our result that low-eduction bettors are more overoptimistic suggests that this term may be positive. The third term is says that if people who consume more sports betting have lower welfare weights, the optimal tax is higher.[63] The fact that sports bettors are richer than the average recipient of government transfers suggests that this term may be positive. Finally, the last term says that in the $k > 1$ case, the planner shades the optimal tax towards the revenue-maximizing rate $1/\overline{\eta}$.

In a special case where sports bettors have homogeneous welfare weights, the planner's objective function can be naturally interpreted as a weighted sum of consumer surplus and government revenues. To see this, consider a population where share p are sports bettors and share $(1-p)$ are behavioral consumers who set $x_i = 0$. Let λ^B define the homogneous sports bettor welfare weight, still assuming that welfare weights average to 1 in the population. Then the planner's objective

[62] Of course, whether these earmarks are truly non-fungible is ultimately an empirical question.

[63] Because we are not analyzing optimized tax system, the Atkinson and Stiglitz (1976) result does not hold and a commodity tax can have distributional benefits.

becomes:

$$\max_{\tau} \quad E_i\left[u_i^{normative}(x_i(\tau))\right] + \frac{k}{\lambda^B}\tau E_i[x_i(\tau)] \tag{C.14}$$

The planner places more weight on revenues when funds are valuable (k large) and when sports bettors have low welfare weights (λ^B small). For readers interested in recombining the components illustrated in Figure 10 into an weighted total surplus measure, this result provides some guidance on how to set weights.

The optimal tax formula also takes an intuitive form in this model.

$$\tau^* = \left(\frac{\lambda^B}{k}\right) \cdot E_i[w_i\gamma_i] + \left(1 - \frac{\lambda^B}{k}\right)\frac{1}{\overline{\eta}} \tag{C.15}$$

When $k = \lambda^B = 1$, this is just the optimal corrective tax as in the main paper. When revenue raising is valuable (k gets large) or sports bettors have low welfare weights (λ^B gets small). the planner shades the tax towards the revenue-maximizing rate $1/\overline{\eta}$.

We illustrate in Figure C.3 how the optimal tax depends on the weight on government funds k/λ^B. Given our model estimates, to rationalize the status quo rate, the planner must value a dollar of government revenue at less than half of a dollar of consumer surplus for sports bettors. The standard reasons for endorsing such a weight would be either that government funds are spent inefficiently or that sports bettors have higher welfare weights than non-sports bettors. Regarding the first reason, we note that sports betting taxes are often allocated towards education, which may be a high-return investment (Jackson et al., 2016; Hendren and Sprung-Keyser, 2020).[64] Regarding the second reason, we note that sports bettors are richer than the average American and richer than the average recipient of government transfers (Table 2).[65]

C.5 Bans

A key theme of the analysis in the main paper is that biases in consumption cause overconsumption of sports betting *at the margin.* To evaluate bans, we need to go a step further and understand whether the costs of sports betting exceed the benefits *overall* – that is, whether consumer surplus is positive or negative after accounting for bias. To estimate consumer surplus with our main bias and price-sensitivity estimates, we would have to use our functional form to extrapolate the demand curve to high counterfactual prices, which is undesirable.

Instead, we introduce new evidence on participants' overall perceived net benefits from sports betting. We then compare perceived net benefits to uninternalized costs. Our measure of perceived net benefits is derived from participants' willingness to accept (WTA) to stop sports bet-

[64] A caveat is that funds earmarked for a program may not causally increase spending on that program on a one-for-one basis, because revenues are at least somewhat fungible.

[65] Sports betting differs dramatically from lottery betting on this dimension. Raising revenues through state-run lotteries is widely regarded as regressive, but this concern may be less relevant for sports betting taxes.

ting entirely for a 30-day period. We elicited this WTA in survey 3 as described in Section A.3 and interpret it as a measure of consumer surplus as perceived by the long run self. Since the long run self reports the WTA and our participants are sophisticated about self-control problems, the only uninternalized cost in this perceived consumer surplus measure comes from the overoptimism. Therefore, by comparing the sizes of perceived consumer surplus and internalities from overoptimism, we can evaluate the welfare effects of shutting down all consumption. Importantly, by measuring perceived surplus directly, this approach makes no assumptions on the functional form of demand.

Our results imply that a ban would reduce welfare. Figure C.1 shows that perceived consumer surplus exceeds uninternalized costs for 93% of participants. We arrive at this conclusion because participants said that they would need to be paid a great deal to reduce future betting. The median WTA to stop betting for 30 days was $100. Such high valuations imply that participants place high value on inframarginal bets. These inframarginal bets are valuable enough to participants that a ban would be harmful.

On top of the caveats that apply to the rest of our analysis, this result should be interpreted with particular caution. Our study was mainly designed to measure bias and demand response parameters as presented in Section 6. This analysis is the only one that relies on the WTA to stop sports betting on tracked accounts, which is by design because experimental elicitaions of WTAs are known to be sensitive to framing choices in the survey. For example, Allcott et al. (2020) show that the valuations of Facebook implied by similar methods vary greatly across research studies.[66] Also, while 30 days is not a short time, the montly cost of giving up gambling for a single month may differ from the monthly cost of giving up gambling forever.[67] Overall, we view this analysis as suggestive evidence that our measured internalities are not large enough to make a ban welfare-enhancing in our sample of participants. These results have little to say about whether bans would be optimal after accounting for externalities or other unmeasured internalities.

C.6 Tables and Figures

[66]We do provide some evidence that our high valuations were not driven by confusion about the BDM incentivization mechanism: we elicited hypothetical WTAs before the incentivized WTAs, and these were even larger.

[67]For example, people may find it more challenging to substitute to other forms of gambling in the short run, making the short term WTA overstate the true perceived cost of a ban.

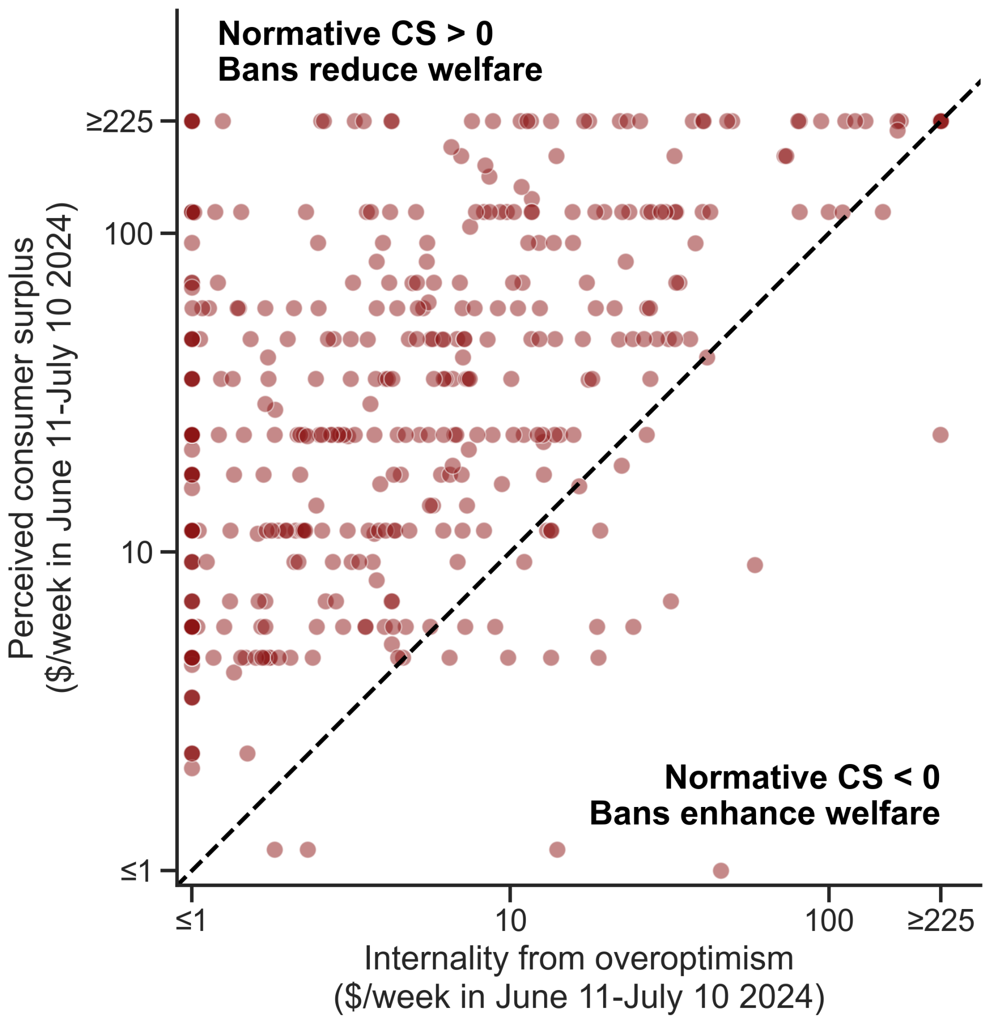

Figure C.1: Evaluating bans

Notes: The figure compares the perceived value of betting to uninternalized costs from overoptimism. Approximating self-control problems to zero, normative consumer surplus is the sum of these two components. Bans are welfare enhancing if normative consumer surplus is negative. The perceived consumer surplus measure is the WTA to stop betting on tracked apps, as elicited via an incentivized BDM mechanism. The internality from overoptimism is the expected total uninternalized financial costs. We compute it by multiplying our estimate of overoptimism γ_i^M by predicted consumption. For both measures, values are reported in units of dollars per week in the 30 days following survey 3.

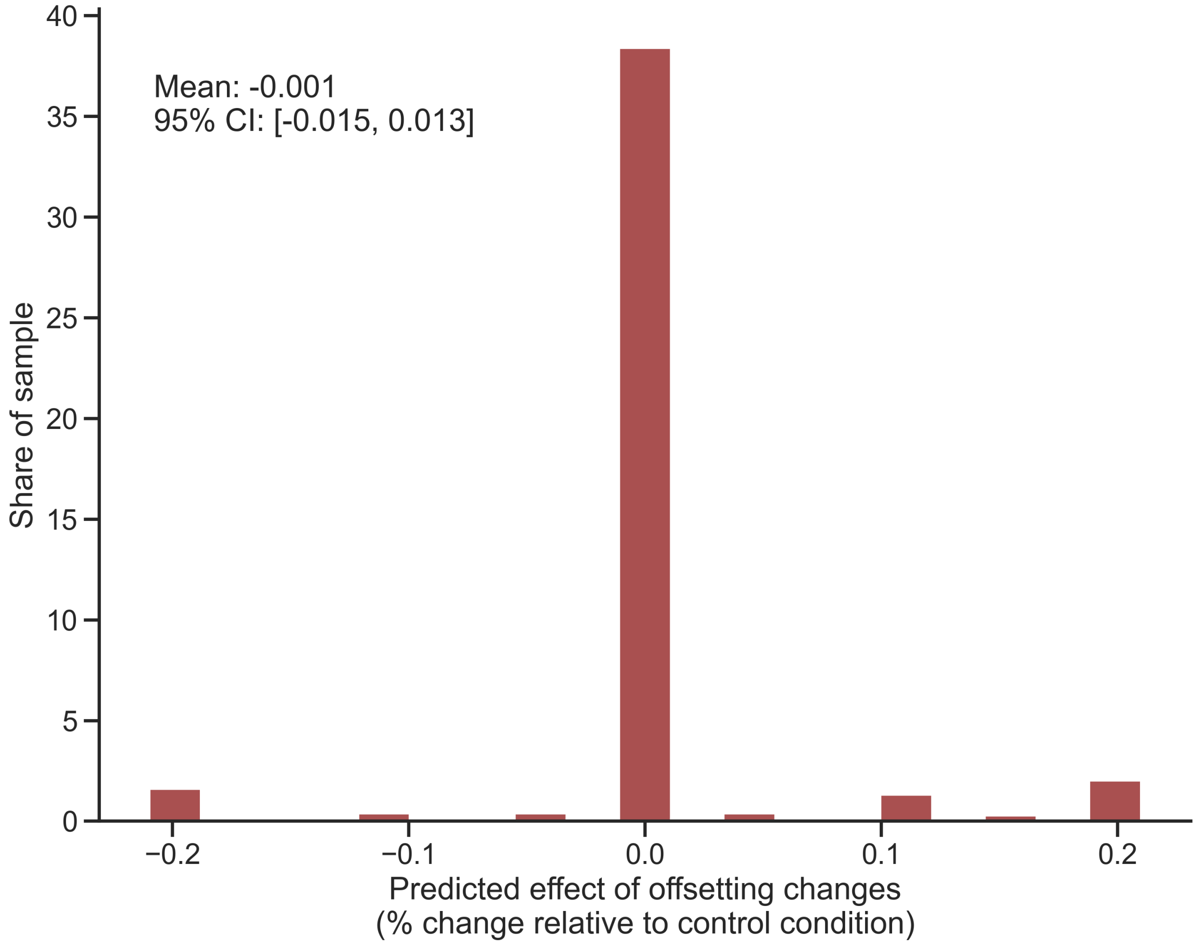

Figure C.2: Predicted effect of offsetting skill and house cut changes

Notes: The figure illustrates the predicted effects of hypothetical offsetting house cut decrease and skill increase on consumption, as described in Section **??**. Responses are truncated to fall in $[-0.2, 0.2]$.

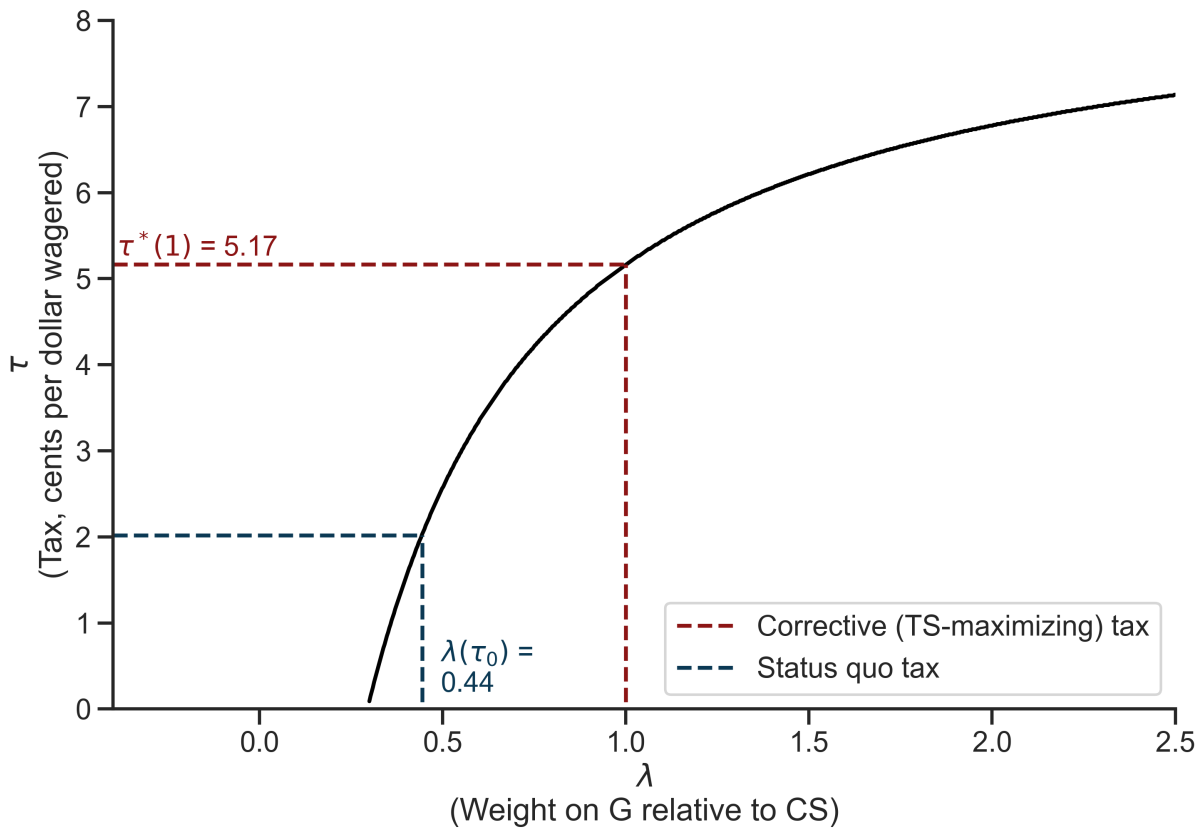

Figure C.3: Optimal taxes and the weight on government revenues

Notes: The figure plots tax rates on the y-axis against the weight on government revenues on the x-axis. The figure can be read in two ways. First, starting with a weight λ, the figure gives the optimal tax for that weight $\tau(\lambda)$. Second, starting with a tax τ, the figure gives the weight that makes the tax optmal $\lambda(\tau)$. The red point corresponds to the optimal corrective tax. The blue point shows the weight on revenues implied by status quo policy ($\tau_0 = 2.02$).

D Pre-registration Appendix

D.1 Sample restrictions

Some participants began the study with multiple synced accounts but dropped some accounts as the experiment proceeded. We pre-registered that we would treat participants with dropped accounts differently depending on the stated reason for dropping. On each survey, we asked participants to explain the reason for their dropped account. If the participant indicated that they had closed their account, then we retain them in the analysis sample and assume that they placed no wagers on that account. In all other cases, including cases where no reason for deactivation was given, we drop the participant from the analysis sample. In practice, only four participants were retained in the sample with incomplete data; all other cases were dropped.

D.2 Pre-registered analyses

The analysis presented in the body of the paper deviated at times from the pre-registered analyses. In the interest of transparency, we justify our choices and present the initial pre-registered analyes in this subsection. There are three important deviations.

First, we pre-registered randomized treatment effect regressions with winsorized log dependent variable for dollars wagered. Unfortunately, our data contains many zeros, which meant that regressions with log dependent variables are sensitive to arbitrary scaling and truncation choices. We illustrate this fact in Table D.1. The first column is our pre-registered specification for the bonus treatment effect, winsorizing at the 5th and 95th percentiles of the $t = 0$ consumption distribution of monthly wagers. Other columns report results for different winsorization choices, and coefficient estimates vary wildly. Tables D.2 and D.3 contain all pre-registered treatment effect regressions for the Bonus and limit treatments respectively.[68]

Second, the pre-analysis plan described a second study, the *screenshot study*. In the screenshot study, we collected information on betting activity with self-reports and screenshots. We also collected predicted net returns and qualitative survey responses as in the main study. Participants in the screenshot study made predictions as in the main study, and half of them were randomized into the *limits treatment*, as in the main study. None of them were randomized to receive the Bet Less Bonus or the history transparency treatment, because data limitations made these impossible to implement.

We did not include the screenshot study data in the main paper for ease of exposition, improved data quality, and because the sample size was smaller than anticipated: 54 participants completed the final screenshot study. Since we pre-registered that we would conduct relevant analyses pooling the main sample and the screenshot sample, we replicate our main overoptimism result in the pooled sample in Figure D.1. We cannot replicate our self-control problems or

[68]Due to a coding error, the *self-control scale* and *gambling literacy scales* did not appear in survey 1. Therefore, we omit them as covariates from the pre-registered specification.

Bonus treatment effect results with this sample, since the Bet Less Bonus was not included in the screenshot study.

Third, we pre-registered an average misperceptions estimate that weighted by the inverse variance of net winnings. In the main analysis sample, this estimate is 4.22¢/$. Directionally, this estimate is lower than our raw average difference presented in Figure 5, because low-variance participants overestimate net winnings by less. However, for reasons related to the discussion of shrinkage in Section B.4, this kind of weighted average is not an unbiased estimator of average overoptimism in a heterogeneous populations when true overoptimism is correlated with the precision of winnings. The Chen (2024) shrinkage procedure handles the correlation between precision and parameters explicitly and also allows us to estimate heterogeneous overoptimism, so we use it for our main estimates.

D.3 Tables and Figures

	Log weekly wagers (winsorized)					
	(1)	(2)	(3)	(4)	(5)	(6)
t=1 × Bonus	-0.556***	-0.823***	-0.781***	-0.629***	-0.416***	-0.306***
	(0.114)	(0.165)	(0.156)	(0.126)	(0.094)	(0.072)
Bonus × t=2	-0.132	-0.153	-0.149	-0.135	-0.122	-0.112
	(0.116)	(0.168)	(0.159)	(0.129)	(0.097)	(0.075)
Winsorization	[p5, p95] of t=0	[p0.5, p99.5] of t=0	[p1, p99] of t=0	[p3, p97] of t=0	[p10, p90] of t=0	[p20, p80] of t=0
Upper bound (dollars/wk)	3,993.4	23,573.7	13,684.4	6,591.3	1,847.2	701.7
Lower bound (dollars/wk)	9.36	1.17	1.69	5.83	18.8	36.0
Treatment group mean (t=1)	4.18	3.70	3.77	4.05	4.40	4.57
Control group mean (t=1)	4.77	4.59	4.62	4.73	4.84	4.87
Treatment group mean (t=0)	5.09	5.09	5.09	5.09	5.09	5.09
Control group mean (t=0)	5.14	5.14	5.14	5.14	5.14	5.14
R^2	0.61534	0.54066	0.55531	0.60314	0.61997	0.59613
Observations	884	884	884	884	884	884

Table D.1: The sensitivity of regressions with log dependent variables to winsorization choices

Notes: This table reports results from regressions of the following form: $\tilde{Y}_{it} = \alpha\tilde{Y}_{i0} + \tau_t^B B_i + \beta_t X_i + \varepsilon_{it}$ where Y_{it} is dollars wagered per week in period t, $\tilde{Y}_{it}$ is a winsorized verson of $\log(Y_{it})$, B_i is a bonus treatment indicator, and X_i is a vector of controls as defined in the main text. We handle zero values by truncating the observed $t = 1, 2$ wagers at quantiles of the $t = 0$ wager distribution (this distribution does not have zeros by construction, since it was an eligiblity criterion that participants must have placed wagers in Period 0). Columns differ in the restrictiveness of the winsorization bonuds. Column 3 reports our pre-registered spcification that truncates at the 5th and 95th percentile.

	Log weekly wagers (winsorized)			Positive play index				Makes life better survey question			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
t=1 × Bonus	-0.549***	-0.604***	-0.524***	0.910***				0.208			
	(0.115)	(0.178)	(0.151)	(0.319)				(0.128)			
t=2 × Bonus	-0.128	-0.039	0.013	0.869***				0.170			
	(0.117)	(0.152)	(0.164)	(0.313)				(0.136)			
Above median t=0 wagers × t=1 × Bonus		0.068									
		(0.231)									
Above median t=0 wagers × t=2 × Bonus		-0.206									
		(0.240)									
Above median t=0 returns × t=1 × Bonus			-0.040								
			(0.224)								
Above median t=0 returns × t=2 × Bonus			-0.275								
			(0.232)								
Bonus					0.889***	0.477	0.953**		0.189	0.087	0.220
					(0.289)	(0.397)	(0.424)		(0.119)	(0.170)	(0.166)
Above median t=0 wagers × Bonus						0.813				0.198	
						(0.584)				(0.238)	
Above median t=0 returns × Bonus							-0.147				-0.067
							(0.585)				(0.233)
Dep. var. Range	-	-	-	[-10,10]	[-10,10]	[-10,10]	[-10,10]	[-5,5]	[-5,5]	[-5,5]	[-5,5]
Dep. var. SD	1.83	1.83	1.83	3.50	3.50	3.50	3.50	1.74	1.74	1.74	1.74
Dep. var. Mean	4.44	4.44	4.44	7.10	7.10	7.10	7.10	1.42	1.42	1.42	1.42
R^2	0.62262	0.62604	0.62414	0.09127	0.09126	0.09436	0.09346	0.40031	0.40028	0.40104	0.40122
Observations	884	884	884	884	884	884	884	884	884	884	884

Table D.2: Pre-registered Bonus treatment effect regressions

Notes: The table contains all pre-registered Bonus treatment effects, as specified in section 3.4 of the pre-analaysis plan. An observation is an individual $\times$ $t = \{1, 2\}$. We drop two participants who did not place any wagers in the 30 days before the study began. The dependent variables are log weekly wagers (winsorized at the 5th and 95th percentile), the positive play index (defined in Section A.2), and responses to the "sports betting makes life better" survey question (defined in Section A.2), for columns (1-3), (4-7), and (8-11) respectively. All columns include the following covariates: a limits treatment indicator, log winsorized pre-study wagers, elicited beliefs about period-1 winnings, a measure of information conveyed in the information treatment, white, income terciles, and randomization stratum indicators. The "makes life better" columns also include the baseline outcome meaured on survey 1 as an ourcome. Columns (1), (4), and (8) implement regressions as specified in equation B.8 in Section 6.3, and coefficients are interpreted as in that section. Columns (2) and (3) add heterogeneity by pre-study wagers and pre-study returns respectively. Columns (5) and (9) pool the bonus treatment effect across periods [CHANGE THIS].

	Log weekly wagers (winsorized)			Positive play index				Makes life better survey question			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
t=1 × Limits	0.044	0.019	-0.047	0.093				0.223*			
	(0.107)	(0.153)	(0.152)	(0.314)				(0.127)			
t=2 × Limits	0.140	0.086	-0.055	-0.275				-0.105			
	(0.117)	(0.146)	(0.169)	(0.330)				(0.133)			
Above median t=0 wagers × t=1 × Limits		0.019									
		(0.211)									
Above median t=0 wagers × t=2 × Limits		0.086									
		(0.234)									
Above median t=0 returns × t=1 × Limits			0.188								
			(0.220)								
Above median t=0 returns × t=2 × Limits			0.398*								
			(0.229)								
Limits					-0.091	0.202	-0.410		0.059	0.255*	0.114
					(0.294)	(0.389)	(0.426)		(0.119)	(0.149)	(0.170)
Above median t=0 wagers × Limits						-0.560				-0.428*	
						(0.586)				(0.240)	
Above median t=0 returns × Limits							0.612				-0.117
							(0.581)				(0.237)
Dep. var. Range	-	-	-	[-10,10]	[-10,10]	[-10,10]	[-10,10]	[-5,5]	[-5,5]	[-5,5]	[-5,5]
Dep. var. SD	1.83	1.83	1.83	3.50	3.50	3.50	3.50	1.74	1.74	1.74	1.74
Dep. var. Mean	4.44	4.44	4.44	7.10	7.10	7.10	7.10	1.42	1.42	1.42	1.42
R^2	0.61156	0.61379	0.61435	0.09734	0.09665	0.09904	0.10038	0.39653	0.39429	0.40050	0.39548
Observations	884	884	884	884	884	884	884	884	884	884	884

Table D.3: Pre-registered Limits treatment effect regressions

Notes: The table contains all pre-registered Bonus treatment effects, as specified in section 3.4 of the pre-analaysis plan. An observation is an individual × $t = \{1, 2\}$. We drop two participants who did not place any wagers in the 30 days before the study began. The dependent variables are log weekly wagers (winsorized at the 5th and 95th percentile), the positive play index (defined in Section A.2), and responses to the "sports betting makes life better" survey question (defined in Section A.2), for columns (1-3), (4-7), and (8-11) respectively. All columns include the following covariates: a limits treatment indicator, log winsorized pre-study wagers, elicited beliefs about period-1 winnings, a measure of information conveyed in the information treatment, white, income terciles, and randomization stratum indicators. The "makes life better" columns also include the baseline outcome meaured on survey 1 as an ourcome. Columns (1), (4), and (8) implement regressions as specified in equation B.8 in Section 6.3, and coefficients are interpreted as in that section. Columns (2) and (3) add heterogeneity by pre-study wagers and pre-study returns respectively. Columns (5) and (9) pool the bonus treatment effect across periods [CHANGE THIS].

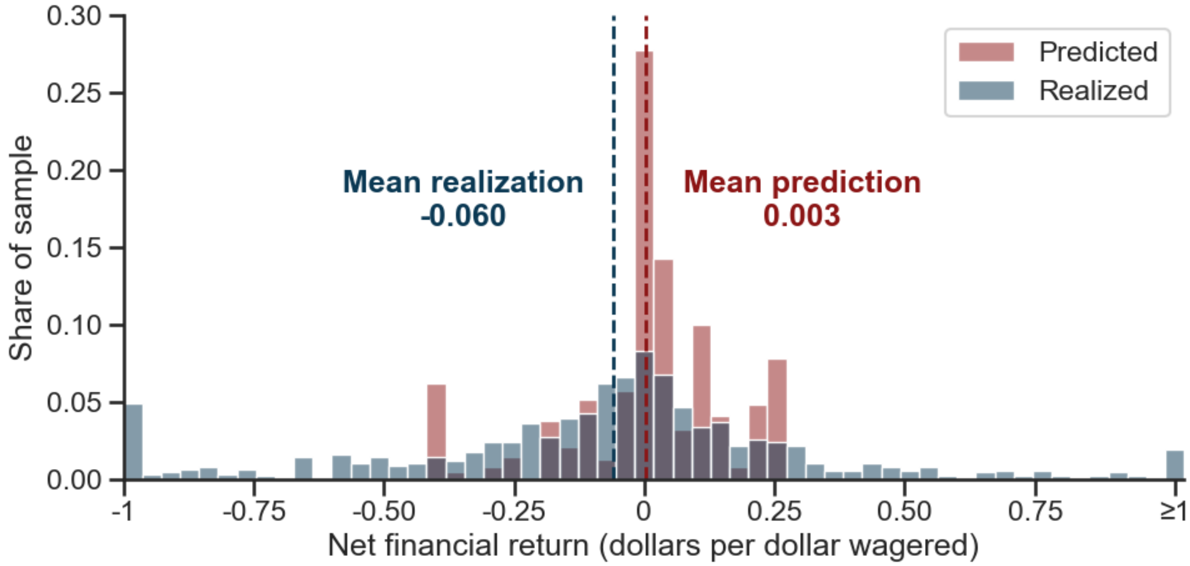

Figure D.1: Comparing predicted financial returns to realized financial returns (pooled sample)

Notes: This figure is the pooled-sample analog of Figure 5. It containes prediction and realization data for the participants in our main analysis sample as well as 52 additional participants from the screenshot study. For participants in the screenshot study, realized returns come from self-reported monthly dollars wagered and winnings on DraftKings and Fanduel. We use screenshots of account history screens on those apps to validate these self-reports. Predictions are censored to lie within $[-0.4, 0.25]$. Dotted lines and annotations represent averages.

E Proofs Appendix

E.1 Proof of Proposition 1

Let $U(\tau)$ denote the predicted indirect utility function for the agent choosing in advance. That is, we let $\tilde{x}_i(\tau) = \arg\max_x \tilde{u}^{Choice}(x)$ be the maximizer of the predicted utility function and and $U(\tau) = u_t^{Choice}(\tilde{x}_i(\tau))$. be the utility of that predicted value. We consider the difference between $U(\tau_0)$ and $U(\tau_0 + \Delta)$. We can expand this expression as a taylor series:

$$U(\tau_0 + \Delta) - U(\tau_0) = \Delta \frac{dV}{d\tau} + \frac{\Delta^2}{2}\frac{d^2 V}{d\tau^2} + \frac{\Delta^3}{6}\frac{d^3 V}{d\tau^3} + ... \tag{E.1}$$

Writing out each derivative,

$$\frac{dU(\tau_0)}{d\tau} = \underbrace{\tilde{x}_i'(\tau_0) \cdot \left(\frac{du_t^{Choice}(\tilde{x}_i(\tau_0))}{dx}\right)}_{\text{Effect of behavior change}} - \underbrace{\tilde{x}_i(\tau_0)}_{\text{Mechanical Effect}} \tag{E.2}$$

$$= \tilde{x}_i'(\tau_0) \cdot \left(-\tilde{\gamma}_i^{SC}\right) - \tilde{x}_i(\tau_0) \tag{E.3}$$

$$\frac{d^2 U(\tau_0)}{d\tau^2} = \tilde{x}_i''(\tau_0) \cdot \left(-\tilde{\gamma}_i^{SC}\right) - \tilde{x}_i'(\tau_0) \tag{E.4}$$

$$\frac{d^3 U(\tau_0)}{d\tau^3} = O(\tilde{x}_i''(\tau)) \tag{E.5}$$

Substituting these derivatives into the difference in value functions, we get

$$U(\tau_0 + \Delta) - U(\tau_0) = \Delta\left(\tilde{x}_i'(\tau_0) \cdot \left(-\tilde{\gamma}_i^{SC}\right) - \tilde{x}_i(\tau_0)\right) + \frac{\Delta^2}{2}\left(\tilde{x}_i''(\tau_0) \cdot \left(-\tilde{\gamma}_i^{SC}\right) - \tilde{x}_i'(\tau_0)\right) \tag{E.6}$$

$$+ O(\Delta^3)$$

$$= -\Delta \tilde{x}_i(\tau_0) - \Delta\left(\tilde{\gamma}_i^{SC} + \frac{1}{2}\Delta\right)\tilde{x}_i'(\tau_0) - \frac{\Delta^2}{2} x_i''(\tau_0) \cdot \tilde{\gamma}_i^{SC} + O(\Delta^3) \tag{E.7}$$

$$= -\Delta \tilde{x}_i(\tau_0) - \Delta\left(\tilde{\gamma}_i^{SC} + \frac{1}{2}\Delta\right)\tilde{x}_i'(\tau_0) + O(\Delta^3, \Delta^2 \tilde{x}_i''(\tau)) \tag{E.8}$$

$$\implies -\frac{U(\tau_0 + \Delta) - U(\tau_0)}{\Delta} = \tilde{x}_i(\tau_0) + \left(\tilde{\gamma}_i^{SC} + \frac{1}{2}\Delta\right) x_i'(\tau_0) + O(\Delta^3, \Delta^2 \tilde{x}_i''(\tau)) \tag{E.9}$$

Ans we can rearrange the last expression (again using the assumption that $\Delta^2 x_i''(\tau)$ terms are negligible) to get

$$\underbrace{\frac{U(\tau_0 + \Delta) - U(\tau_0)}{\Delta}}_{\text{Predicted value of price increase}} = \underbrace{\frac{\tilde{x}_i(\tau_0) + x_i(\tau_0 + \Delta)}{2}}_{\text{Predicted time consistent surplus loss}} - \underbrace{\tilde{\gamma}_i^{SC} \tilde{x}_i'(\tau_0)}_{\text{Behavior change premium}} + \underbrace{O(\Delta^3, \Delta^2 x_i''(\tau))}_{\text{Approximation error}} \tag{E.10}$$

Now, we compare equation (E.10) for participants with no self-control problems and those who do not (i.e, $V(\Delta;\tilde{\gamma}^{SC})$ versus $V(\Delta;0)$). These terms are defined as $U(\tau_0+\Delta)-U(\tau_0)$ for a particular level of $\tilde{\gamma}^{SC}$. Note that the first term (predicted time consisitent surplus loss) does not depend on $\tilde{\gamma}^{SC}$ at all. So we end up with the following difference:

$$\frac{V(\Delta;\tilde{\gamma}^{SC})-V(\Delta;0)}{\Delta}=\tilde{\gamma}^{SC}\tilde{x}_i'(\tau_0)+O(\Delta^3,\Delta^2\tilde{x}_i''(\tau)) \tag{E.11}$$

Multiplying both sides by Δ yields the result of the proposition.

E.2 Proof of Proposition 3

The derivative of normative utility with respect to x at the chosen consumption level is always $-\gamma_i$, since the agent chooses to set to decision marginal utility to zero and normative marginal utility is γ_i lower than this. Using this fact and substituting the budget constraint into the objective function yields the following first-order condition:

$$E[-\lambda_i\gamma_i x_i'(\tau)]-E[\lambda_i x_i(\tau)]+k(\tau E[x_i'(\tau)]+E[x_i(\tau)])=0 \tag{E.12}$$

We isolate for τ and conduct a few algebraic manipulations.

$$\tau=-\frac{1}{-kE[x_i'(\tau)]}\left(E[-\lambda_i\gamma_i x_i'(\tau)]-E[\lambda_i x_i(\tau)]+kE[x_i(\tau)]\right) \tag{E.13}$$

$$\implies \tau=\frac{1}{k}\left(E\left[\lambda_i\gamma_i\frac{x_i'(\tau)}{E[x_i'(\tau)]}\right]-E\left[\lambda_i\frac{x_i(\tau)}{-E[x_i'(\tau)]}\right]\right)-\frac{E[x_i(\tau)]}{E[x_i'(\tau)]} \tag{E.14}$$

$$=\frac{1}{k}\left(E\left[\lambda_i\gamma_i\frac{x_i'(\tau)}{E[x_i'(\tau)]}\right]-\left(1\cdot E\left[\frac{x_i(\tau)}{-E[x_i'(\tau)]}\right]+Cov\left(\lambda_i,\frac{x_i(\tau)}{-E[x_i'(\tau)]}\right)\right)\right)-\frac{E[x_i(\tau)]}{E[x_i'(\tau)]} \tag{E.15}$$

$$=\frac{1}{k}\left(E\left[\lambda_i\gamma_i\frac{x_i'(\tau)}{E[x_i'(\tau)]}\right]-\left(\frac{1}{-\overline{\eta}}+\frac{1}{-\overline{\eta}}Cov\left(\lambda_i,\frac{x_i(\tau)}{-E[x_i(\tau)]}\right)\right)\right)-\frac{1}{-\overline{\eta}} \tag{E.16}$$

$$=\frac{1}{k}\left(E\left[\lambda_i\gamma_i\frac{x_i'(\tau)}{E[x_i'(\tau)]}\right]-\left(\frac{1}{-\overline{\eta}}+\frac{1}{-\overline{\eta}}Cov\left(\lambda_i,\frac{x_i(\tau)}{-E[x_i(\tau)]}\right)\right)\right)+\frac{1}{-\overline{\eta}} \tag{E.17}$$

$$=\frac{1}{k}\left(E\left[\lambda_i\gamma_i\frac{x_i'(\tau)}{E[x_i'(\tau)]}\right]-\frac{1}{-\overline{\eta}}Cov\left(\lambda_i,\frac{x_i(\tau)}{-E[x_i(\tau)]}\right)\right)+\frac{k-1}{k}\frac{1}{-\overline{\eta}} \tag{E.18}$$

Defining the weights w_i as in Proposition 3 allows us to derive the final result.

$$\tau=\frac{1}{k}\left(E\left[\lambda_i\gamma_i w_i\right]-\frac{1}{-\overline{\eta}}Cov\left(\lambda_i,\frac{x_i(\tau)}{-E[x_i(\tau)]}\right)\right)+\frac{k-1}{k}\frac{1}{-\overline{\eta}} \tag{E.19}$$

$$=\frac{1}{k}\left(1\cdot E\left[\gamma_i w_i\right]+Cov\left(\gamma_i w_i,\lambda_i\right)-\frac{1}{-\overline{\eta}}Cov\left(\lambda_i,\frac{x_i(\tau)}{-E[x_i(\tau)]}\right)\right)+\frac{k-1}{k}\frac{1}{-\overline{\eta}} \tag{E.20}$$

E.3 Proof of Proposition 2

We begin with an expression for the price-metric bias and conduct algebraic manipulations until we arrive at the result in the proposition.

$$\gamma_i(x) = \frac{\frac{du_i^{decision}(x)}{dx} - \frac{du_i^{normative}(x)}{dx}}{E_{F_i}[v_i'(y_i + a \cdot x_i)]} \tag{E.21}$$

$$= \frac{E_{\tilde{F}}[v'(y + ax)a] + \gamma^{SC} - E_F[v'(y + ax) \cdot (a)]}{E_F[v'(y + ax)]} \tag{E.22}$$

$$= \frac{E_{\tilde{F}}[v'] \cdot E_{\tilde{F}}[(a)] - E_F[v'] \cdot E_F[(a)] + Cov_{\tilde{F}}(v', a) - Cov_F(v', a) + \gamma^{SC}}{E_F[v']} \tag{E.23}$$

$$= \gamma^O + \gamma^{SC} + (E_{\tilde{F}}[v'] - 1) \cdot E_F[(a)] + Cov_{\tilde{F}}(v', a) - Cov_F(v', a) \tag{E.24}$$