

# 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 is driven by behavioral bias rather than a normative preference to gamble. 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 about 8 cents for every dollar wagered. Self-control problems are smaller and less common. 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, implementation is challenging: we show that two bias-correction interventions favored by the gambling industry do not deliver the targeting improvements.

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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 consumption of a new good. However, some critics argue that sports betting is driven by behavioral biases. One issue 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 misperceptions could be worse for sports betting than for other kinds of gambling, like lotteries. A second issue 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 the vast majority of wagers 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 sports betting is partially driven by bias rather than preferences, and we combine this evidence with a model of biased sports betting consumption to evaluate various policy interventions. We conduct a field experiment with a sample of high-volume sports bettors and use techniques from behavioral economics to study the biases of interest. We present reduced-form evidence on bias and use the evidence to estimate a structural model of biased betting demand. We use the estimated model to evaluate the welfare effects of corrective interventions. We simulate the effects of corrective interventions such as taxes and information provision. 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 that we will measure experimentally: 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. Both biases enter as costs to betting that are not internalized in consumption choices. These uninternalized costs are key objects for optimal policy. In particular, money-metric uninternalized costs determine the optimal corrective tax (Bernheim and Taubinsky, 2018), since the socially optimal level of betting arises when a tax causes consumers to internalize all net costs of betting.

Our experiment has two goals: to measure the biases that we define in the model and to evaluate real-world bias-correction interventions. We measure overoptimism of financial returns by

comparing predicted financial returns to the truth. We measure self-control problems by eliciting participants' willingness to pay to reduce future betting consumption. We evaluate bias-correcting interventions by studying two experimental treatments that simulated the most important features of two real-world interventions.

We recruited a sample of high-volume sports bettors for the experiment and collected observational data on their sports betting activity. We focus on high-volume sports bettors because they are the population who would be most impacted by any policy interventions. Compared to a representative sample of weekly sports bettors, our sample is more educated and does better on qualitative measures of bias. These differences suggest that our unweighted bias estimates will be conservative. All participants were required to sync their sportsbook accounts to our study via an online portal, which allowed us to directly observe their consumption. We use this observational consumption data because self-reports can be unreliable in gambling contexts (Heirene et al., 2022).

To measure the first bias, overoptimism about financial returns, we elicited predictions of future financial returns. We elicited predicted net returns in three surveys over a two-month period. We interpret the difference between average predicted net returns and average realized net returns as a measure of average overoptimism.

We find evidence of substantial overoptimism: the average participant predicts that they will break even, but in fact loses 7.5¢ for every dollar wagered. This overoptimism is larger for bettors who prefer wagering on multi-leg bets, or parlays, which validates concerns that these complex wagers are particularly confusing for consumers. Overoptimism is also larger for bettors with lower levels of education, which implies that bias is regressive in the sense that more disadvantaged consumers are most susceptible to mistakes. To estimate the distribution of overoptimism more flexibly, we use empirical bayes methods (Chen, 2024). We find substantial heterogeneity: 10% of participants are *underoptimistic* about financial returns to betting, and another 10% are overoptimistic by more than 20¢/dollar.

To measure the second bias, self-control problems, we elicited participants' valuations of the Bet Less Bonus, which was an experimental incentive to reduce future sports betting. Intuitively, the Bet Less Bonus will be more valuable for participants who perceive themselves to have self-control problems. This is because they predict that they will bet too much in the future, so they are willing to pay to reduce future betting. Following a recent literature in behavioral economics (Carrera et al., 2022), we use valuations of the Bonus to identify participants' perceptions of their own self-control problems. Separately, to estimate whether participants are sophisticated about their own self-control problems, we elicited predictions of future consumption and compare the predictions to the truth. We combine our estimate of sophistication with our estimate of perceived self-control problems 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 were time-consistent. Ex-

cess valuations of the Bet Less Bonus are correlated with proxies of self-control problems, which validates them as a measure of perceived self-control problems. However, when translated into dollar units, self-control problems are much smaller than overoptimism. This result suggests that for our sample, overoptimism about financial returns are the most important bias to address with policy. We also find that participants are sophisticated about self-control problems: they do not underestimate their future consumption. 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 can estimate our model of sports betting consumption and evaluate corrective policies, beginning with taxes. For tractability, we assume a constant semielasticity of demand functional form, and we use participants' predictions of the effect of the Bet Less Bonus on their own consumption to estimate that semielasticity. In our preferred specification, the implied optimal corrective tax on sports betting is 5.17¢ per dollar, which is about twice as large as prevailing taxes. This tax would reduce betting by 32% 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 that have been proposed by sportsbooks. To address overoptimism about financial returns, some sportsbooks have recently improved the extent to which winnings and losses are transparently displayed on apps and websites. In our experiment, participants in the *history transparency treatment* viewed summary information about their own past net returns. To address self-control problems, sportsbooks typically include a functionality 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 does not reduce overoptimism, and the limits treatment only partially addresses self-control problems. The history transparency treatment did not affect beliefs about future returns on average because the average participant was not overoptimistic about their historical winnings – they were only overoptimistic about future winnings. Moreover, overoptimism about the past was not correlated with overoptimism about the future, so the treatment effects were not well-targeted. In the limits treatment, we find that some participants chose to set binding limits, which reduces overconsumption from self-control problems. However, the limits only partially mitigate self-control problems, because 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 bias-correction as currently implemented is 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 has established that some gamblers experience self-control problems.<sup>1</sup> 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.<sup>2</sup>

There is also a reduced-form literature in economics on the impacts of legal gambling.<sup>3</sup> Two new papers, Baker et al. (2024) and Hollenbeck et al. (2024), show that sports betting legalization causes adverse outcomes for households, increasing credit card debt and bankruptcies while reducing investments in asset markets. These analyses, while valuable, do not quantify the extent to which betting is driven by bias vs. preferences, which is a fundamental question for policy evaluation.

Our analysis of bias-correction interventions builds off recent work that highlights the importance of targeting for the welfare effects of nudges and evaluates targeting in lab experiments (Ambuehl et al., 2022b,a; Allcott et al., 2022a; List et al., 2023). We apply these insights in our field context. Our result on the poor targeting of the history transparency treatment supports the view that analyses of nudges that abstract from heterogeneity can be misleading.

We connect to three other literatures in economics. First, we contribute to the literature on biased beliefs in gambling.<sup>4</sup> Many papers in this literature posit a specific model of biased beliefs; our overoptimism measure can be interpreted as a reduced-form measure of the biased beliefs that arise from these models. Second, we contribute to the behavioral economics literature on overoptimism.<sup>5</sup> We use multiple measurements and empirical Bayes methods to estimate a distribution of overoptimism in a context with noisy belief elicitation and noisy outcomes. Third, we contribute to the behavioral economics literature on commitment devices. This literature has made the theoretical point that uncertain taste shocks reduces the demand for commitment devices (Laibson,

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<sup>1</sup>See Potenza et al. (2019) for a review of the psychiatric and medical literatures.

<sup>2</sup>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.

<sup>3</sup>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. Papers specifically studying the rollout of legal sports betting in the U.S. include: Baker et al. (2024); Hollenbeck et al. (2024); Taylor et al. (2024); Couture et al. (2024); Matsuzawa and Arnesen (2024); Humphreys and Ruseski (2024).

<sup>4</sup>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.

<sup>5</sup>See Grubb (2015), Daniel and Hirshleifer (2015), and Santos-Pinto and de la Rosa (2020) for reviews of work in field settings and Windschitl and Stuart (2015) for a review of psychological papers documenting overoptimism. An influential lab experiment literature has documented that overoptimistic beliefs can arise because people over-weight feedback with "good news" relative to "bad news" (for example, they might weight winning bets more strongly than losing bets when updating about ability). Möbius et al. (2022) is one such paper and also provides a review of related work. By contrast, we provide only limited evidence about the *mechanism* by which overoptimistic beliefs arise; our goal is only to measure overoptimism in dollar units and study implications for policy.

2015; Strack and Taubinsky, 2021). In our analysis of the limits treatment, we provide suggestive evidence that such concerns could reduce the effectiveness of a real-world commitment device.

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, policy-makers 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 which offer sports bets) to accept wagers via cell phone apps and websites.<sup>6</sup> 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-known that some gamblers report feeling urges to gamble that are challenging to resist.<sup>7</sup> Combining gambling with smartphones 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 both on the odds set by sportsbooks and on the bettors’ skill at picking advantageous bets. Sportsbooks make money by setting unfair odds. 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, it is possible for skilled bettors to 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

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<sup>6</sup>Source: <https://www.americangaming.org/research/state-gaming-map-mobile/>

<sup>7</sup>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)

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.<sup>8</sup>

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.<sup>9</sup> 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.<sup>10</sup> 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.

## 2.2 Existing Sports Betting Regulations in the U.S.

From 1992 to 2018, sports betting was regulated at the federal level by Professional and Amateur Sports Protection Act (PAPSA). PAPSA made sports betting mostly illegal, with exceptions 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.<sup>11</sup> 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.<sup>12</sup> 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

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<sup>8</sup>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)

<sup>9</sup>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.

<sup>10</sup>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.

<sup>11</sup>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.”

<sup>12</sup><https://www.sportsbookreview.com/news/us-betting-revenue-tracker/>.

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 both 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 2 reports the share of dollars wagered that end up as state revenues for each state in 2023. At the high end, states like New York tax 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. is currently 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.<sup>13</sup> 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 a variety of 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 on their wagering activity” on a monthly basis (Commonwealth of Australia, 2022).<sup>14</sup> To correct self-control problems, some states require sportsbooks to let users set binding limits on their own wagering activity. As of 2022, more than two thirds of jurisdictions with legal sports betting required operators to provide a mechanism for users to self-limit.<sup>15</sup>

Perhaps surprisingly, it is common for sportsbook operators to voluntarily design, implement, and promote bias-correction interventions on their platforms. Regarding history transparency, DraftKings recently rolled out *My Stat Sheet*, a page which provided users with enhanced visual and numerical information about their own past winnings and losses. The express goal of *My Stat Sheet* is to “help customers evaluate their play and make informed choices.”<sup>16</sup> Regarding self-imposed limits, sportsbooks typically include limit functionality even in states where it is not required by law.

These bias-correction interventions have obvious upsides. They are light-touch, cheap to implement, and potentially well-targeted at heterogeneous bias. However, skeptics have argued that

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<sup>13</sup><https://www.americangaming.org/policies/hot-issue-sports-betting/>

<sup>14</sup>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)

<sup>15</sup><https://www.americangaming.org/resources/responsible-gaming-regulations-and-statutes-guide>

<sup>16</sup>DraftKings press release: <https://draftkings.gcs-web.com/news-releases/news-release-details/draftkings-launches-my-stat-sheet-new-tool-promote-responsible>

in practice, they do not have material impacts and distract from 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.

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 the so-called “entertainment value” of betting. 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  $a \in [-1, \infty)$  to be the *realized net return* of the wager. 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 specifically 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  $z_i(x_i; \tilde{F}_i)$ . This function captures reasons for enjoying 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 assume that  $z_i$  is concave.<sup>17</sup> We allow for

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<sup>17</sup>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.

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  $z_i(x; F_i)$ . The temptation parameter  $\gamma_i^{SC}$  captures the extent to which compulsions cause the agent to bet more than would be normatively optimal for himself, given his own understanding of his net returns distribution.

As is standard in the literature, we distinguish between sophistication and naivete about self-control problems by defining *perceived* self-control problems  $\tilde{\gamma}_i^{SC}$ . Perceived self-control problems govern the agent's predictions of future consumption: he predicts he will consume as if his nonfinancial utility is  $z_i(x; \tilde{F}_i) + \tilde{\gamma}_i^{SC}x$ .

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

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

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.

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

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 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 definitions in a demand curve diagram in Figure 1. 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  $\gamma_i^O$ . Further adding self-control problems causes choices to be driven by short-term demand 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  $\gamma_i^{SC}$ . Both parameters appear graphically as vertical distances, so they are defined in price units.

These price-metric biases are crucial objects for policy design (Allcott and Taubinsky, 2015). Intuitively, biases measured in these units 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 1, a price change equal to the sum of the price-metric biases would shift consumption to point A, the consumption which maximizes normative utility. Policymakers can use instruments like corrective taxes to induce such price changes.

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 micro-foundations that could generate either bias. Instead, we view  $\gamma_i^O$  and  $\gamma_i^{SC}$  as reduced-form objects capturing the extent to which overoptimism and on self-control problems cause net costs of betting to not be internalized in decisions, whatever the underlying behavioral model of bias.<sup>18</sup>

### 3.3 Discussion

While we allow for general heterogeneity in bias across people, we impose that within person, the bias parameters  $\gamma_i^O$  and  $\gamma_i^{SC}$  are constant with respect to the level of consumption. This assumption makes our counterfactual analysis tractable by allowing us to hold  $\gamma_i^O$  and  $\gamma_i^{SC}$  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 who are concerned about such issues, we note that our experimental measures of bias have natural interpretations even when bias can vary with consumption.<sup>19</sup>

Quasilinear utility is another important simplification. In Appendix C, we study a more general version of the model with nonlinear numeraire consumption utility. In Section 8, 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.<sup>20</sup>

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<sup>18</sup>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.

<sup>19</sup>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,  $(\int_0^{x_i} \gamma_i^O(x) dx) / x_i$ . Our estimates of self-control problems correspond to average self-control problems at the chosen level of consumption,  $\gamma_i^{SC}(x_i)$ .

<sup>20</sup>See Bernheim and Taubinsky (2018) for a more detailed discussion of normative approaches in behavioral public economics.

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 own actions: they think betting is less costly than it actually 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). Overoptimism may cause also cause the agent to bet more for other reasons – for example, he may find it more fun to gamble when he thinks you are 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. Formally, perceptions  $\tilde{F}_i$  enter nonfinancial utility identically in the normative and decision utility functions. In Appendix C, we consider an extension where overoptimism can cause misperceptions of nonfinancial utility, discussing implications and evidence.

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

## 4 Field Experiment

Our experiment was designed to measure the biases of interest in dollar units for real-world sports bettors. We study overoptimism by comparing predicted net returns to realized net returns, and we study self-control problems by measuring participants' willingness to pay to reduce their own future betting. Separately, we implemented two experimental treatments to study the impacts of bias-correction interventions on bias.

### 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. Given resource constraints, we decided to focus on high-volume bettors because they are the group for whom the welfare effects of sports betting policy would be most important. In practice, they account for almost all sports betting consumption: a U.K. study shows that the top 20% of bettors account for 94% of dollars wagered (Forrest and McHale, 2024). To reach this population, we conducted a targeted social media ad campaign.<sup>21</sup> Appendix Figure A.1 presents

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<sup>21</sup>A few participants were recruited via a snowball design, in which current participants were asked to refer friends who were regular mobile sports bettors. Referred participants were always assigned to the same treatment

a sample advertisement, and Appendix Table A.1 summarizes our targeting procedures. Upon clicking our ad, we directed participants to an intake survey where the first module contained an initial eligibility screen. 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 our 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.<sup>22</sup> 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, which allows us to correlate observable features of betting activity with our bias estimates.<sup>23</sup>

Many prospective participants found our syncing procedure challenging or invasive, so they dropped out before the study began. Despite extensive technical support from the researchers, only 666 eligible participants successfully synced one account. However, conditional on syncing one account, people were likely to finish syncing all of their accounts: 555 people were fully compliant by the time they received Survey 1.

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 be the 30-day period between May 11 and 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.

We implemented all randomizations (the Bonus treatment, History Transparency treatment, and Limits treatment) in Survey 1. All treatments were independently cross-randomized, stratified

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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.

<sup>22</sup>According to Baker et al. (2024), DraftKings and FanDuel alone account for more than 70% of sports betting transactions.

<sup>23</sup>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.

fied by period-0 net returns and period-0 dollars wagered. The History Transparency and Limits treatments were randomized 50-50 into treatment and control. The Bonus treatment condition was control with probability 0.6475, treatment with probability 0.3475, and a third condition to ensure incentive compatibility of our MPL elicitations with probability 0.005. We describe the nature and purpose of these treatment conditions in the following sections.

We undertook several procedures to minimize attrition among the 533 participants who completed Survey 1.<sup>24</sup> 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. We exclude the participants randomized into the MPL condition from this group to define our main analysis sample of 444 participants.

## 4.2 Measuring Overoptimism About Financial Returns

All surveys began with a module where participants predicted their financial return for the next period. Specifically, 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  $\gamma^M$ . 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

Our measure of self-control problems combines a measure of perceived self-control problems with a measure of naivete about self-control problems. Equation (3) summarizes our approach at a high level. The self-control problems parameter  $\gamma^{SC}$  is the object governing distortions in sports betting

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<sup>24</sup>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.

consumption. To measure it, we separately study participants' perceptions of their own future self-control problems ( $\tilde{\gamma}^{SC}$ ), and their naivete about self-control problems, or the extent to which those perceptions are mistaken ( $\gamma^{SC} - \tilde{\gamma}^{SC}$ ).

$$\gamma^{SC} = \underbrace{\tilde{\gamma}^{SC}}_{\text{Perceived self-control problems}} + \underbrace{(\gamma^{SC} - \tilde{\gamma}^{SC})}_{\text{Naivete about self-control problems}} \quad (3)$$

Our design follows a recent literature in behavioral economics that uses similar methods to study models of self-control problems with partial sophistication (Acland and Levy, 2015; Chaloupka et al., 2019; Augenblick and Rabin, 2019; Carrera et al., 2022; Allcott et al., 2022c,b).

#### 4.3.1 Measuring Naivete About Self-control Problems

We measure naivete about self-control problems by comparing predictions 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.

We elicited predictions of future dollars wagered in a survey module that immediately followed the elicitation of future net returns described in Section 4.2. As in that Section, we used a structured elicitation procedure to ensure that responses were not contaminated by confounding factors. 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. As with the return prediction elicitation, we randomized whether the predictions were incentivized in surveys 1 and 2.

#### 4.3.2 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.<sup>25</sup> For bettors who wager less than \$B·30 in the relevant period, the bonus corresponds to a 2¢ increase in the price of wagering \$1.<sup>26</sup> The payment arrived at the end of Survey 3.

We measure perceived self-control problems from valuations of the Bet Less Bonus. Intuitively, these valuations identify perceived self-control problems because agents who perceive themselves to have self-control problems are willing to pay to correct those problems, so they will place higher value on the Bonus. On the other hand, people who do not perceive self-control problems would not want to change future behavior, so they will place lower value on the Bonus.

Since the logic of this identification strategy requires participants to understand how the Bonus affects their future consumption, we took steps to ensure that this was the case. 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 own consumption. Participants made these predictions on an interactive screen. After they entered their predicted consumption, the screen told them how large the bonus would be given the prediction. They could iterate on this predicted consumption-bonus size pair as many times as they liked, which allowed them to transparently see the rate at which consumption reductions mapped into Bonus payouts.

We used this prediction data to help make our elicitation of Bet Less Bonus valuations more natural for participants, following the procedure from Allcott et al. (2022c). As a first step, we asked all participants to make a binary choice between the Bet Less Bonus and an unconditional transfer equal to the Bonus's expected payout given the participant's prediction. Both payments would arrive at the same time (at the end of Survey 3). This binary choice is informative about whether the agent perceives themselves to have self-control problems. The logic is as follows. Both alternatives have the same expected financial value, but if someone chooses the Bet Less Bonus, they actually have to bet less to receive the payment. People without perceived self-control problems will find this consumption distortion costly, which drives them towards the unconditional payment. However, people who perceive themselves to have self-control problems may *value* the reduction in future consumption, which could cause them to choose the Bonus.

We wanted participants to have these considerations in mind during the Bonus valuation elicitation, so we included the following text below the binary choice:

*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].*

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<sup>25</sup>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 cap bonus payments at \$90.

<sup>26</sup>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.

*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.*

After this, we used a multiple price list (MPL) to elicit valuations with more granularity. Participants made a series of binary choices between receiving the Bet Less Bonus and a fixed payment. 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.

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 Bet Less Bonus. The Bonus’s treatment effect on consumption allows us to measure price-responsiveness.

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.<sup>27</sup> 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).<sup>28</sup> 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.17). 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.

#### 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

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<sup>27</sup>See, for example DellaVigna and Malmendier 2006; Kaur et al. 2015; Augenblick et al. 2015.

<sup>28</sup>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.

address overoptimism and voluntary wager limits to address self-control problems.

#### 4.4.1 History Transparency

In the history transparency treatment, participants receive information about their past net returns. The treatment mimics real interventions like DraftKings’s *My Stat Sheet*, which provides similar information about historical activity at a single sportsbook. The idea is that feedback about past net returns will correct errors about future net returns. Interventions that provide information about participants’ own past behavior have been studied in various lab (Möbius et al., 2022) and field (Carrera et al., 2022) settings.

Treated participants 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.

#### 4.4.2 Voluntary Wager Limits

In the limits treatment, participants were prompted to make an active choice about how to use sportsbook limit tools. We focus specifically on weekly wager limits. These tools allow users to set a cap on 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, though there are some implementation differences across books. The limits treatment was designed to understand how participants use these tools.

We presented limit tools to participants as a way to help them control their sports betting activity. We began by eliciting a stated “ideal” weekly wager volume for all participants in Survey 1 with the following question: *Ideally, in the next 30 days, how much would you wager per week on [synced accounts]?* While this was the end of the module for control participants, treated participants then viewed explanatory materials about how weekly wager limits worked. After the explanatory materials, they 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. Then, we asked participants for their planned weekly wager limits on all synced accounts. We gave participants the following reminder as they made their plan: *For reference, you told us earlier that in upcoming weeks, you'd ideally wager a total of \$[ideal] per week on [synced apps].* Finally, we showed the video instructions for 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 primary measure of limit choices.

We study whether participants chose restrictive limits, and whether restrictiveness correlates with our measures of self-control problems.

The intervention can be interpreted as a way to study limit choices after removing hassle costs or other frictions related to limit tools. For context, we highlight that absent our intervention, most bettors do not use limits. 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. Low take-up could arise because limit tools are often placed behind an opaque series of menus. In other contexts, such as Norway (Auer and Griffiths, 2022) and Italy (Calvosa, 2017), players are required to make choices about limits upon account creation. The limits treatment is informative about the impacts of such a regulation in the American market.

#### 4.5 Qualitative Measures of Bias and Other Supplemental Variables

We asked supplemental questions across the three surveys to enrich our analysis. These survey questions allow us to provide transparent qualitative measures that relate to the objects of interest, connect our work to the existing literature, and understand how participants engaged with various aspects of the study.

We collected several qualitative measures of overoptimism and self-control problems. We focus in the body on three measures that connect to the existing literature. The *Gambling Literacy Index*, developed by 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 about financial returns. 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 perceived self-control problems. Appendix A contains the pre-registered questions and definitions for both indices. Finally, we borrow a simple qualitative measure of self-control problems from Allcott et al. (2022b). We asked participants whether they thought they were sports betting "too little," "too much," or "the right amount." Allcott et al. (2022b) ask a similar question for other goods, which allows for convenient a qualitative 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 us with more data on price responsiveness to supplement the

evidence from the randomized Bonus treatment effect. In Survey 3, we asked about the following two scenarios:

- Scenario 1: *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?*
- Scenario 2: *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.

The second kind of hypothetical price change was a change in the Bet Less Bonus's payment rate. We asked participants to predict their own 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 responsiveness analysis.

Finally, we added a question to Survey 3 to measure participants' overall perceived surplus from betting. We elicited participants' willingness to pay to completely stop betting. We asked various other qualitative and open-ended questions, which we will describe as they become relevant.

## 4.6 Preregistration

We pre-registered our experiment at AEA RCT registry (AEARCTR-0013310). We included a pre-analysis plan which specified restrictions on the analysis sample, variable definitions, and regression specifications for estimation of the treatment effects of the Bonus and Limit treatments. We discuss differences between the pre-registered analysis and the analysis in Appendix D.2. We also present estimates from all pre-registered specifications in that section.

# 5 Sample Characteristics

## 5.1 Betting Activity

Figure 4 summarizes the betting activity of our sample in the 30 days before the study began. The top right figure shows the distribution of dollars wagered per week. 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 sports bettors (Forrest and McHale, 2024).<sup>29</sup> While it is challenging to compare . Even within our sample, bet volume has a long right tail, with 25% of our sample wagering more than \$1000 per week in that period. 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 distribution of *average bet riskiness*, which is defined as the average ratio of potential payouts to stakes across bets, weighted by the size of the bets. As an example, a person with average bet riskiness of 2 was on average placing bets that had the chance of doubling their money. Consistent with previous work (Snowberg and Wolfers, 2010), bettors disproportionately choose long shot bets: the median participant has an average bet riskiness of 4.6.

## 5.2 Qualitative Bias Measures

We present qualitative evidence on overoptimism and self-control problems in Figure 3. Overall, the results suggest that both overoptimism and self-control problems exist for some members of the sample, which motivates our quantitative analysis.

The first set of qualitative results suggests that participants are overoptimistic about financial returns. We illustrate these results in the top panel of Figure 3. First, 38% of participants say that one reason they bet on sports is that they win money on average. Unless our participants are extraordinarily skilled, it is unlikely that many of them are in fact able to overcome the hold rate and even consistently break even. We also show that around 20% of participants give concerning responses to each of the questions that make up our Gambling Literacy Index.

The second set of qualitative results suggests that some participants do have self-control problems, but that these are less widespread than in other contexts. The bottom left panel plots the distribution of Problem Gambling Severity Index 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. The bottom right panel shows that a desire to reduce betting is much more prevalent than a desire to increase betting, which suggests that some people perceive themselves to have self-control problems. However, the self-reported demand to bet less, expressed by 20% of the sample, is not as common as self-reported behavioral change in other settings. As a comparison, for a similar question about social media use, over 50% of respondents reported that they used social media too much (Allcott et al., 2022b).

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<sup>29</sup>We draw this conclusion by comprising \$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 £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.

### 5.3 Demographics and Representativeness

We use external data from surveys conducted by Grubbs and Kraus (2023c) (henceforth “GK”) to understand the characteristics of the population of interest. GK asked qualitative bias questions to a Census-matched nationally representative sample as well as an oversample of self-identified sports bettors. We provide further details on the GK surveys in Appendix B.4. Column 1 of Table 2 reports average demographics and qualitative bias measures from the nationally representative sample. Columns 2 and 3 report the same averages for samples that are representative of self-identified weekly lottery bettors and weekly sports bettors, 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, and find that bias estimates in the sample are likely conservative estimates of bias in the population. Column 4 of Table 2 shows that participants in our analysis sample are more highly educated and earn more than the GK weekly sports bettors. Crucially, they also score better on the qualitative bias proxies. To understand how nonrepresentativeness affects our estimates, we construct a weighted version of our analysis sample to match GK weekly sports bettors on bias proxies and education. We provide details and reweighted versions of our reduced-form results in Appendix B.4.

## 6 Reduced-form Evidence

In this section, we present empirical evidence that sports bettors exhibit both overoptimism and self-control problems. Sports bettors are also responsive 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 in fact 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.5).<sup>30</sup> In our sample, the difference is so

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<sup>30</sup>The association between high parlay share and misprediction persists even after controlling for other observables

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 that is 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, it is crucial to allow for rich heterogeneity in overoptimism 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 with noise. Noise arises both from the intrinsic randomness of gambling outcomes and from mean-zero elicitation noise in the prediction question (Kahneman, 1965; Gillen et al., 2019). To recover estimates of the underlying overoptimism parameter for each participant, we apply a shrinkage procedure (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. We provide details on our procedure in Appendix B.2.

We estimate a great deal of heterogeneity in overoptimism at the individual level. Panel B of Figure 6 shows the distribution of individual level estimates. The estimates imply that 9% of participants *underestimate* their expected returns, and 10% of participants overestimate their expected returns by more than 20¢/dollar. Optimally, corrective policy would clearly treat these two kinds of bettors differently, which suggests targeted policies could deliver large efficiency gains in this context.

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 people consistently overestimate all net returns. We find instead that overestimation is local to predictions about the future: when we ask people to recall past net returns, they actually *underestimate* their winnings (Appendix Figure B.1).<sup>31</sup> 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.6). 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

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(Appendix Table B.2).

<sup>31</sup>The finding that people do not overestimate past net returns is consistent with previous work by gambling studies scholars (Braverman et al., 2014).

the literature. First, in this sample of highly experienced bettors, correct average perceptions of the past coexists with overoptimism about the future. 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 that parlays have 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 peoples’ perceptions of their own self-control problems and their naivete about their own 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 valuations. 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 without perceived self-control problems is just the value of the unconditional transfer minus the consumer surplus loss from the price increase. We use predictions of consumption in the treatment and control conditions to compute these components. On average, participants without perceived self-control problems would value the bonus at \$15.72.<sup>32</sup>

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<sup>32</sup>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.

The approach as written here also 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

The left panel of Figure 7 shows that the participants valued the bonus more than this benchmark. The average observed valuation was \$17.58, which exceeds the benchmark by \$1.86 (12%). We refer to this difference as the *behavior change premium* (or BCP), because it represents the amount of money that 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 pay  $\frac{1.86}{274} = \$0.007$  for the 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).<sup>33</sup> We provide a formal argument in Appendix 3.<sup>34</sup>

We show in Panel B of Figure 7 that the BCP per dollar wagered is larger for agents who self-report a desire to reduce future betting consumption. That the BCP per dollar wagered correlates 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).<sup>35</sup> Figure 8 shows that participants do not underestimate future consumption. In

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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  $\left(\frac{x_i^C + x_i^B}{2} - (B - 4500)\right) \cdot 0.02$ .

<sup>33</sup>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 overvalue 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.

<sup>34</sup>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 over experimental earnings directly, and find that the average participant is risk-averse (Appendix Figure B.7), 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.8). We conclude that deviations from risk neutrality had at most a minor impact on our estimates.

<sup>35</sup>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 offsetting bias must drive a wedge between predicted and true future consumption, and we assume that

fact, they slightly *overestimate* future dollars wagered on both survey 1 and survey 2. We interpret this lack of underestimation as evidence against naivete. An implication of this result is 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 ( $\gamma^{SC}$ ).

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 over time during the period from March to June.<sup>36</sup> 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.<sup>37</sup> Numerical predictions also reflect this understanding: predicted changes in consumption are strongly correlated with observed changes in consumption across participants (Appendix Figure B.11). 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.10).

**Summary** Taken together, our results imply that  $E_i[\gamma_i^{SC}] = E_i[\tilde{\gamma}_i^{SC}] = 0.7\text{¢}$  per dollar wagered. The first equality, showing that perceived self-control problems and true self-control problems coincide, comes from 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, are only effective 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.

Our first set of results are 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 and a discussion of the margins of adjustment in Appendix B.3.

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overoptimism appears identically in both the predicted future utility function and the true decision utility function.

<sup>36</sup>Betting activity does fall during the study period for our sample, which is consistent with trends in aggregate betting activity.

<sup>37</sup>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.”

It was not the case that treated participants simply substituted from legal sports betting to other kinds of gambling, as we show using self-reported consumption data (Appendix Figure B.12). 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.<sup>38</sup> These substitution results come with two important caveats. First, the estimates captures short-run substitution effects, and long-run substitution effects could be larger. Second, the dependent variable self-reported consumption, which may understate true consumption, particularly for illegal gambling.<sup>39</sup>

Our second set of results about predicted responses to various hypothetical price changes. The 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 a number of 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.11), 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* responsiveness 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 which made its expected impact on consumption salient (for example, by calling it the “Bet Less Bonus”). As described in Section 4.3.2, 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

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<sup>38</sup>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 disadvantage legal sports betting operations.” <https://www.americangaming.org/policies/hot-issue-sports-betting/>

<sup>39</sup>Of course, nearly every other study of illegal gambling is also vulnerable to this second issue.

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.13). 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 robustness of our policy conclusions to alternative choices in Section 8.

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, and that conditional on the wager volume subgroup, high overoptimism is associated with larger responses. 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 that instrument. The error bars in Figure 9 are reasonably tight given our modest sample size, which highlights an important benefit of using hypothetical price-responses: they improve our power to detect heterogeneous price responsiveness by letting us observe choices under different price levels within each subject.

## 7 Model Estimation

In this section, use the empirical results from Section 6 to estimate a specialized version of our the biased consumption model from Section 3. The main specialization is a constant semielasticity of demand functional form.<sup>40</sup> To impose constant semielasticity, we assume that the functional form of nonfinancial utility is  $z_i(x) = z_{1i}x \log(x) + z_{2i}x$ .

The demand curve is defined as follows. Let  $\tau_0$  denote a status quo price of betting.<sup>41</sup> Let  $x_i^*(0)$  denote the normative consumption at this price. We consider consumption as a function of bias and other unit prices  $\tau_i$ . We show in Appendix Section C that, under our functional form assumption, demand is characterized by the following equation:

$$\log(x^{choice}(\tau_i)) = \log\left(\underbrace{x_i^*(0)}_{\text{Normative consumption at status quo}}\right) - \underbrace{\eta_i}_{\text{Semielasticity}} \cdot \underbrace{(\tau_i - \gamma_i^O - \gamma_i^{SC}) - \tau^0}_{\text{As-if price change}} \quad (4)$$

Equation 4 illustrates why it was important to experimentally measure bias in units of money

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<sup>40</sup>This functional form is motivated by the shape of the predicted consumption demand curve (Appendix Figure B.15).

<sup>41</sup>The status quo price  $\tau_0$  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  $F_A, \tau_0$  is equivalent to the model with  $F_B, \tau_0 + t$ . Therefore, the choice of  $\tau_0$  does not affect the estimation of any demand parameters.

per dollar wagered. When  $\gamma^M O_i$  and  $\gamma_i^{SC}$  are measured in these units, they enter exactly as a price change does. Intuitively, the equation says that someone with bias parameter  $\gamma_i$  will wager as if the price of betting were  $\gamma_i \epsilon$  per dollar lower than it really is.

For estimation, we make two modifications to equation 4. First, since we are estimating the model on panel data, we impose structure on log normative consumption:  $\log(x^*(0)_{it}) = \xi_i + \delta_t + \varepsilon_{it}$ , normalizing  $\delta_{-1} = 0$ . This equation says that all residual idiosyncratic variation in consumption is driven by normative demand shocks  $\varepsilon_{it}$ , which highlights our assumption that bias is constant over time. The second modification is necessary because our choice data contain zeros, which equation 4 cannot rationalize. To allow for zeros, we interpret observed betting in a given period as a realization of a process where opportunities to bet arrive stochastically during the period. Mechanically, we exponentiate both sides of equation 4 and take an expectation to arrive at our estimating equation:

$$E[x_{it}^{choice}(\tau_0)] = \exp \left( \underbrace{\xi_i + \delta_t}_{\log(x_{it}^*(0))} + \eta_i \cdot (\tau_i - \tau_0 - (\gamma_i^O + \gamma_i^{SC})) \right) \quad (5)$$

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  $\gamma_i^O$ , self-control problems  $\gamma_i^{SC}$ , and semielasticity  $\eta_i$ , respectively. Given these estimates, we then compute residualized consumption  $\tilde{x}_{it}^{choice} = x_{it}^{choice} / \exp(\eta_i \cdot (\tau_i - \tau_0 - (\gamma_i^O + \gamma_i^{SC})))$ , and conduct a poisson regression of residualized consumption on a time and individual fixed effects to recover  $\xi_i$  and  $\delta_t$ .

## 8 Counterfactual Policy Analysis

We use our estimated model to evaluate various policy interventions. Uniform corrective taxes improve welfare, but do not achieve the first-best allocation, because of heterogeneous bias. Targeted policies that differentially increase the prices of parlays or directly reduce biases therefore improve relative to taxes alone.

### 8.1 Implementation

We consider a general set of policies that can change prices and quantities from the status quo policy  $(\tau_i^{old}, x_i^{old})$  to  $(\tau_i^{new}, x_i^{new})$  for each consumer. Our welfare criterion is the change in weighted consumer surplus plus government revenues relative to the status quo.

$$\Delta TS_t(\tau^{new}, x^{new}) = \sum_i (\Delta CS_{it}(\tau^{new}, x^{new})) + \lambda \Delta G_{it}(\tau^{new}, x^{new}) \quad (6)$$

The weight on revenues  $\lambda$  encodes the planner's efficiency and distributional concerns with regard to the use of public funds. It will be higher if the planner thinks that funds are spent efficiently and if the planner thinks that they are directed towards groups with higher welfare weights than sports bettors.

For exposition, we decompose the change in consumer surplus into two components: the change in *revealed-preference consumer surplus*  $\Delta CS_{it}^{\text{revealed}}$ , which is consumer welfare evaluated according to biased preferences, and the *internality benefits*  $\Delta I_{it}$ , which is the reduction in uninternalized costs borne by consumers.<sup>42</sup>

We set  $\tau_0 = 2.02\text{¢}/\$$  to be the status quo tax per Figure 2. For interpretability, we fix  $\delta_t = \delta_{-1}$  for our results, so all results are in units of dollars per participant during the period from Feb 8 to March 9, 2024.

## 8.2 First-best policy

Under a first-best policy, bettors would consume as if they had internalized all costs of betting. One example of such a policy would be an individual-specific tax equal to the total price-metric bias:  $\tau_i^{\text{firstbest}} = \gamma_i$ ;  $x_i^{\text{firstbest}} = x_i^{\text{choice}}(\tau_i^{\text{firstbest}})$ . Such a scheme is of course not implementable, since the planner does not observe each agent's  $\gamma_i$ , so we treat the welfare impacts of the first-best policy as an upper bound for the potential gains from corrective policies.

The first-best policy increased welfare by \$16.08 per consumer-week in the period of interest. Figure 11 summarizes the effects of our policies on surplus components. Interestingly, after accounting for internality benefits, the personalized tax both improves consumer surplus and raises revenues.

It is challenging to aggregate the benefits for experimental participants to benefits in the market more broadly. We take a back of the envelope approach which assumes that policy benefit per dollar wagered for the average experimental participant in this period is equal to the average benefit per dollar for all U.S. sports bettors. 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 given the 2023 level of wager demand.

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<sup>42</sup>Revealed consumer surplus is the area between the (bias-inclusive) demand curve and the market price from 0 to  $x_i$ . Formally,  $\Delta CS_{it}^{\text{revealed}}(\tau^{\text{new}}, x^{\text{new}}) = CS_{it}^{\text{revealed}}(x^{\text{new}}, \tau^{\text{new}} - \tau_0) - CS_{it}^{\text{revealed}}(x^{\text{old}}, \tau^{\text{old}} - \tau_0)$ , where  $CS_{it}^{\text{revealed}}$  is

$$CS_i^{\text{revealed}}(x_i, \tau - \tau_0) = \int_0^{x_i} \left( \frac{\xi_i - \log(x)}{-\alpha_i} + (\gamma_i^M + \gamma_i^{SC}) - (\tau - \tau_0) \right) dx \quad (7)$$

Internality benefits are  $\Delta I_{it}(\tau^{\text{new}}, x^{\text{new}}) = (x_i^{\text{new}} - x_i^{\text{old}}) \cdot (\gamma_i^M + \gamma_i^{SC})$ .

### 8.3 Taxes

Our estimates imply that the optimal uniform corrective tax on sports betting is 5.17%. This tax is more than twice as large as the average status quo rate.<sup>43</sup> Therefore, our estimates suggest that higher taxes on sports betting would increase total surplus. Our estimate is substantially lower than the raw average bias  $\gamma_i$  because the bettors with the largest demand responses (high-volume bettors) have smaller biases. We highlight that this result depends only on our bias estimates and on the heterogeneity of bias with respect to the semielasticity of demand; it does not depend on the *aggregate* semielasticity of demand.

Moving beyond pure corrective taxation, we illustrate in Figure 12 how the optimal tax depends on the weight on revenues. 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).<sup>44</sup> 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). 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.

The optimal uniform tax increases total surplus by \$8.3 per consumer week (Figure 11), which is only half as large as the first-best surplus gain. The problem is that the uniform tax cannot differentially reduce the betting of the most biased users while allowing unbiased users to continue betting. Motivated by this observation, we turn to the analysis of policies that address overconsumption by directly reducing bias.

Our estimates of the optimal corrective tax rate and of the welfare benefits of an optimal tax are sensitive to estimation approaches, but the conclusion that the optimal rate is higher than the status quo rate is robust. We show results for a variety of alternative estimates in Table 3.

Table 3 clarifies that allowing for heterogeneity in bias reduces the optimal tax rate estimate. The dependence arises because high-volume bettors are less biased than low-volume bettors. Theoretically, the optimal corrective tax is a weighted average of individual-level biases, with weights proportional to size of consumption response to a tax change (i.e, the negative slope of the demand curve). Under homogeneous bias, weights are irrelevant and the optimal corrective tax rate is equal to the bias, which is 9.12% (row (2) of Table 3). In our more general model, the impact

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<sup>43</sup>It is, however, close to the combination of federal taxes plus state taxes in U.S. jurisdictions with the highest tax rates. New York State levies a 51% tax on sports betting revenues, which amounts to a 4.45% tax on dollars wagered (Figure 2). Adding the federal excise tax comes out to 4.7%, which is not far from our estimate.

<sup>44</sup>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.

of weights depends on the correlation between demand slopes and bias. This correlation of interest is primarily driven by the relationship between consumption levels and overoptimism – consumption levels drive most of the variation in demand slopes, and overoptimism is by far the dominant bias in our model. In practice, high-volume bettors are less overoptimistic (Figure 6), which means that the bettors with the largest consumption responses are less biased. Accounting for this pattern yields the smaller optimal tax implied by our preferred estimates, 5.17% (row (1) of Table 3).

The optimal corrective tax is less sensitive to estimation strategies for the semielasticity  $\eta$ .<sup>45</sup> Specifications with heterogeneous  $\eta$  imply slightly higher taxes than specifications with homogeneous  $\eta$ , since overoptimistic bettors are more responsive to price changes than under-optimistic bettors. We observe this pattern across all identification strategies (Appendix Figure B.14). However, Table 3 shows that these differences do not affect the optimal tax rate much. Across the three different identification strategies described in Section 6.3 and regardless of whether we allow for heterogeneous  $\eta$ , the optimal tax rate remains in the relatively narrow range [4.63%, 5.17%]. Different identification strategies do imply different *consequences* of corrective taxation, both positive and normative. Estimates from the bonus effect strategy imply larger consumption responses and also larger total surplus gains from optimal taxation. This result arises because taxation can correct more deadweight loss when choices are more responsive to prices, and we estimate more responsiveness with the bonus effect strategy.

## 8.4 Simulated Bias-correction Interventions

Aside from taxation, policymakers have considered using behavioral bias-correction interventions to reduce overconsumption. In theory, bias-correction interventions are attractive because their effects could be exceptionally well-targeted (Camerer et al., 2003; Thaler and Sunstein, 2003; Allcott et al., 2022a; List et al., 2023). Unlike targeted tax schemes, which can at best target a proxy for bias, these interventions target bias directly.

We illustrate the power of bias-correction by evaluating hypothetical perfectly targeted bias correction intervention. Specifically, we consider a policy that causes consumers to choose  $x_i^{BiasCorrection}$  as if bias  $k \in \{M, SC\}$  was equal to  $\gamma_i^k = (1 - \omega^k)\gamma_i^k$ .  $\omega^k$  parameterizes the strength of the intervention for the bias of interest. In Figure 11, we illustrate the effects of policies that reduce bias by 20%:  $\omega^M = 0.2, \omega^{SC} = 0.2$ . Perhaps surprisingly, even a modest intervention correcting overoptimism delivers more total surplus than the optimal uniform tax. Correcting self-control problems is less valuable, since that is a smaller bias. The distributional consequences also differ. With bias-correction, benefits accrue to consumers through internality reductions, while the government loses money because the status quo gambling tax collects less revenue.<sup>46</sup>

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<sup>45</sup>Of course, the optimal tax depends critically on the semielasticity when revenue-raising motives become relevant, for the usual reasons first described by Ramsey (1927).

<sup>46</sup>We assume away any direct welfare benefits or costs of the intervention of the kind described in Glaeser (2006);

Many sportsbooks have promoted bias-correction interventions, which might seem surprising given their financial motive to encourage consumption. Figure 14 provides a potential explanation. We plot the total surplus against aggregate betting consumption for various levels of both the overoptimism reduction intervention and a tax. The plot shows that a given gain in total surplus can be achieved with much less of an impact on aggregate betting consumption when it is achieved via bias-correction rather than through taxation. In particular, while the optimal tax would reduce consumption by more than 30% relative to the status quo, the same gains could be delivered via bias-correction with a less than 10% reduction in consumption.

By construction, these hypothetical interventions are perfectly targeted at bias. Therefore, we interpret the results in Figures 11 and 14 as a best-case scenario for real-world bias-correction interventions. Overall, the results suggest that such interventions have tremendous potential. In practice, of course, bias-correction can be challenging (Ambuehl et al., 2022b; Allcott et al., 2022a). In Section 9, we study whether bias-correction interventions as implemented in practice actually deliver on this potential.

## 8.5 Bans

A key theme of our analysis so far has been 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 responsiveness 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 betting entirely for a 30-day period. We elicited this WTA in Survey 3 as described in Section 4.5 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 13 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 elicitations 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.<sup>47</sup> Also, while 30 days is not a short time, the monthly cost of giving up gambling for a single month may differ from the monthly cost of giving up gambling forever.<sup>48</sup> 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.

## 9 Experimental Evidence on Bias-correction Interventions

We illustrated in the previous section that, in principle, targeted bias-correction interventions could improve welfare relative to taxes alone. However, it is challenging to create interventions that actually 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. While beliefs responded to information, the treatment did not reduce bias on average. The lack of bias-correction arose because people did not on average underestimate past winnings and mistakes about the past were not correlated with mistakes about the future.

Information about past returns impacted predictions in the expected direction. Panel A of Figure 15 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.

While this result shows that the intervention affected beliefs, the key question is whether intervention affected beliefs *in a way that reduced bias*. We provide two results showing that it did

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<sup>47</sup>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.

<sup>48</sup>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.

not. First, we show that the average treatment effect did not counteract average bias. In fact, the treatment *positive* updates on average, though the average update is not distinguishable from zero (Appendix Table B.4). Second, we evaluate the extent to which heterogeneous treatment effects were well-targeted at bias. Even if the intervention has no average effect, it could still be that the intervention reduces overoptimism for the most overoptimistic bettors. We show in Panels B and C of Figure 15 that treatment effects were not well targeted. Bettors with high overoptimism according to two measures (raw prediction errors and shrunk overoptimism estimates) do not update more negatively.

These results are unsurprising given the underlying structure of forward- and backward-looking errors. First, regarding the average treatment effect, recall that people did not underestimate past returns on average (Appendix Figure B.1). Since participants did not receive bad news on average, it is natural that the treatment did not reduce overoptimism on average. Second, for an information intervention to have well-targeted treatment effects, more overoptimistic bettors must receive worse news. Since the information in the history transparency treatment is about whether recollections of the past are mistaken, this situation would occur if those who most over-estimate future returns are the same people who most over-estimate past returns. Panel D of Figure 15 shows that in fact, over-estimation of future returns is not correlated with over-estimation of past returns.

On the whole, these patterns 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 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. The ability to set limits does reduce self-control problems, but only partially, because not all bettors with self-control problems choose to set binding limits.

Voluntary limit tools only partially address self-control problems. To show this, we plot the reduction in betting consumption that participants chose when setting limits alongside participants' ideal consumption reductions in the left panel of Figure 16. The first measure of ideal consumption reduction comes from participants' stated ideal consumption. These stated ideals imply that the average participant would want to reduce future dollars wagered by 21%. Our second measure of ideal consumption reduction comes from a counterfactual where we remove self-control problems in the model, using our average estimates of self-control problems  $\gamma_i^{SC}$  and  $\eta_i$ . Those estimates imply that people would reduce consumption by 8% if they were not subject to self-control problems. These estimates are quantitatively different, but we find that the reduction in consumption implied by limit choices is substantially smaller than either estimate. The average

bettor only sets limits that would reduce consumption by 3%.<sup>49</sup> Even using our 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 is not all bad news. Partial bias correction is better than nothing. Also, unlike history transparency, voluntary limits are well-targeted at bias. The right panel 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 in a world where policymakers set optimal taxes to correct average biases (Allcott et al., 2022a).

Why do 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.17) and that this uncertainty played a role in limit decisions (Appendix Figure B.18). 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.

Overall, we conclude that voluntary wager limit tools are useful, but imperfect, tools to mitigate harms from self-control problems. We highlight that the demand for flexibility may create fundamental issues for commitment devices as bias-correction interventions in this context.

## 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, we only consider two reasons for overconsumption: overoptimism about financial returns and self-control problems. On top of these, there are reasons to believe that sports betting can cause negative externalities. Potential sources of externalities include, but are not limited to: impacts on intimate partner violence (Card and Dahl, 2011; Matsuzawa and Arnesen, 2024), 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 convert the harms into units of damage per dollar wagered, and

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<sup>49</sup>Such low take-up is consistent with qualitative evidence that people do not express interest in using limit tools (Appendix Figure B.16).

add a new component to the money-metric bias term  $\gamma$ . To the extent that these concerns are empirically relevant, optimal policy is more restrictive than implied by our estimates.

Second, our model does not capture 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. Biases in the habit formation process can exacerbate static self-control problems (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 could matter for the tax's local impact. Historically, sportsbooks have set uniform odds across state lines.<sup>50</sup> Under uniform 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 because they cause underconsumption. 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 in other kinds of betting, and the state of Oregon has granted such a monopoly to DraftKings. 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, we assume that consumers perfectly perceive price changes induced by the tax. In practice, the hold rate plays the role of the price, and hold rates are generally opaque, so people may underreact to changes in the hold rate (Enke et al., 2024). In this case, the optimal tax rate is more restrictive than implied by our estimates and the total surplus gains from optimal taxation are smaller (Chetty et al., 2009; Farhi and Gabaix, 2020).

Sixth, 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 or mistakes. In a field experiment with a sample of high-volume sports bettors,

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<sup>50</sup>The practice of setting uniform prices across space consistent with the practice of large U.S. retail chains (DellaVigna and Gentzkow, 2019). While 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>

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-responsiveness, we estimate a model of biased sports betting. Using our preferred estimates, we compute a surplus-maximizing corrective tax of 5.17¢ per dollar wagered. 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.

While this paper has focused on our empirical results about overoptimism and self-control problems, our economic model can be flexibly adapted to accommodate many other considerations as described in Section 10.1. 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 the information and limit treatments highlights the importance of further research on platform design. 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 effectively 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. We encourage sportsbooks, regulators, and researchers to experiment with new interventions, with the goal of creating a product that allows consumers to derive entertainment value while preventing harms from overconsumption.

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## Tables and Figures

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.

Variable	Grubbs and Krauss			Brown, Grasley, and Guido Analysis Sample
	Census Matched	Weekly Lottery Bettors	Weekly Sports Bettors	
N	2806	406	517	444
<b>Demographics</b>				
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)
<b>Qualitative bias measures</b>				
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)

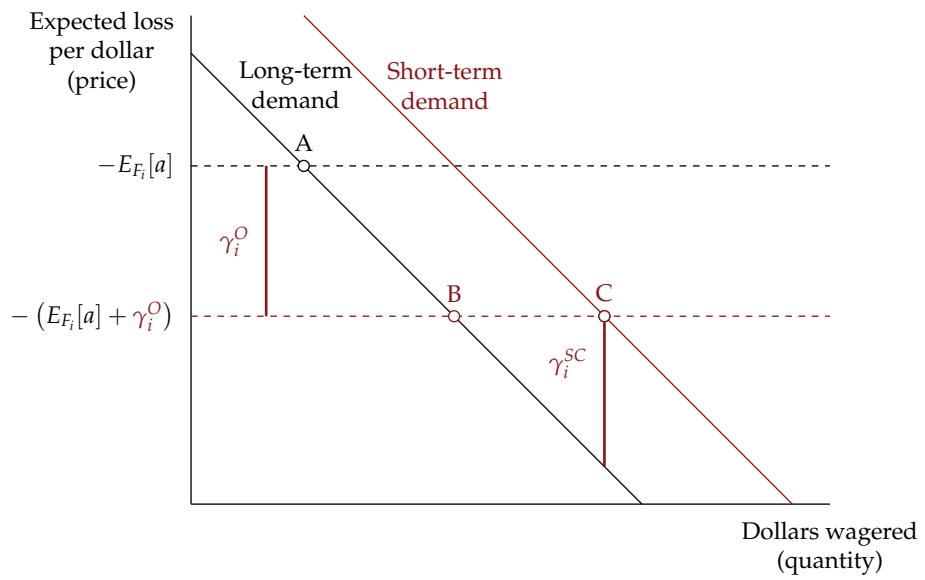
**Table 2:** Sample demographics, qualitative measures of bias, and representativeness

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.

	Specification		Uniform tax		First-best	
	(1)	(2)	(3)	(4)	(5)	(6)
	Identification of $\eta$	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

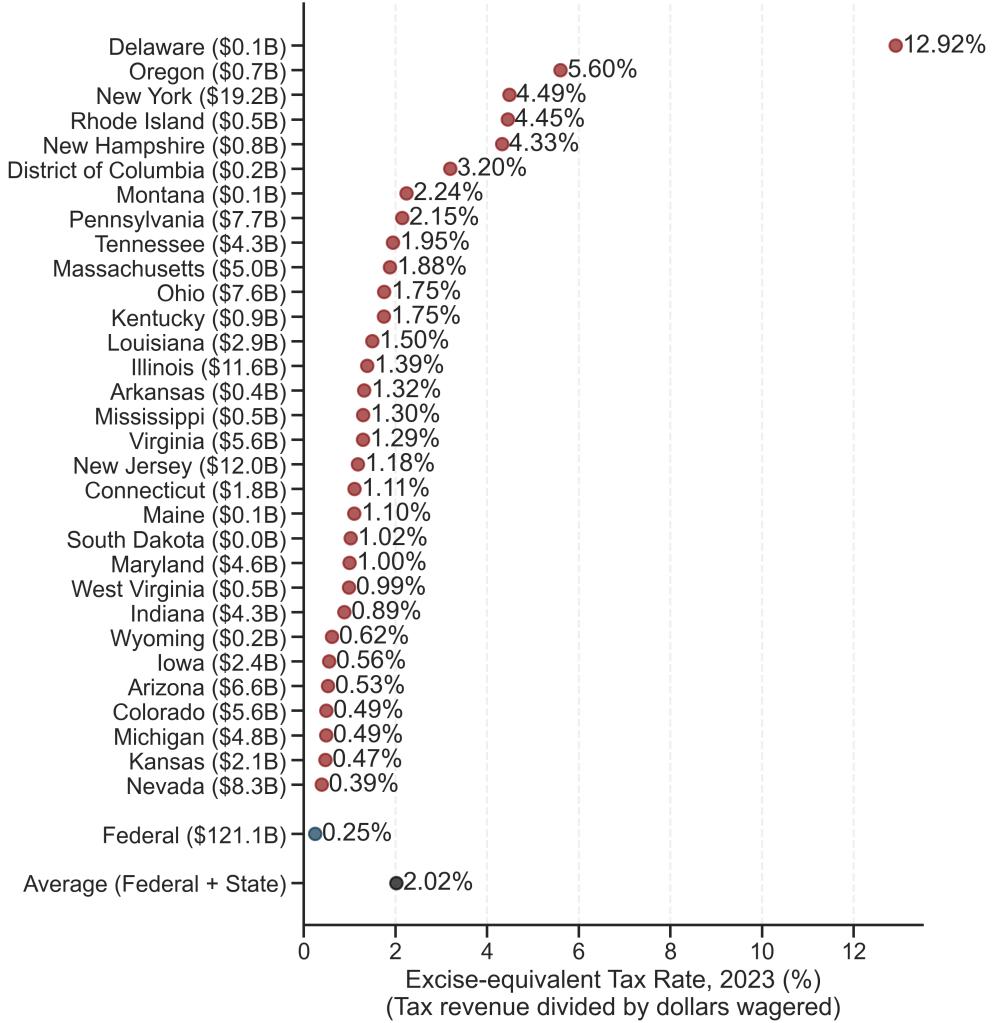
**Table 3:** Sensitivity of corrective taxation analysis to alternative estimation strategies

Notes: This table illustrates how our optimal tax simulations depend on whether we allow for heterogeneous parameters and on our estimation strategy for the semielasticity term  $\eta$ . 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  $\eta$ , using the three approaches outlined in Section 6.3 and visualized in Figure 9. When estimating heterogeneous  $\eta$ , we allow for heterogeneity by above/below median in dollars wagered and misperceptions, as in Figure 10. 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  $\eta$  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.



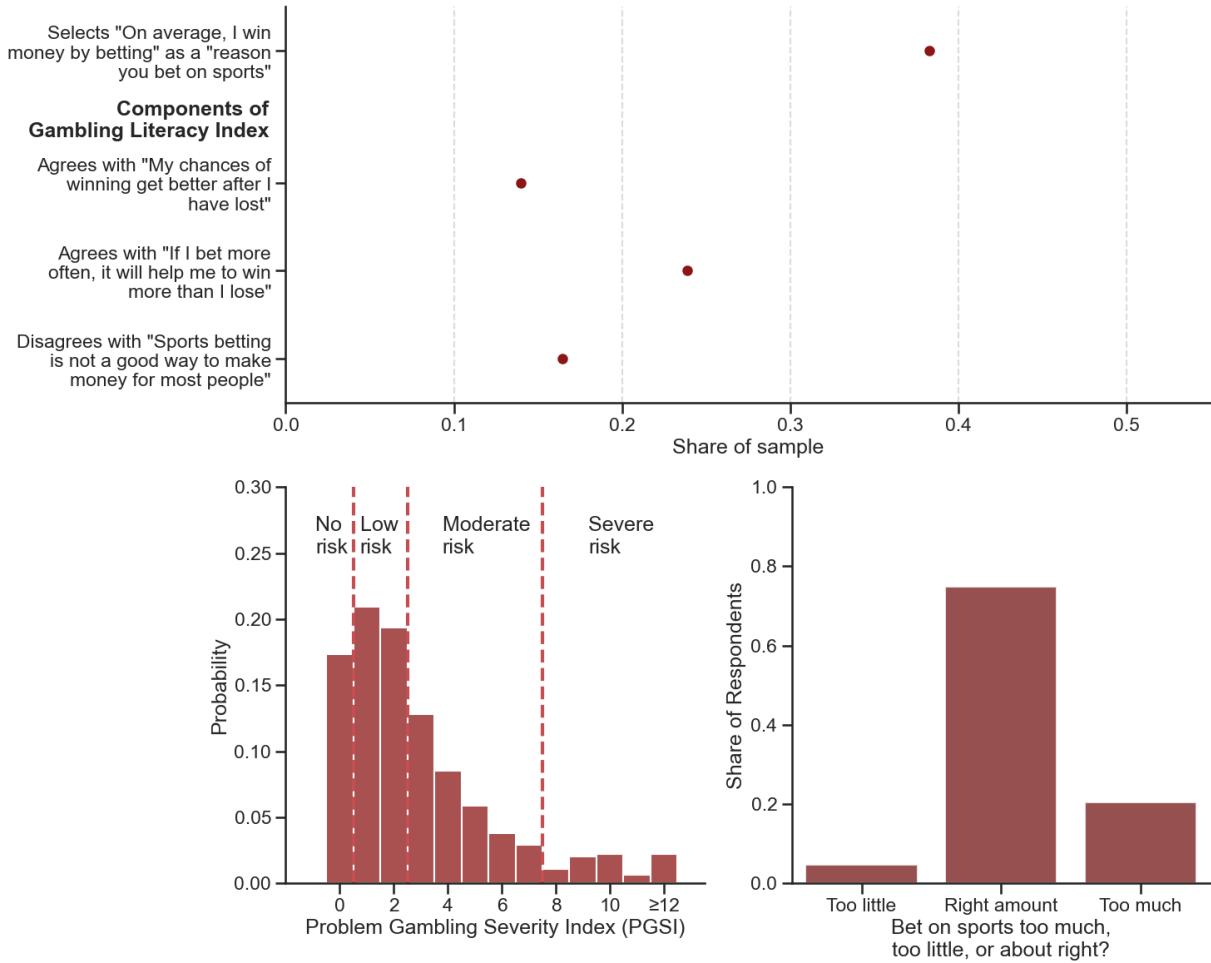
**Figure 1:** Overconfidence and self-control problems as price-metric biases

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.



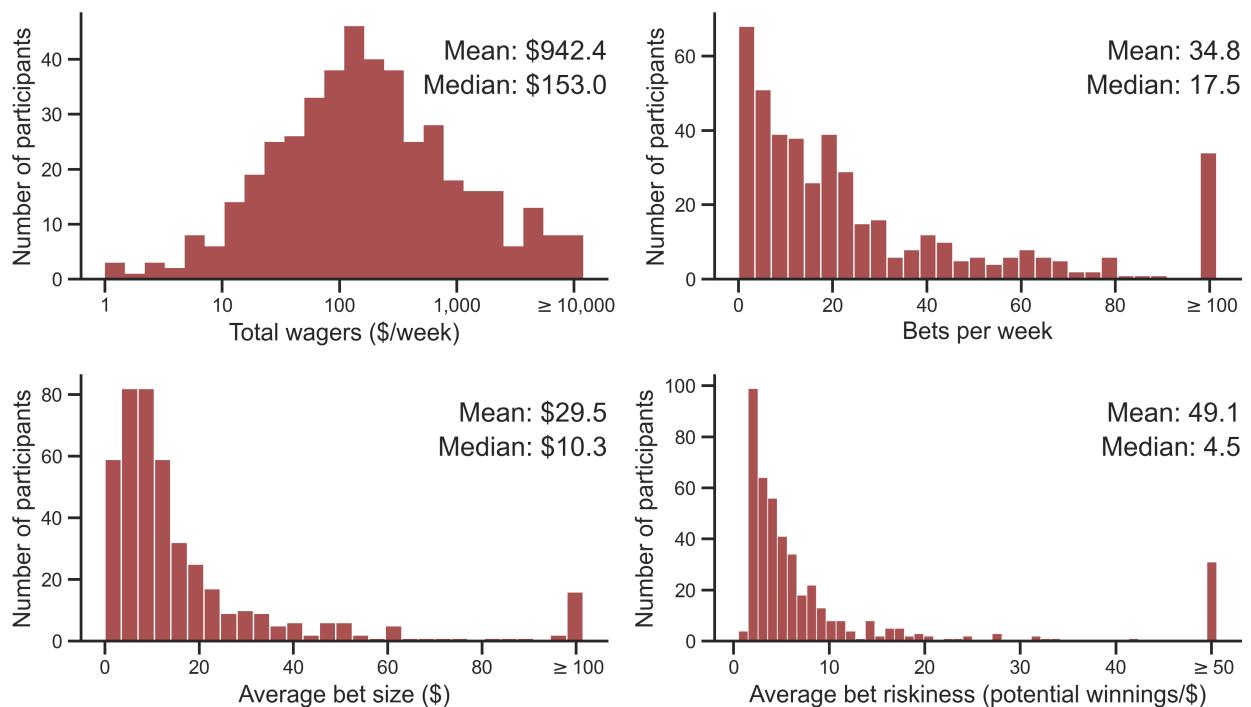
**Figure 2: Summary of sports betting taxes in 2023**

Notes: The table 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 wagered 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).



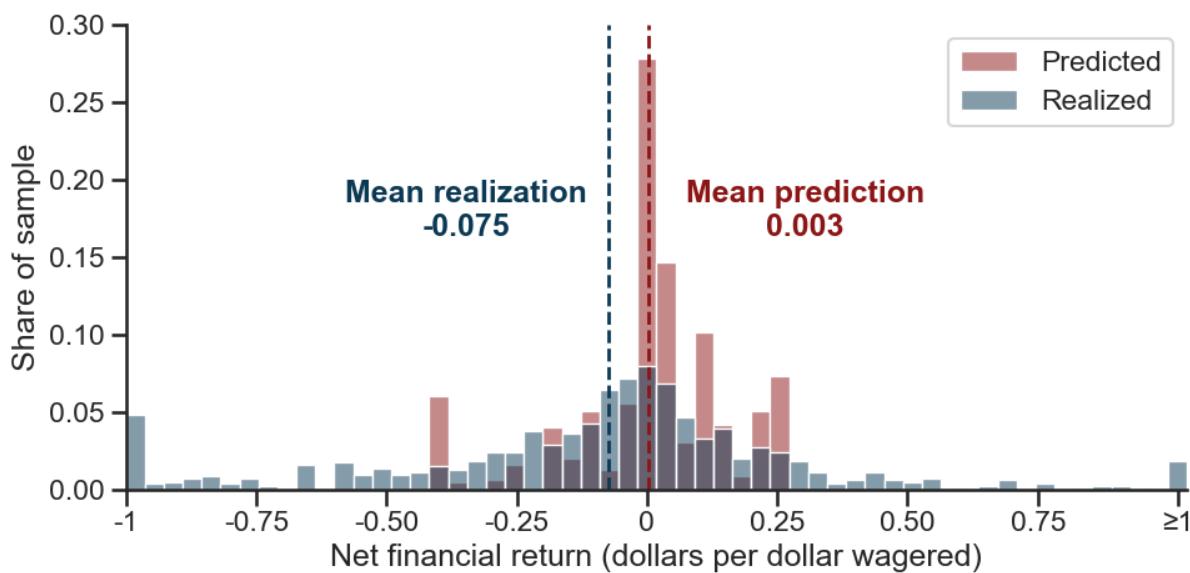
**Figure 3: Qualitative evidence on overoptimism and self-control problems**

Notes: The figure presents qualitative evidence on our two biases of interest. The top panel focuses on beliefs about financial returns. The bottom three measures collectively form the Gambling Literacy Index. The bottom panels focus on self-control problems. The left panel shows the distribution 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. The right panel shows the distribution of responses to the question: *How do you feel about your own sports betting in a typical week?*



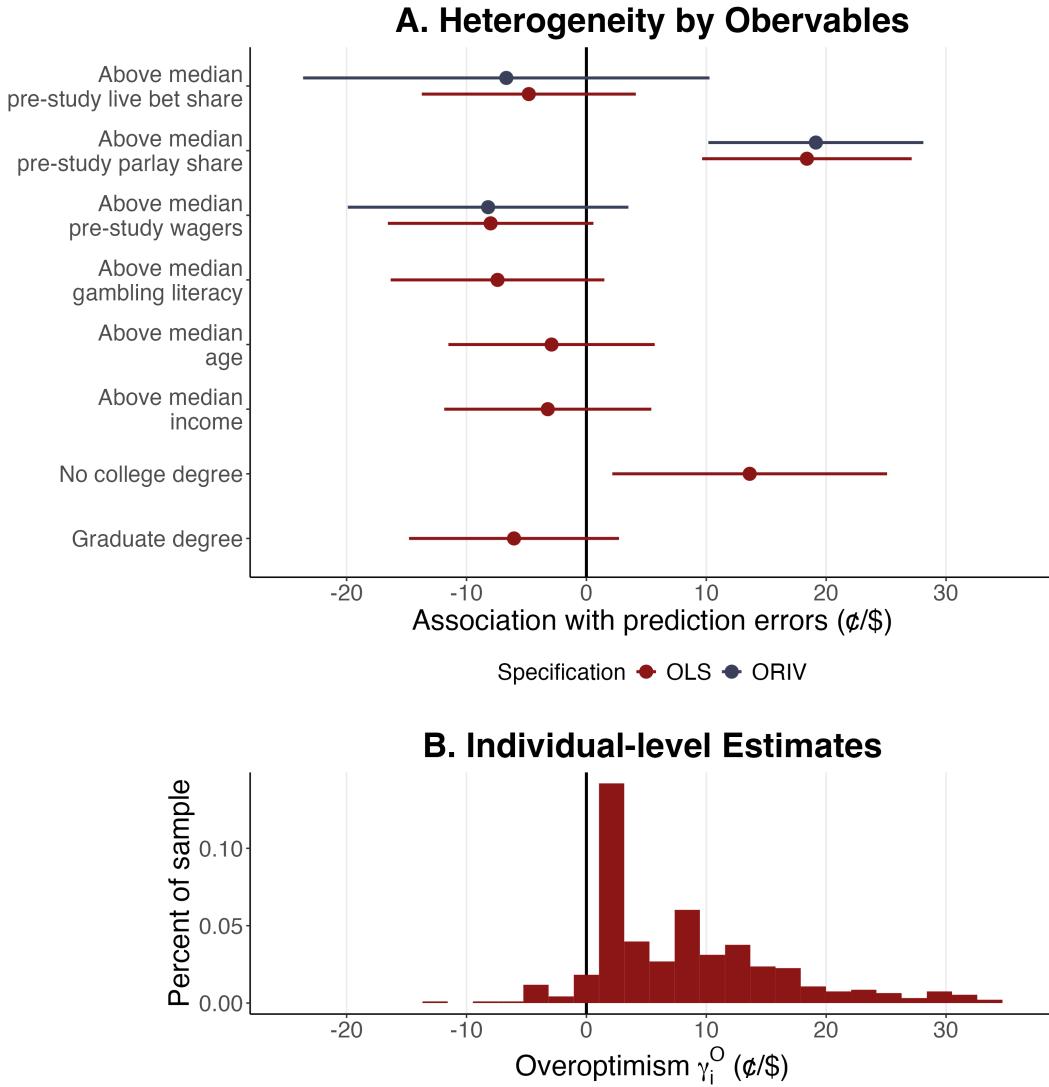
**Figure 4:** Pre-study betting activity accross individuals

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.



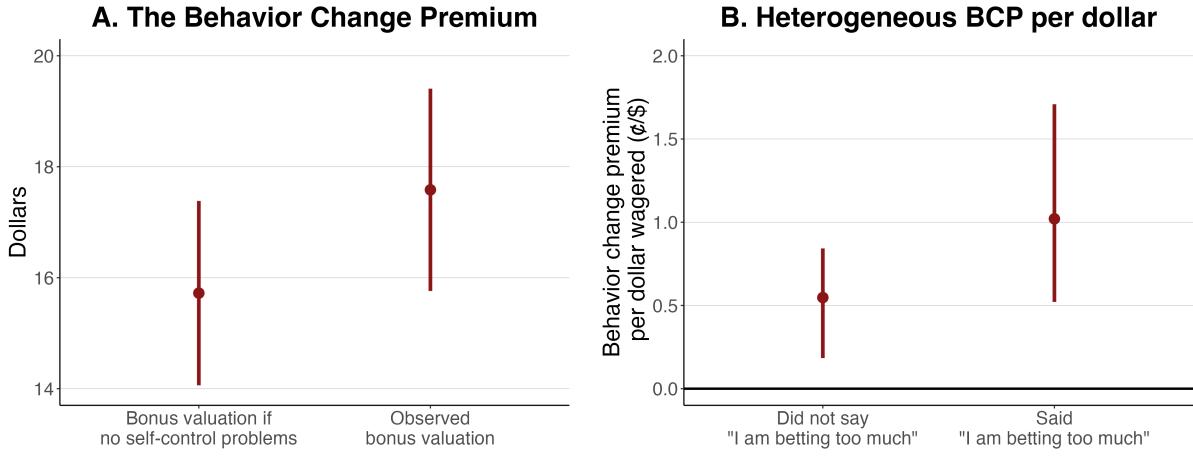
**Figure 5:** Comparing predicted financial returns to realized financial returns

Notes: This figure shows the distribution of predicted and 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.



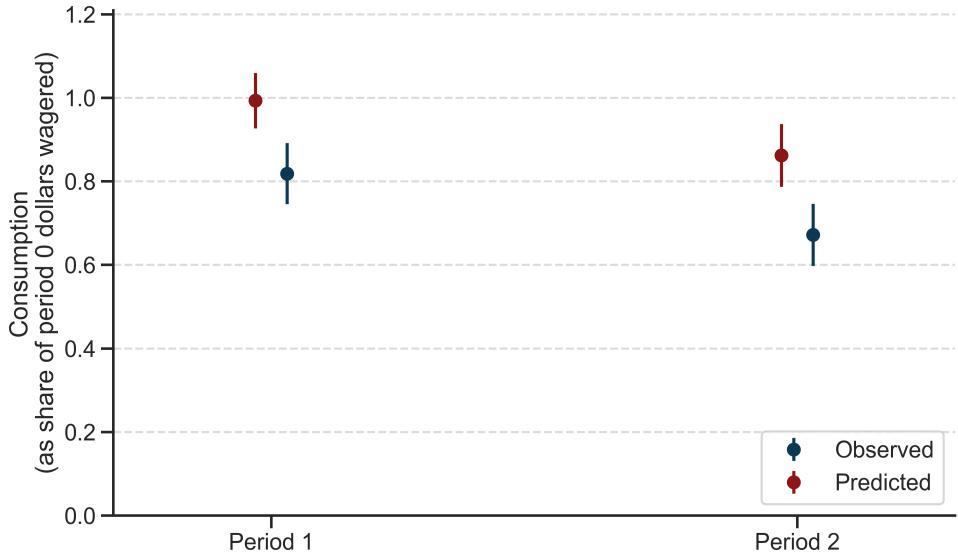
**Figure 6:** Heterogeneity in overoptimism

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.2.



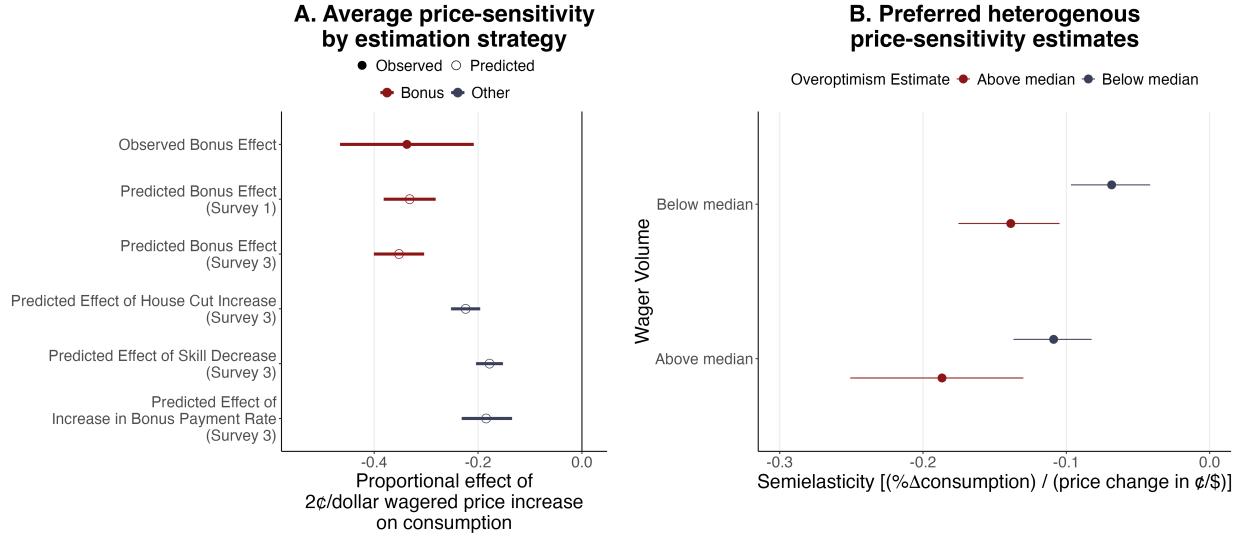
**Figure 7:** Evidence on self-control problems from Bonus valuations

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).



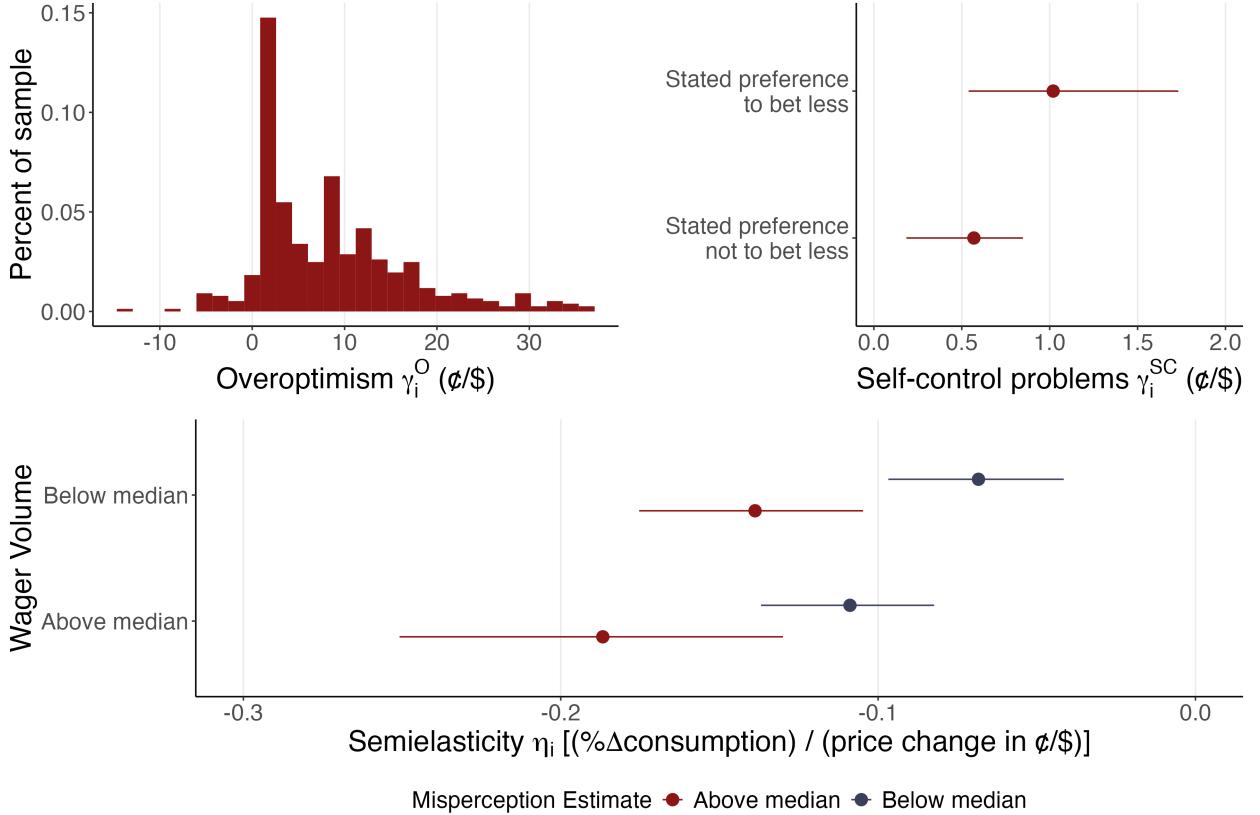
**Figure 8:** Evidence against naivete from predictions of future dollars wagered

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]



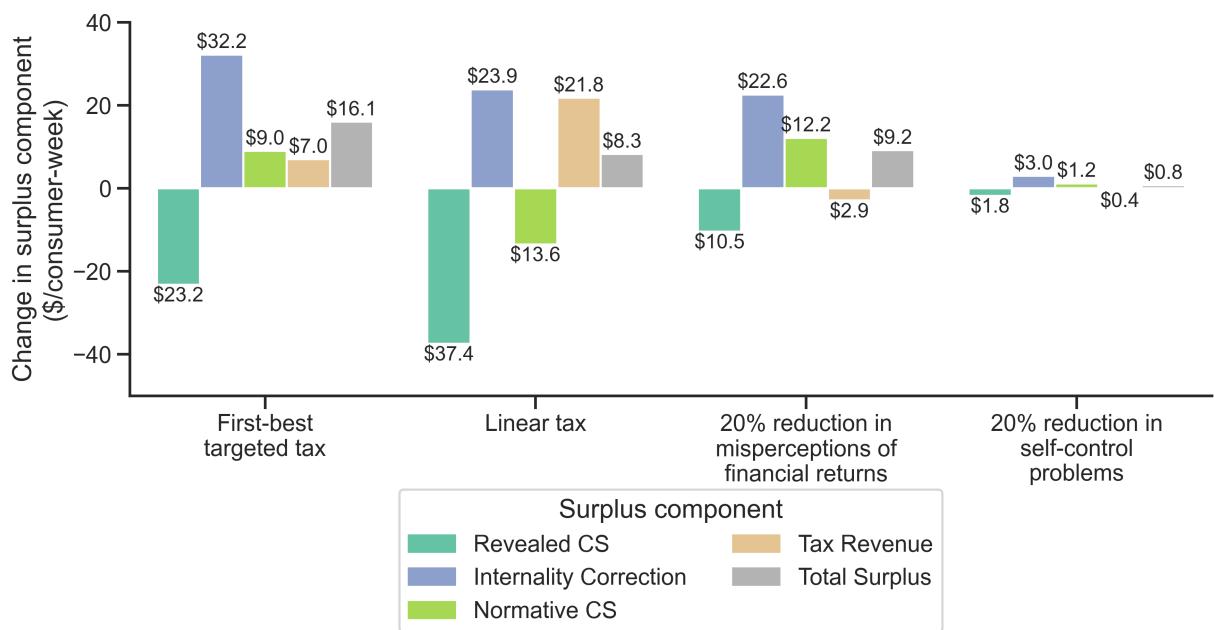
**Figure 9: Evidence on price-sensitivity**

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.4). 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. We estimate the regression  $\log(E_i[x(p)/x(0)]) = a + \beta p$ , where  $p$  is the payment rate of the Bet Less Bonus and  $E_i[x(p)/x(0)]$  is the average proportional reduction in consumption as a result of the payment rate. The estimate  $\hat{\beta}$  is a semielasticity estimate, and we report in the figure the implied effect of a 2¢ price change given this estimate. 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.



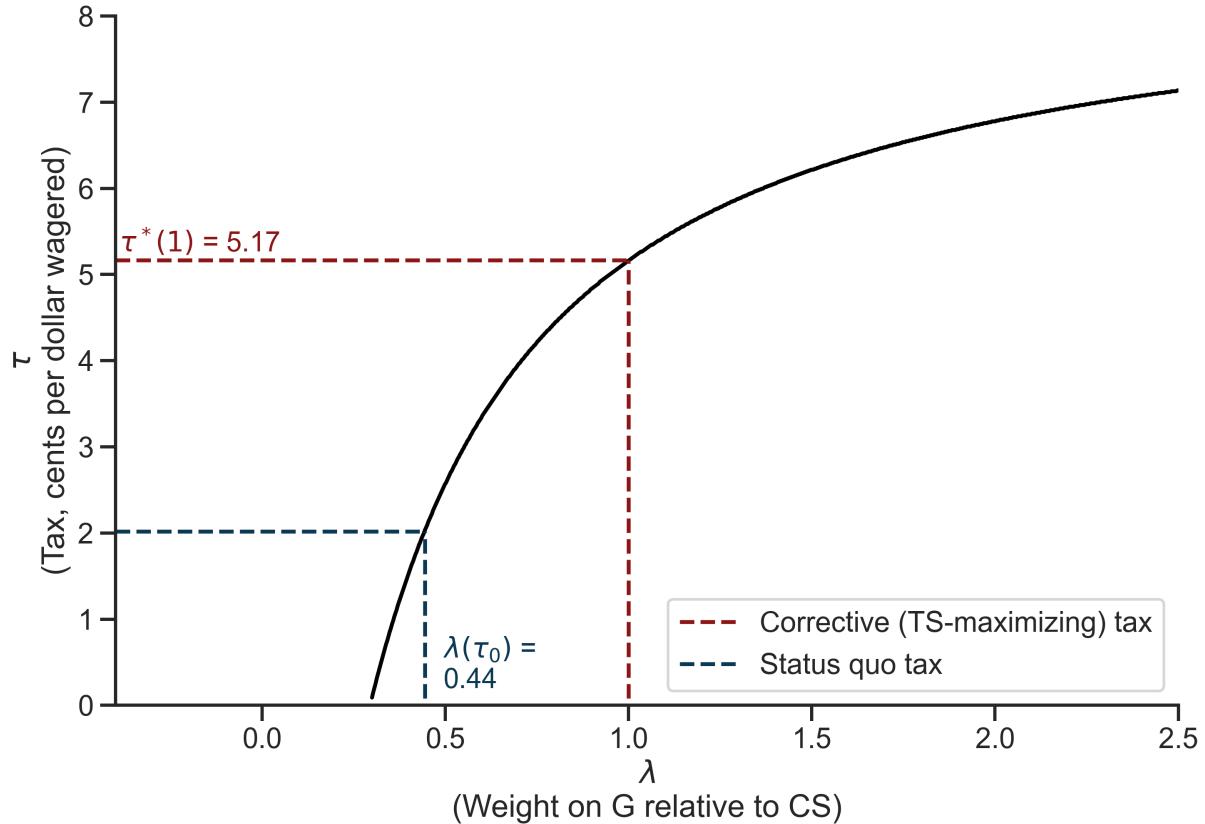
**Figure 10:** Marginal distributions of parameter estimates

Notes: The figure plots parameter estimates from the estimation strategy described in Section 7. The top panels plot marginal distributions of bias parameters  $\gamma_i^M$  and  $\gamma_i^{SC}$ . Error bars represent bootstrapped 95% confidence intervals. The randomized p-value for the difference between the two estimates is  $p = 0.055$ . In the top right panel, participants are split into two groups for estimation of self-control problems: those who say “I am betting too much” and those who do not. The top half of the bottom panel plots estimates of the average semielasticity for four groups, below/above median wager volume and below/above median misperception estimate  $\gamma_i^M$ . Error bars represent analytical 95% confidence intervals.



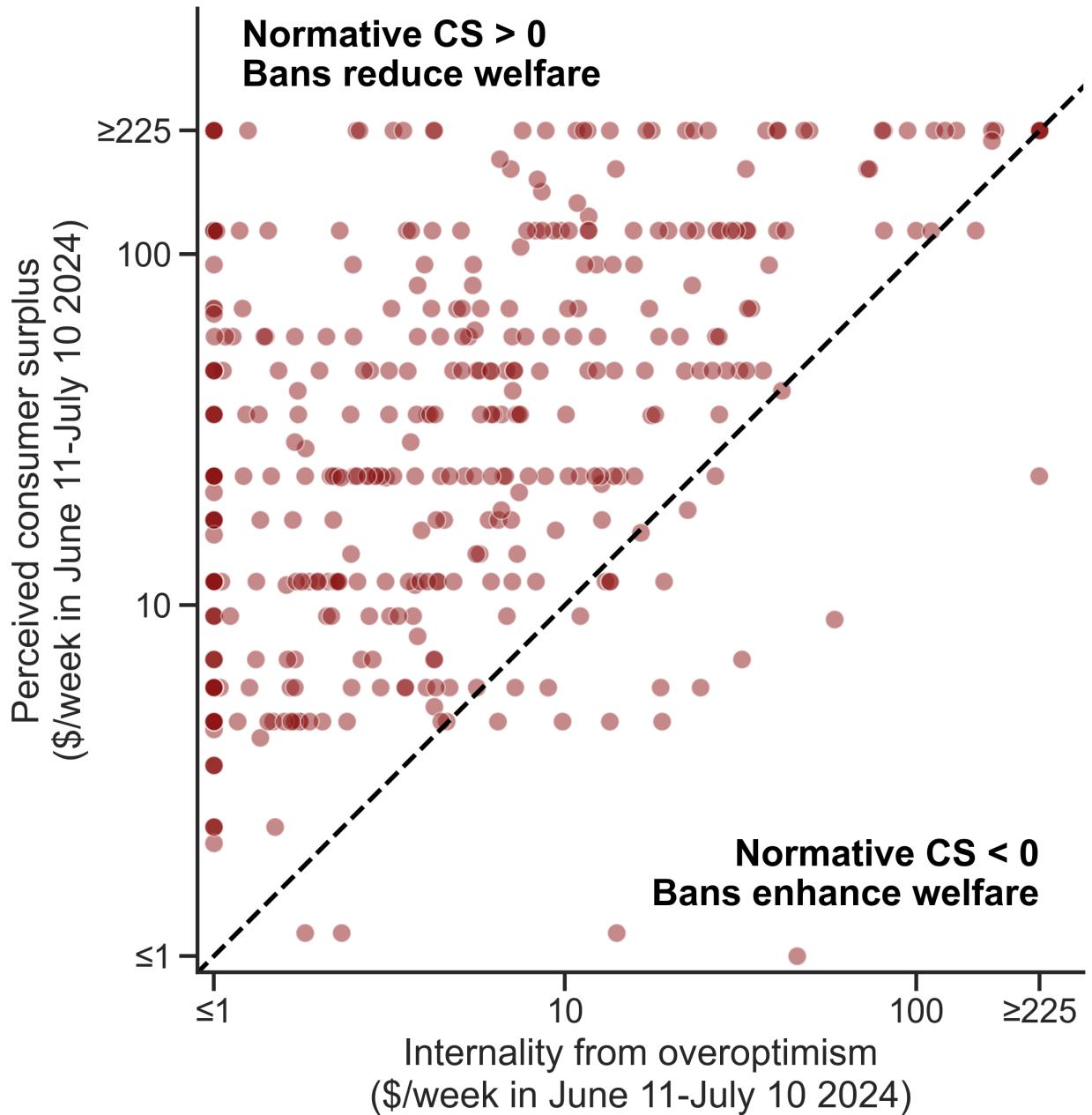
**Figure 11:** Effects of policies on surplus components

Notes: The plots decompose the welfare effects of the policies described in Section 8. The simulation used normative consumption from period  $-1$  (Feb 9-March 7), so welfare calculations apply to that time period. The total surplus bar is the unweighted sum of Normative CS and Revenues.



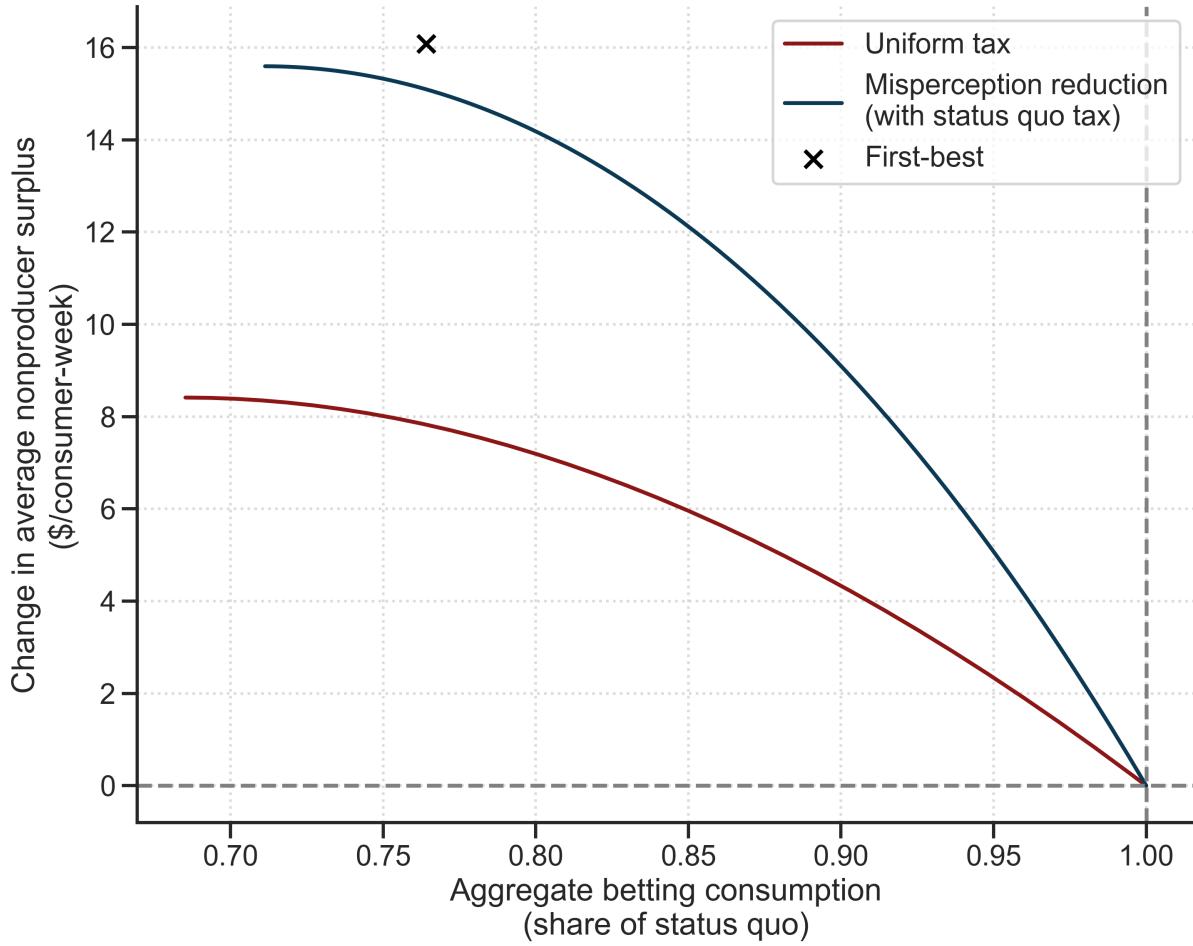
**Figure 12:** Optimal taxes and the weight on government revenues

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  $\lambda$ , the figure gives the optimal tax for that weight  $\tau(\lambda)$ . Second, starting with a tax  $\tau$ , the figure gives the weight that makes the tax optimal  $\lambda(\tau)$ . The red point corresponds to the optimal corrective tax. The blue point shows the weight on revenues implied by status quo policy ( $\tau_0 = 2.02$ ).



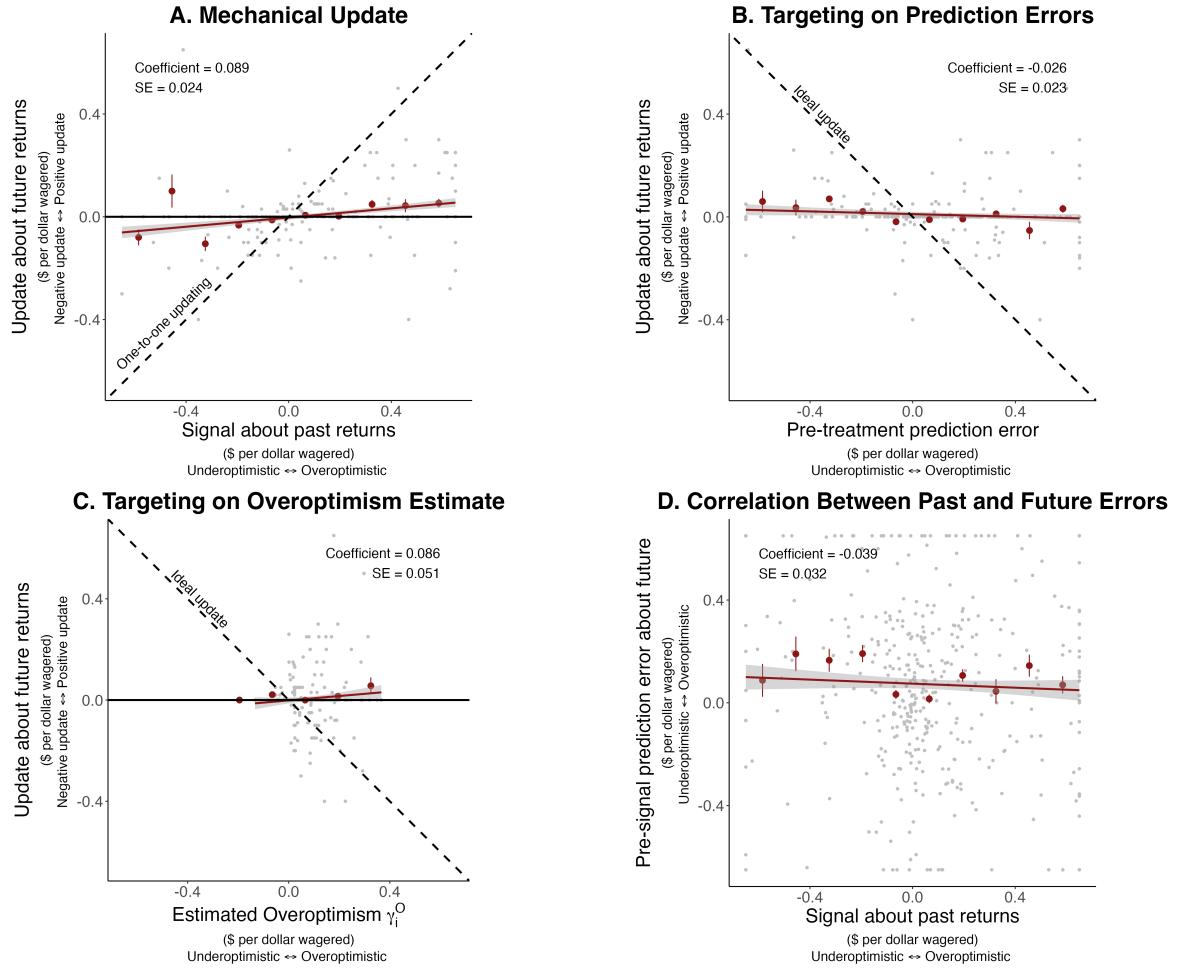
**Figure 13:** Evaluating bans

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  $\gamma_i^M$  by predicted consumption. For both measures, values are reported in units of dollars per week in the 30 days following Survey 3.



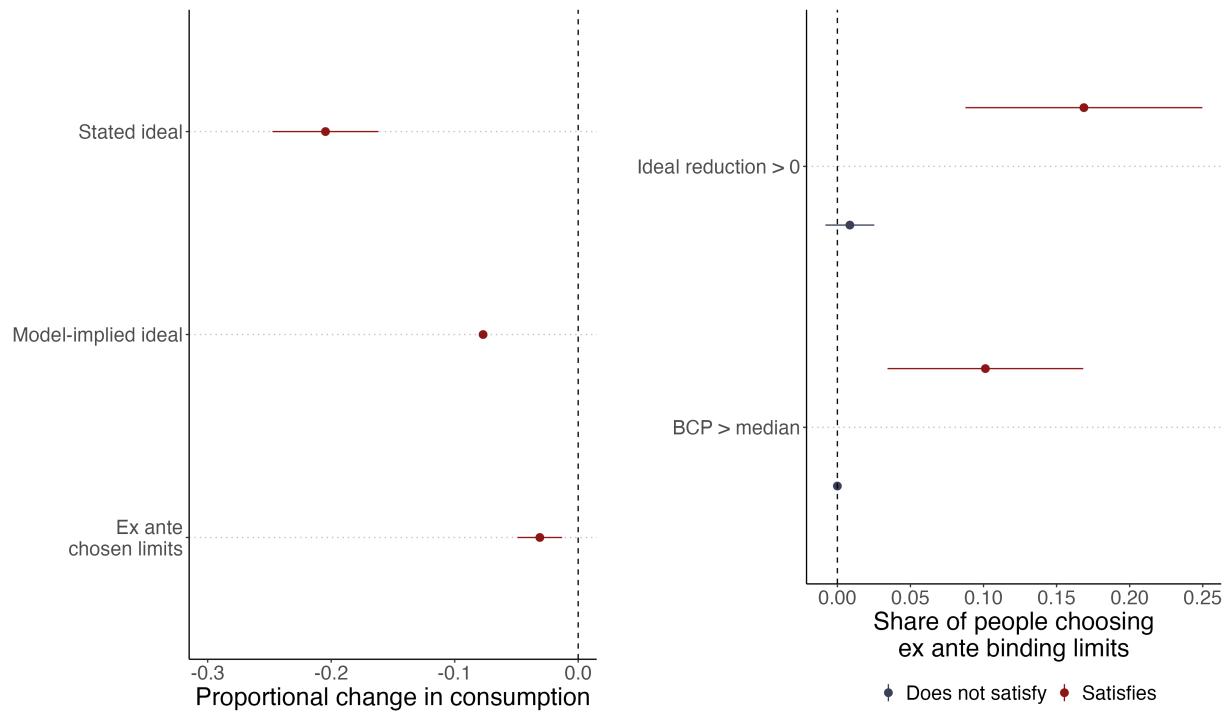
**Figure 14:** Tradeoffs between betting volume and nonproducer surplus

Notes: The figure illustrates the tradeoffs between improved welfare and the implied reduction in betting consumption for a variety of policy types described in Section 8. Nonproducer surplus is the sum of normative consumer surplus and government revenues. The linear tax curve scales rates from the status quo to the optimal tax. The misperception reduction curve scales  $\omega^M$  from 0 to 1. The first-best point is the point from a person-specific tax  $\tau_i = \gamma_i^M + \gamma_i^{SC}$ .



**Figure 15:** 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 domain 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 panel D includes a point for both treated and control participants to improve precision. Lines, coefficients, and standard errors all come from univariate regressions.



**Figure 16: 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 proportional reduction implied by limit choices (or *limit tightness*). 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  $E[\tilde{\gamma}_i^{SC}] \cdot E[\eta_i] = 0.007 \cdot 11\% = 7.7\%$ . 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.

# Appendices

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## A Experimental Design Appendix

Survey instruments are available at [mattbrownnecon.github.io](https://mattbrownnecon.github.io).

### A.1 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 the self-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.2 Total surplus elicitation

We elicited 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

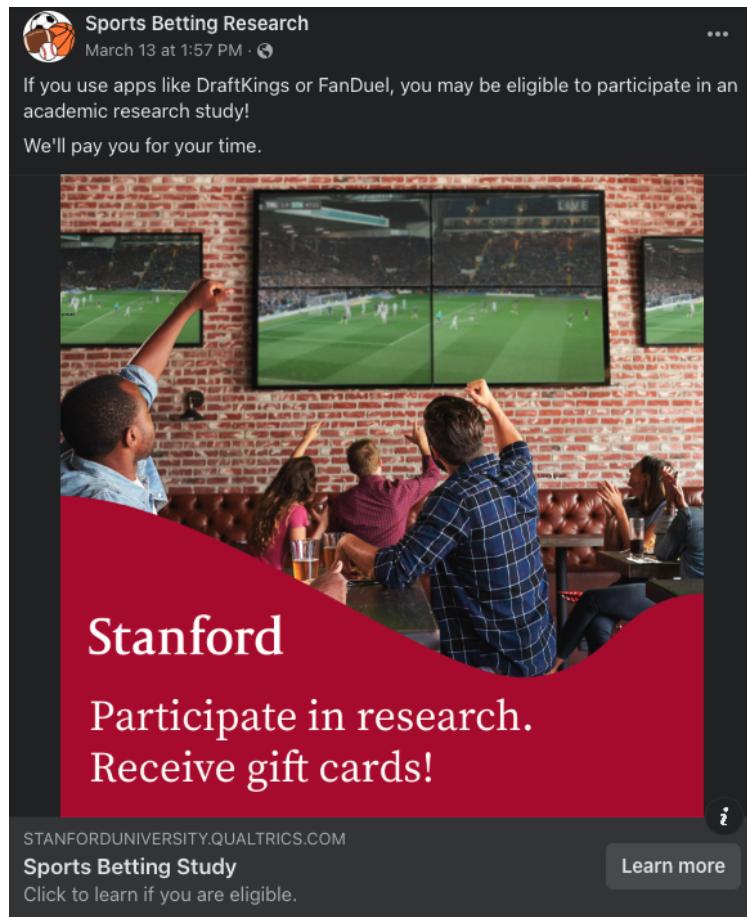
30 days if the sports schedule had been “similar to the schedule between Survey 1 and Survey 2 (April 9th to May 9th).” Differences between WTAs to hypothetical questions are informative about the extent to which the incentivized WTA differs from the target.

### A.3 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.

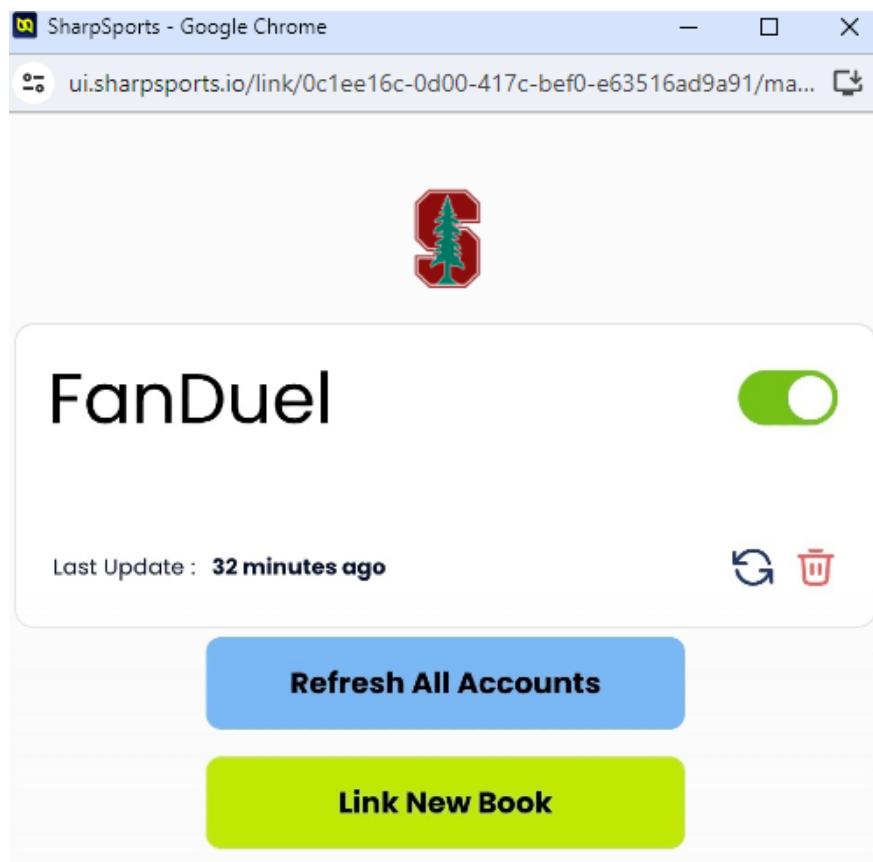
Our implementation follows an elicitation procedure from Allcott et al. (2022c). We frame the 50% chance as a “coin toss for \$100.” Then, we introduce a two-step incentivized MPL. We begin by showing participants binary choice between \$50 for sure and the coin toss. Then, depending on the response 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.4 Tables and Figures



**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.

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

## 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  $p_j$ s, we measure the excess mass at each focal prediction, that is, the share of respondents 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-rounded 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 predictions.

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 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  $\gamma_i^O$  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 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  $\hat{r}_{it}^{Perc}$  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  $\hat{\gamma}_{it}^O = \hat{r}_{it}^{Perc} - \hat{r}_{it}$  overstates the variance of latent overoptimism  $\gamma_i^O$ .

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.

$$\hat{r}_{it} = r_i + \nu_{it}^{\text{Realization}} \quad E[\nu_{it}^{\text{Realization}}] = 0; \quad V[\nu_{it}^{\text{Realization}}] = \sigma_{\text{Realization},it}^2 \quad (\text{B.1})$$

$$\hat{r}_{it}^{\text{Perc}} = r_i^{\text{Perc}} + \nu_{it}^{\text{Response}} \quad E[\nu_{it}^{\text{Response}}] = 0; \quad V[\nu_{it}^{\text{Response}}] = \sigma_{\text{Response}}^2 \quad (\text{B.2})$$

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

The variance of responses  $\sigma_{\text{Response}}^2$  is identified from the variance of changes in responses across surveys.<sup>51</sup> We estimate that response errors have a standard deviation of  $\hat{\sigma}_{\text{Response}} = 12.3\%$ .

Observation-specific realization variances  $\sigma_{\text{Realization},it}^2$  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

$$\sigma_{\text{Realization},it}^2 = \sum_k s_{itk}^2 (q_{itk} - 1) \quad (\text{B.3})$$

where  $s_{itk}$  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  $\sigma_{\text{Realization},it}$  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 we observe heterogeneous prediction errors 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 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  $\gamma_i^O$  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.

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<sup>51</sup>Specifically, the variance of changes in response across surveys  $\text{Var}(\Delta \hat{r}^{\text{Perc}})$  is equal to  $2\sigma_{\text{Response}}^2$ .

### B.3 Bonus treatment effects

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

$$Y_{it} = \tau_t^B B_i + \beta_t X_i + \delta_t + \varepsilon_{it} \quad (\text{B.4})$$

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  $\delta_t$  is a period fixed effect.<sup>52</sup> 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  $Y_{it}^{\text{Norm}}$  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  $Y_{it}^{\text{Norm}}$  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 can neither rule out large habit formation nor zero habit formation.

It was not the case that treated participants simply substituted from legal sports betting to other kinds of gambling, as we show using self-reported consumption data (Appendix Figure B.12). 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

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<sup>52</sup>The covariates are: a limit 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.

betting consumption was diverted to the illegal market.<sup>53</sup> There are two important caveats for our substitution results. First, the estimates captures short-run substitution effects, and long-run substitution effects could be larger. Second, the dependent variable self-reported consumption, which may understate true consumption, particularly for illegal gambling. Of course, nearly every other study of illegal gambling is also vulnerable to this second issue.

#### B.4 Representativeness and weighted sample analyses

Grubbs and Kraus (2023c) (henceforth GK) conducted two YouGov surveys to learn about the population of sports bettors.<sup>54</sup> 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-weighted 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 responsiveness 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 responsiveness 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

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<sup>53</sup>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 disadvantage legal sports betting operations." <https://www.americangaming.org/policies/hot-issue-sports-betting/>

<sup>54</sup>The authors and collaborators have published several other descriptive studies using this data, including Grubbs and Kraus (2023b, 2022, 2023a); Connolly et al. (2024)

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.5 Tables and Figures

State	Dollars Wagered (millions)	Tax Revenues (millions)	Excise-equivalent Tax (%)
<i>State taxes</i>			
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
<b>Total</b>	<b>121,075</b>	<b>2,144.5</b>	<b>1.77</b>
<i>Federal excise tax</i>			
<b>Total</b>	<b>121,075</b>	<b>302.7</b>	<b>0.25</b>
<i>Federal + State</i>			
<b>Total</b>	<b>121,075</b>	<b>2,447.2</b>	<b>2.02</b>

**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 (0.0638)	0.0351 (0.0662)	0.0271 (0.0643)	0.0284 (0.0189)	-0.0078 (0.0623)
Above median parlay share	0.1736*** (0.0467)	0.1875*** (0.0510)	0.1660*** (0.0489)	-0.0241* (0.0138)	-0.2018*** (0.0456)
No college degree	0.1345** (0.0678)	0.1348** (0.0678)	0.1298* (0.0685)	0.0204 (0.0201)	-0.1045 (0.0662)
Graduate degree	-0.0065 (0.0496)	-0.0097 (0.0499)	-0.0078 (0.0498)	0.0139 (0.0147)	0.0252 (0.0485)
Above median live bet share	-0.0730 (0.0452)	-0.0700 (0.0455)	-0.0747 (0.0454)	-0.0102 (0.0134)	0.0688 (0.0442)
Above median wagers	-0.0227 (0.0466)	-0.0258 (0.0468)	-0.0206 (0.0468)	0.0105 (0.0138)	0.0362 (0.0455)
Above median age	-0.0319 (0.0458)	-0.0336 (0.0459)	-0.0291 (0.0461)	-0.0100 (0.0135)	0.0276 (0.0447)
Above median income	0.0213 (0.0483)	0.0223 (0.0483)	0.0205 (0.0483)	-0.0279* (0.0143)	-0.0441 (0.0471)
Above median bet riskiness		-0.0344 (0.0504)			
Standardized bet riskiness			0.0129 (0.0247)		
R <sup>2</sup>	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 standarized 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*** (0.0780)	0.0506 (0.1298)
Control Prediction $\times$ Control	0.3173*** (0.0436)	0.3480*** (0.0781)
Bonus Prediction $\times$ Bonus	0.2093*** (0.0478)	0.1516*** (0.0575)
Control Prediction $\times$ Bonus		0.1462* (0.0848)
Bonus Prediction $\times$ Control		0.0758* (0.0444)
R <sup>2</sup>	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)		Prediction (t=2) - Prior (t=1)	
	(1)	(2)	(3)	(4)
Constant	0.0062 (0.0072)	-0.0030 (0.0075)	-0.0147 (0.0124)	0.0745*** (0.0168)
Signal		0.0894*** (0.0245)	0.0649 (0.0406)	-0.0393 (0.0553)
Sample	Treated	Treated	Treated	Wager in t1
Observations	231	231	231	396
R <sup>2</sup>	0.05493		0.01103	0.00128
Adjusted R <sup>2</sup>	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			Brown, Grasley, and Guido	
	Census Matched	Weekly Lottery	Weekly Sports	Unweighted	Weighted
N	2806	406	517	444	444
<b>Demographics</b>					
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)
<b>Qualitative bias measures</b>					
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)

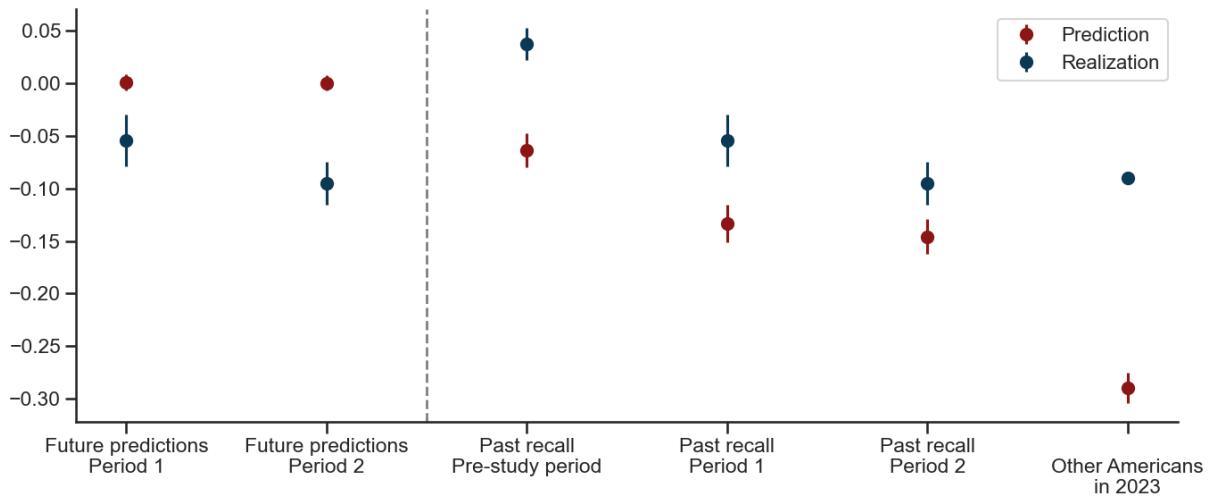
**Table B.5:** Weighted sample characteristics

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 variables 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*** (0.0657)	-0.1085*** (0.0330)	-0.2926*** (0.0760)	-0.0630 (0.0508)
Bonus × t=2	-0.1429 (0.1306)	-0.0021 (0.0324)	-0.0510 (0.0974)	0.0786 (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 <sup>2</sup>	0.04521	0.09339	0.04818	0.03237
Observations	828	828	828	686

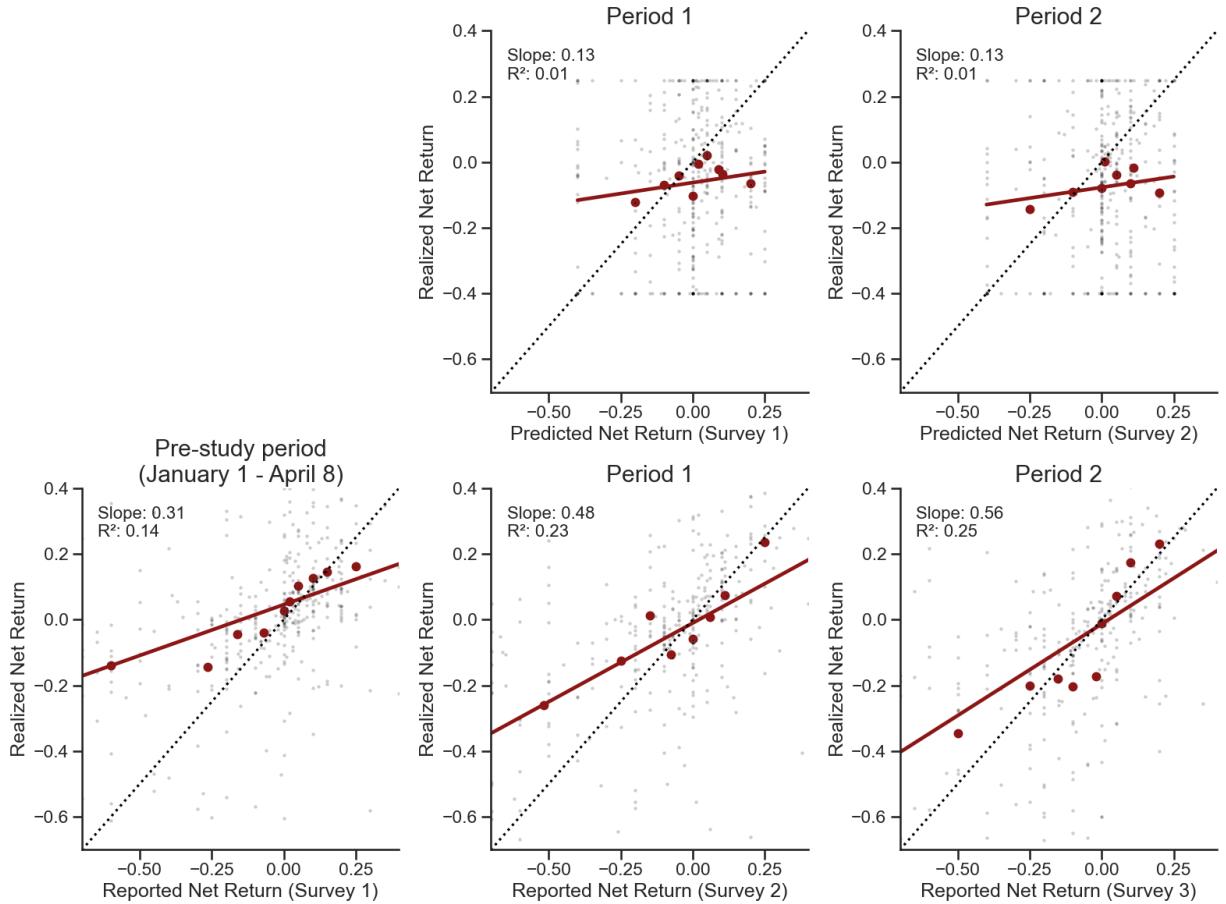
**Table B.6:** Bet Less Bonus treatment effects

Notes: We report regression results from specification defined in (B.4). 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 analogous 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. <sup>+</sup> $p < 0.1$ , <sup>\*</sup> $p < 0.05$ , <sup>\*\*</sup> $p < 0.01$ , <sup>\*\*\*</sup> $p < 0.001$ .



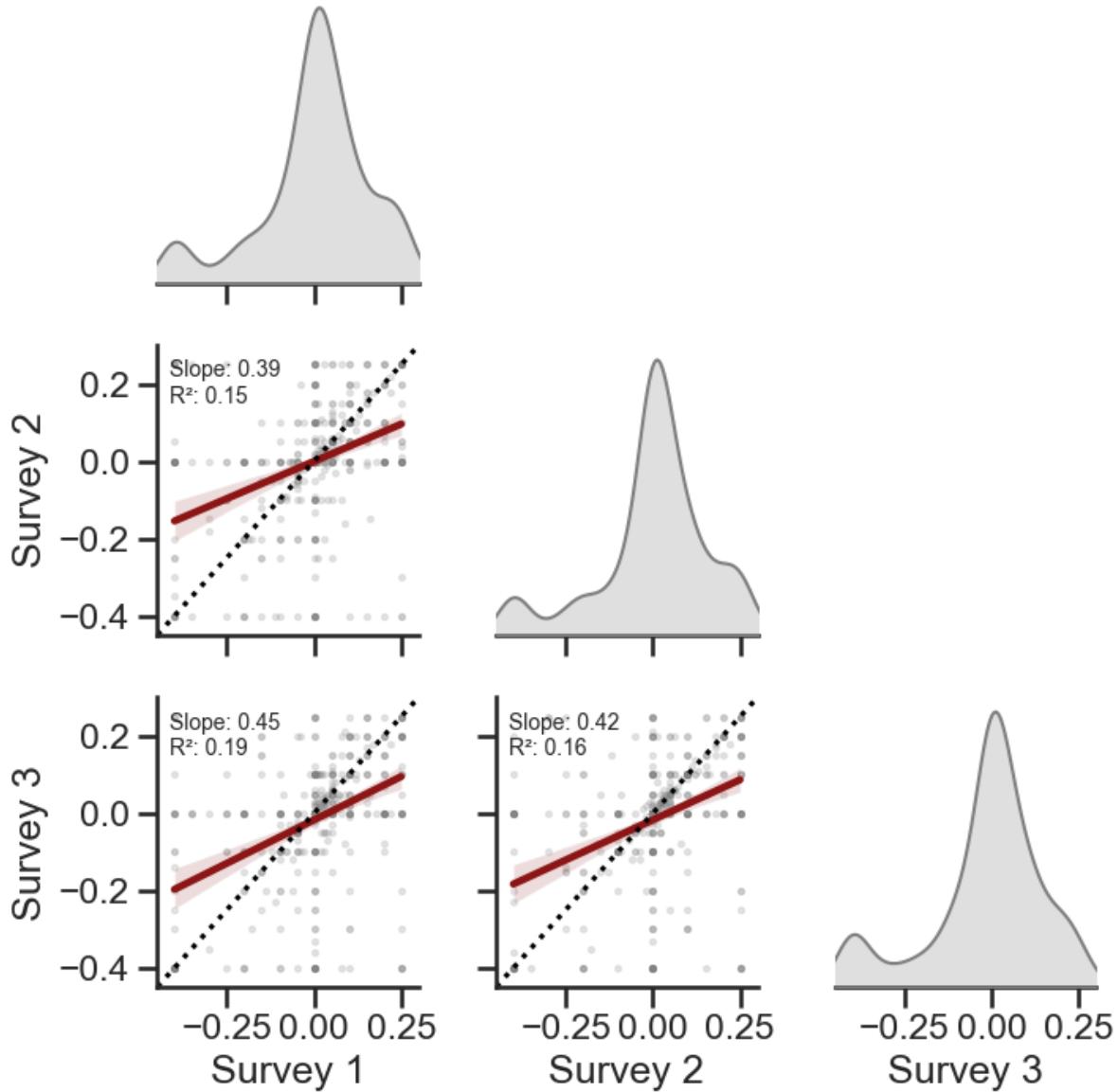
**Figure B.1:** Mispredicted financial returns in the future and the past

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.



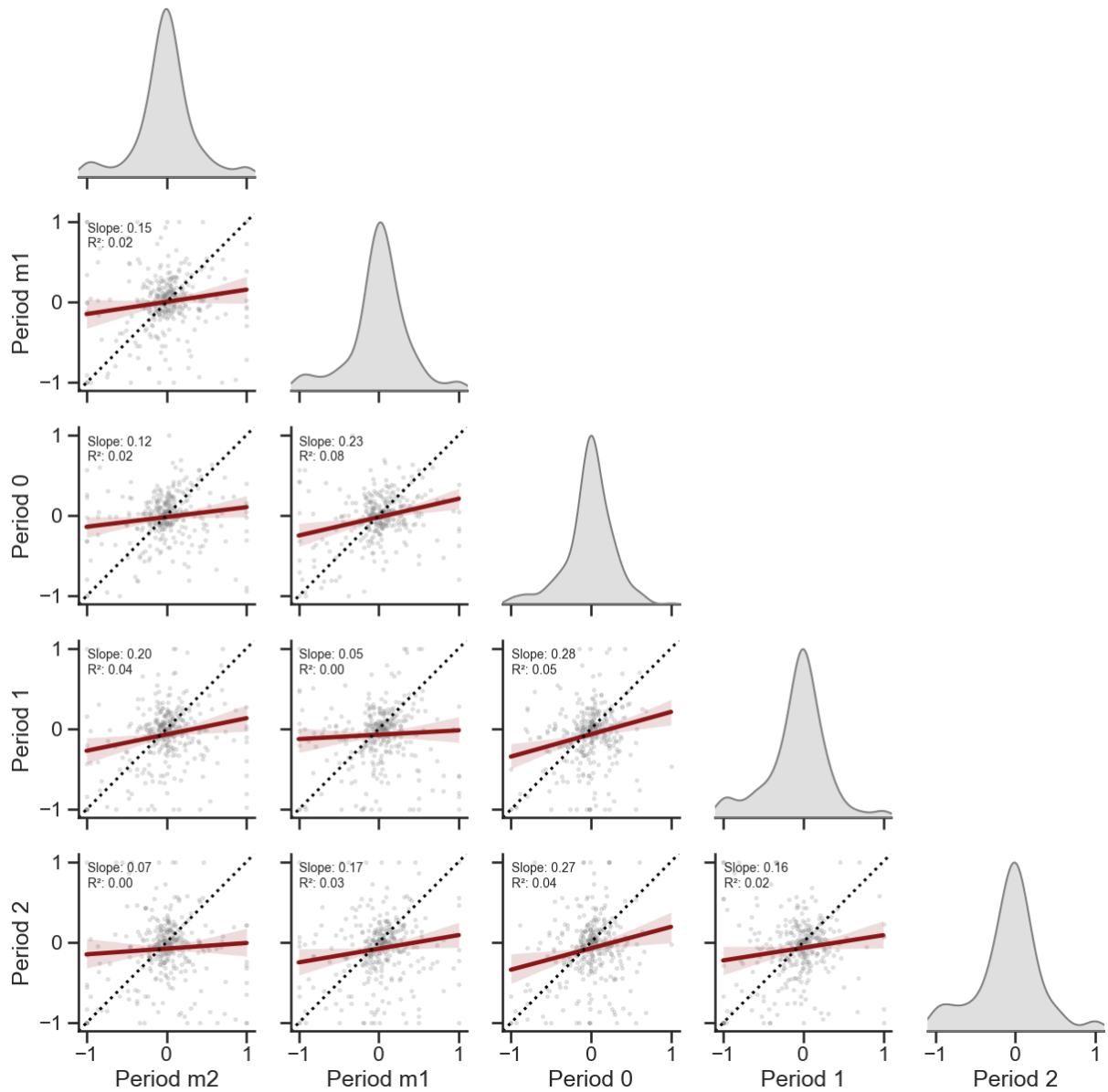
**Figure B.2:** Correlation between predicted and true financial returns in the future and past

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  $R^2$ .



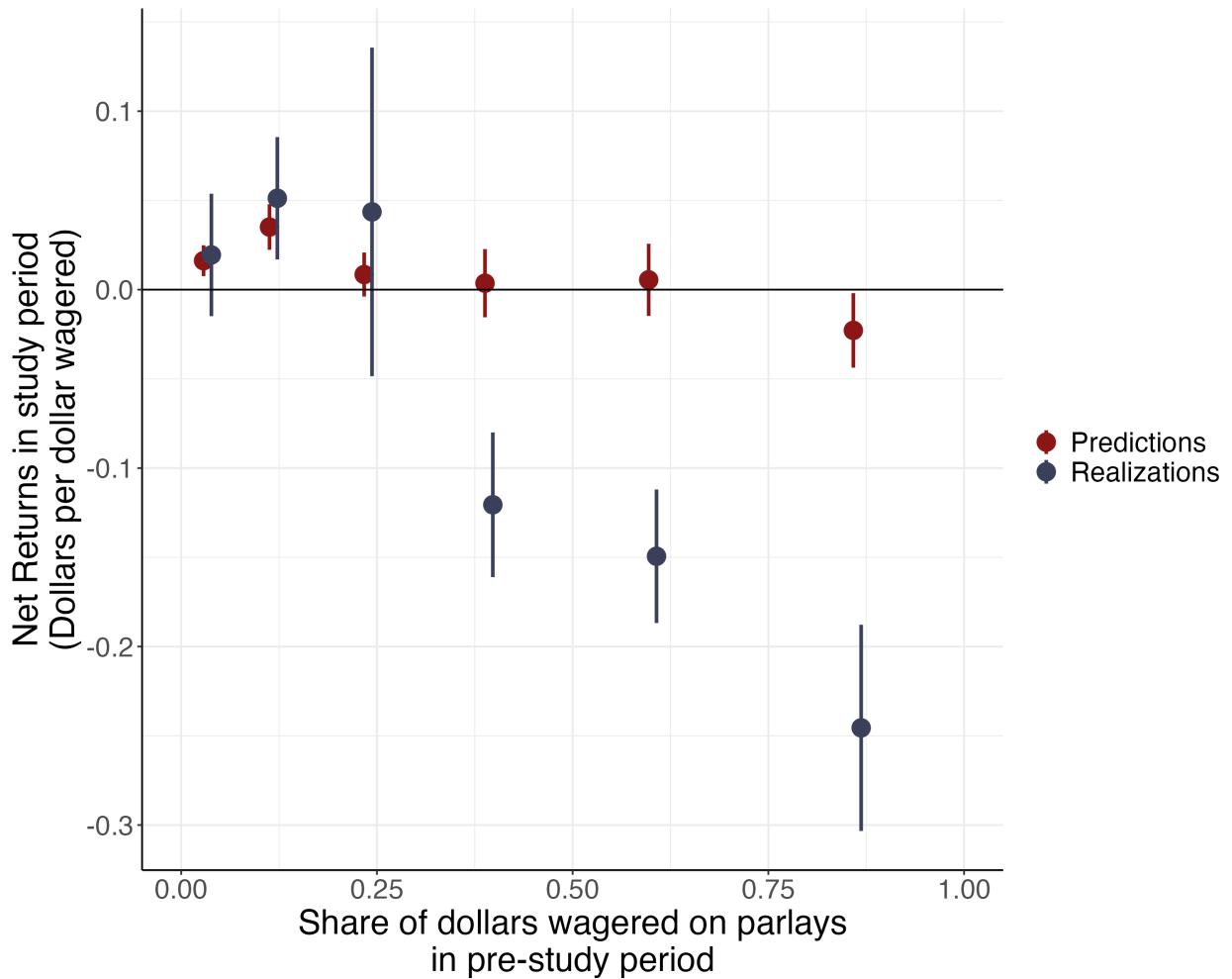
**Figure B.3:** Correlation of predicted net returns within individual over time

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



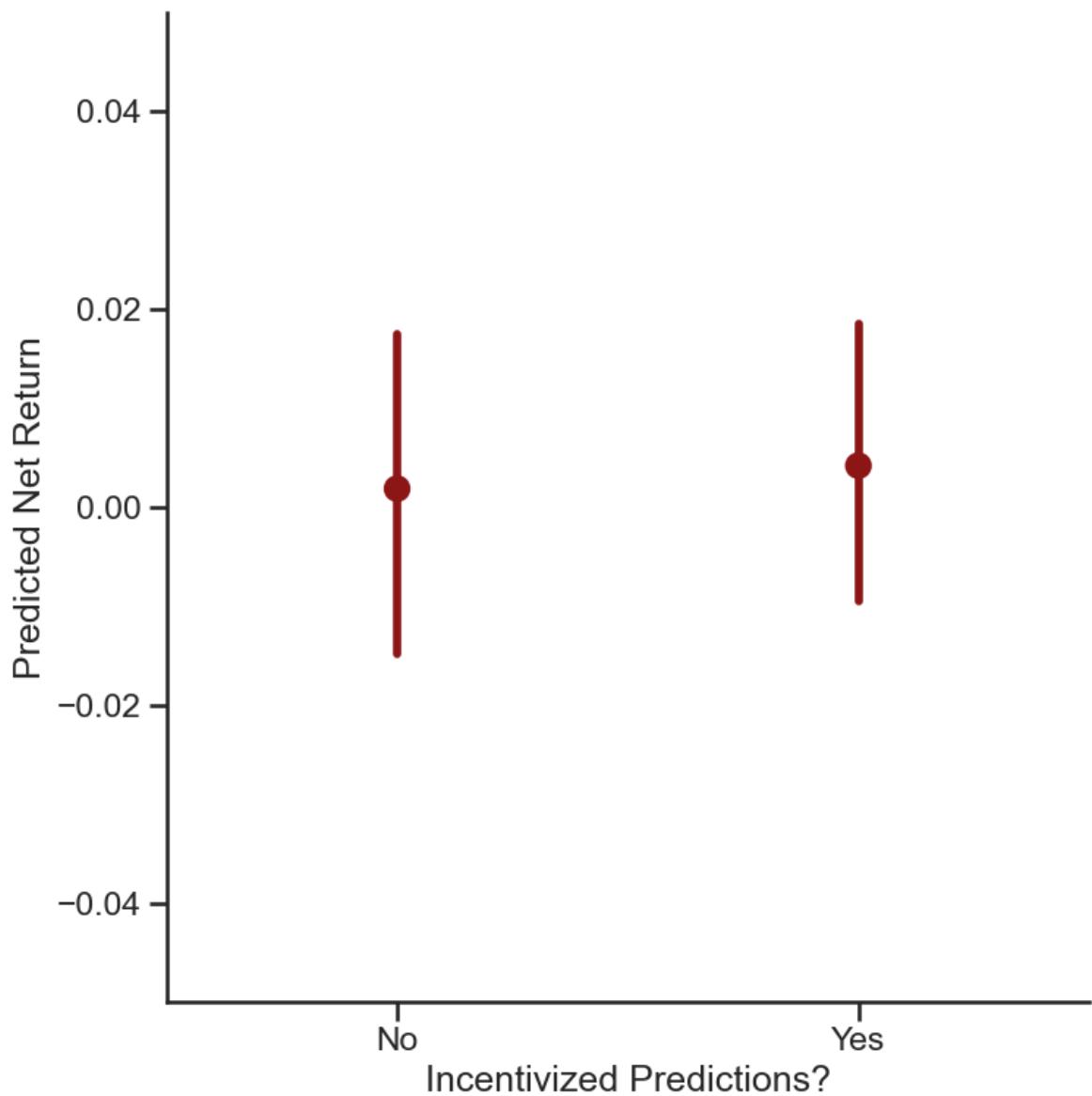
**Figure B.4: Correlation of realized returns within individual over time**

Notes: The figure illustrates the correlation of realized net returns across periods. A point is a participant in the analysis 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 densities. The line of best fit is from a univariate regression with raw (untruncated) realization data. We report the slope and  $R^2$ .



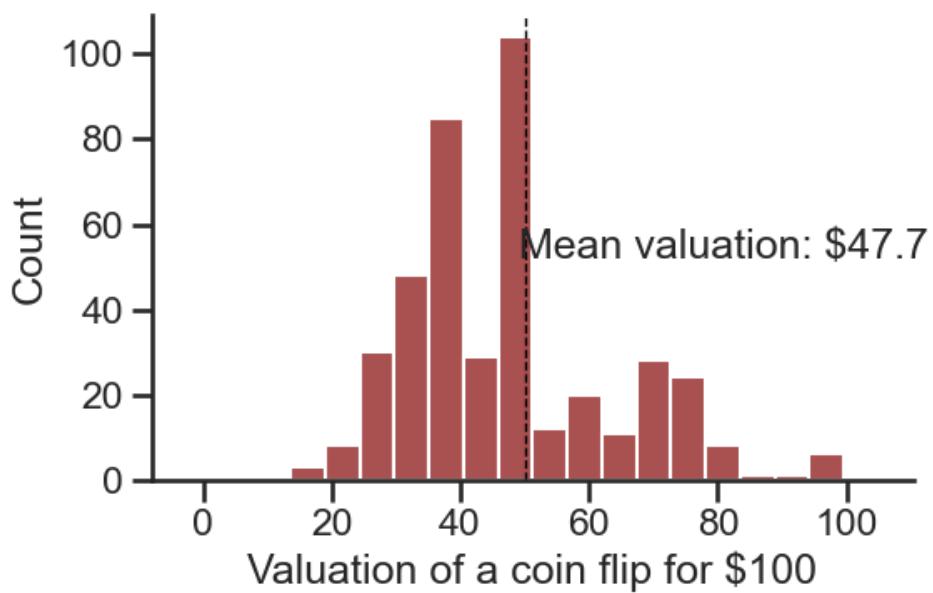
**Figure B.5:** Predicted and realized net returns by pre-study parlay share

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.



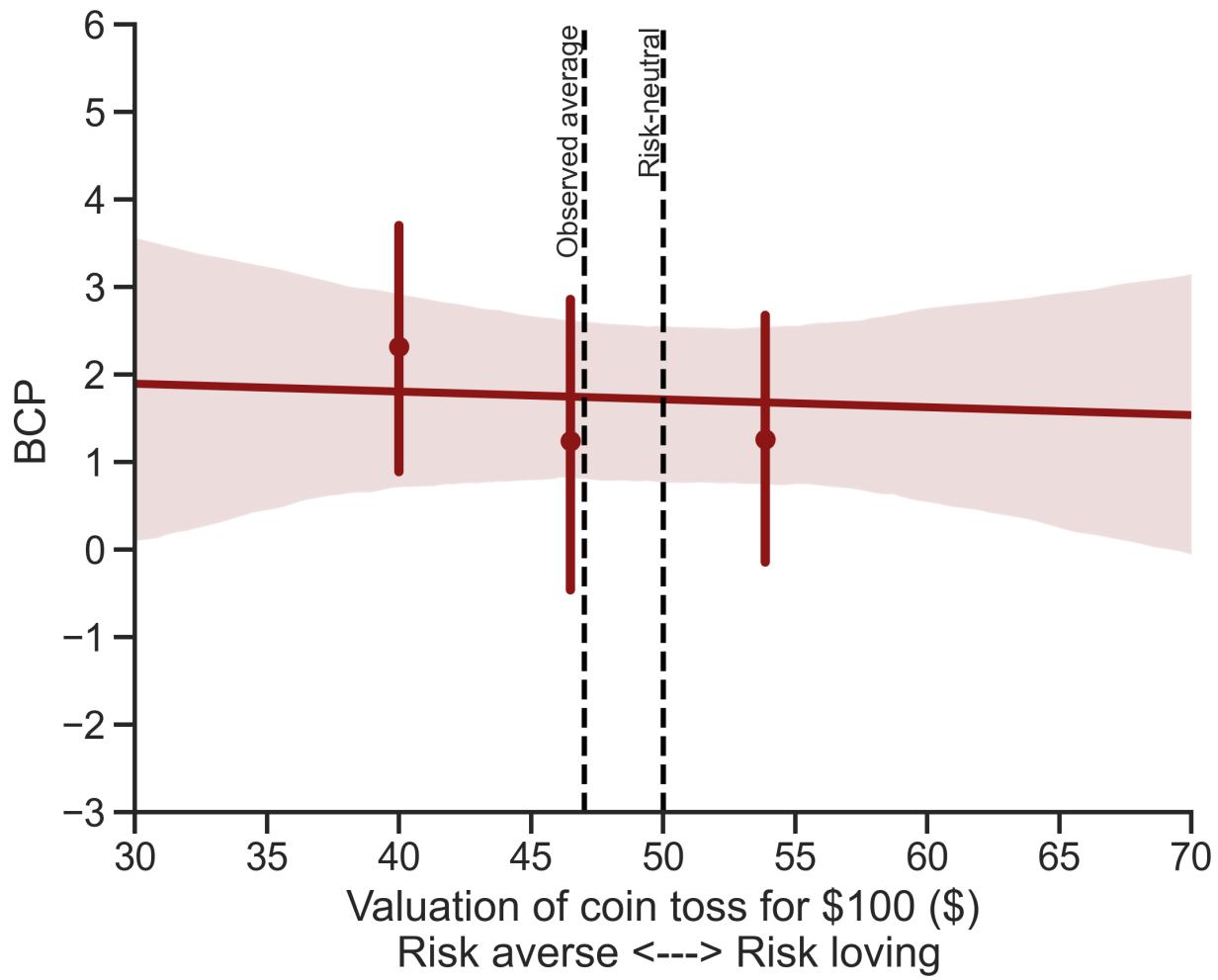
**Figure B.6:** Impact of incentivization on predictions of financial returns

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.



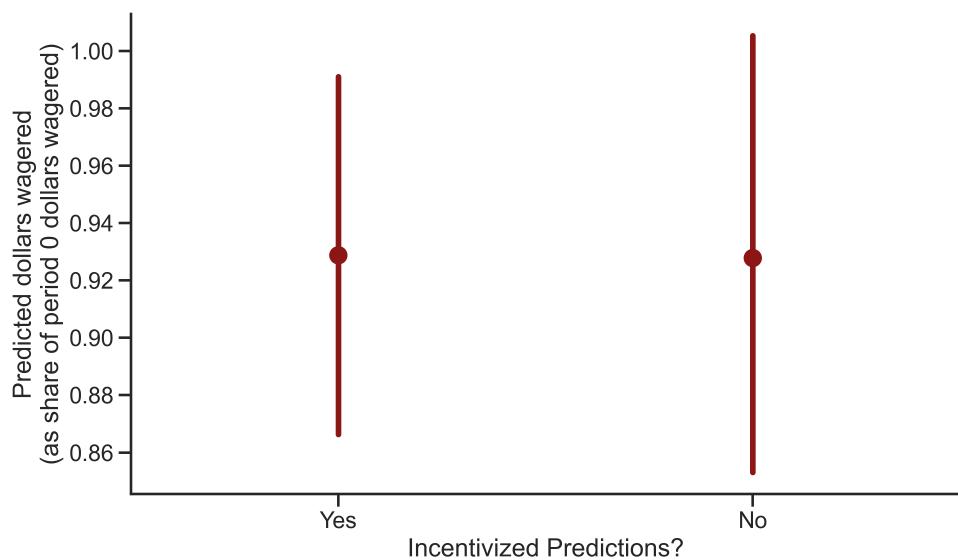
**Figure B.7:** Valuations of a \$100 coin toss

Notes: The figure illustrates the elicited valuations of a 50% chance to win \$100. The elicitations were incentivized as described in Section A.3. A \$50 valuation would correspond to risk-neutrality over experimental earnings, negative valuations to risk aversion, and positive numbers to risk-loving.



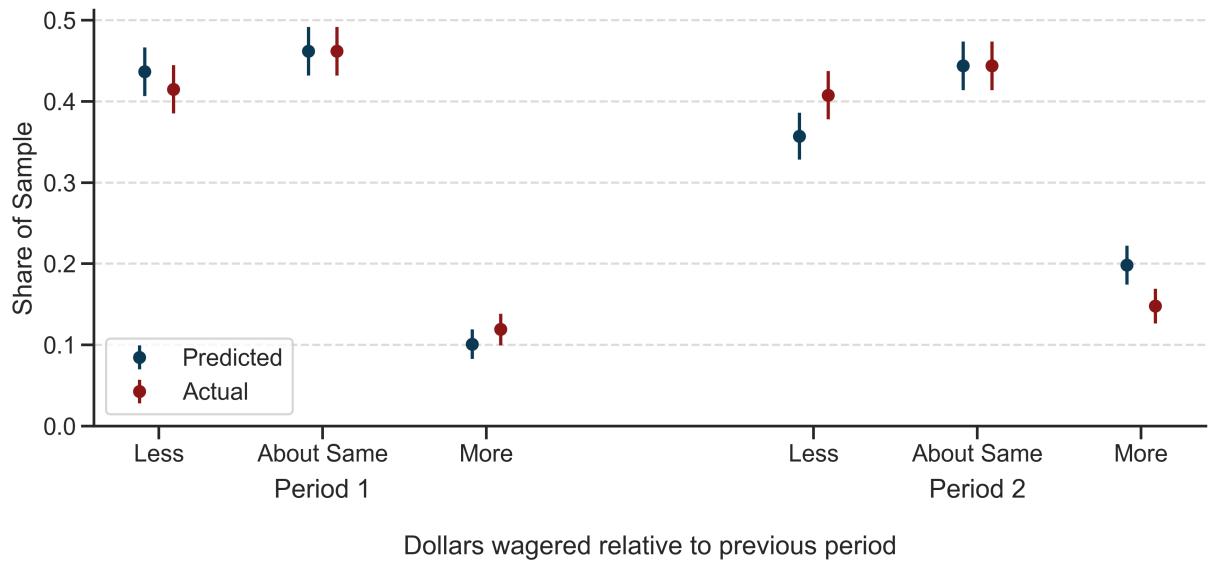
**Figure B.8:** Correlation between behavior change premia and risk aversion

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.



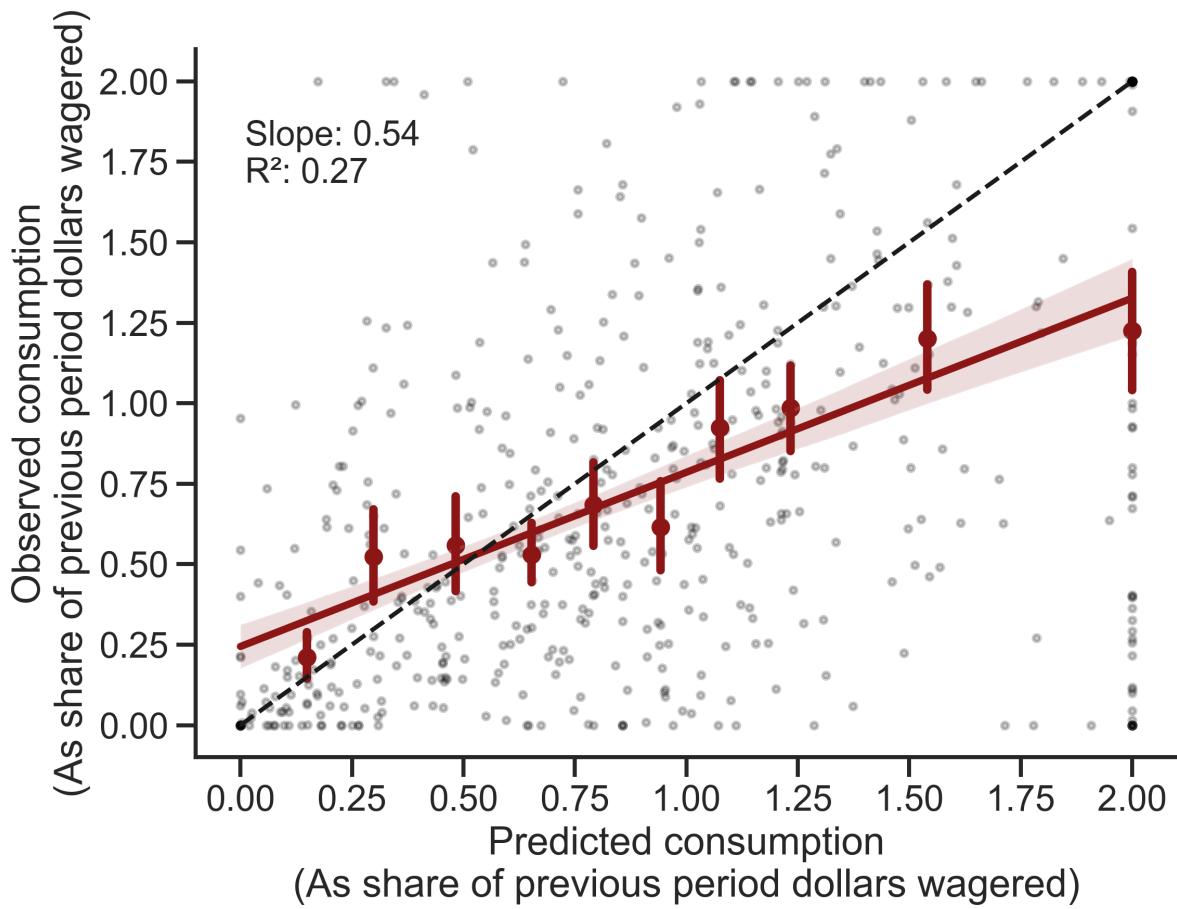
**Figure B.9:** Impact of incentivization on predictions of dollars wagered

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.



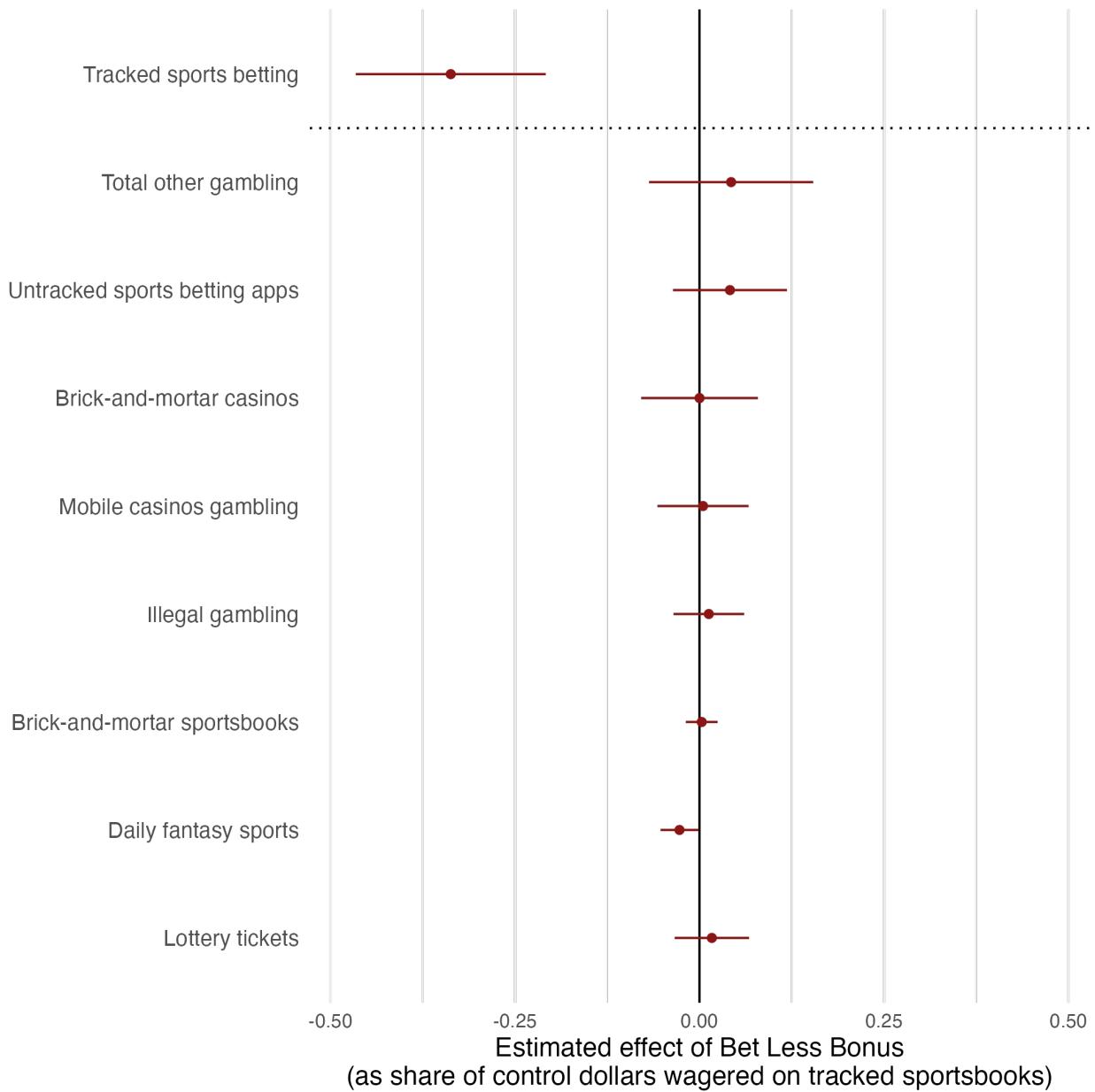
**Figure B.10:** Evidence against naivete from qualitative predictions

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.



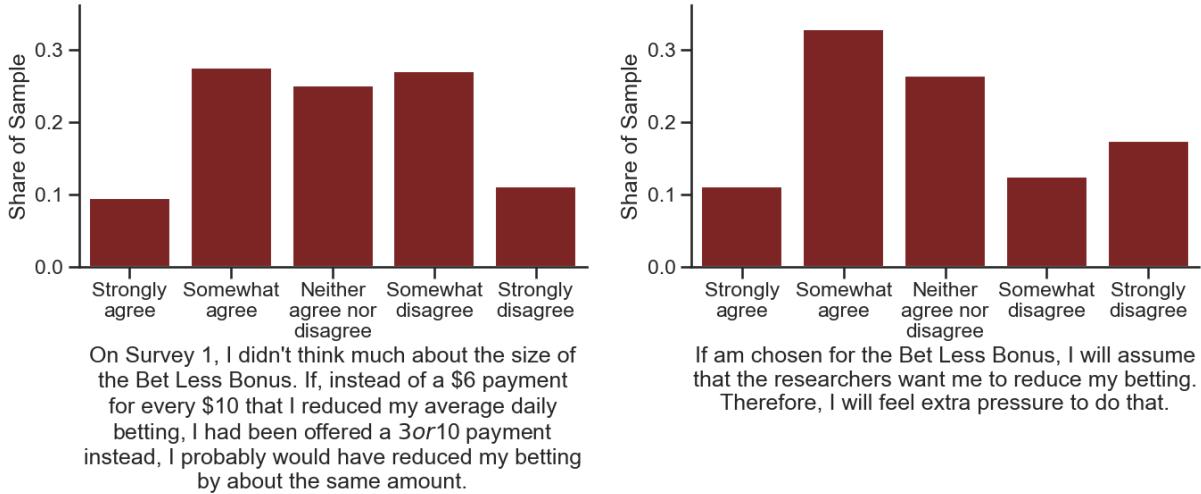
**Figure B.11:** Correlation between predicted and true future dollars wagered

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.



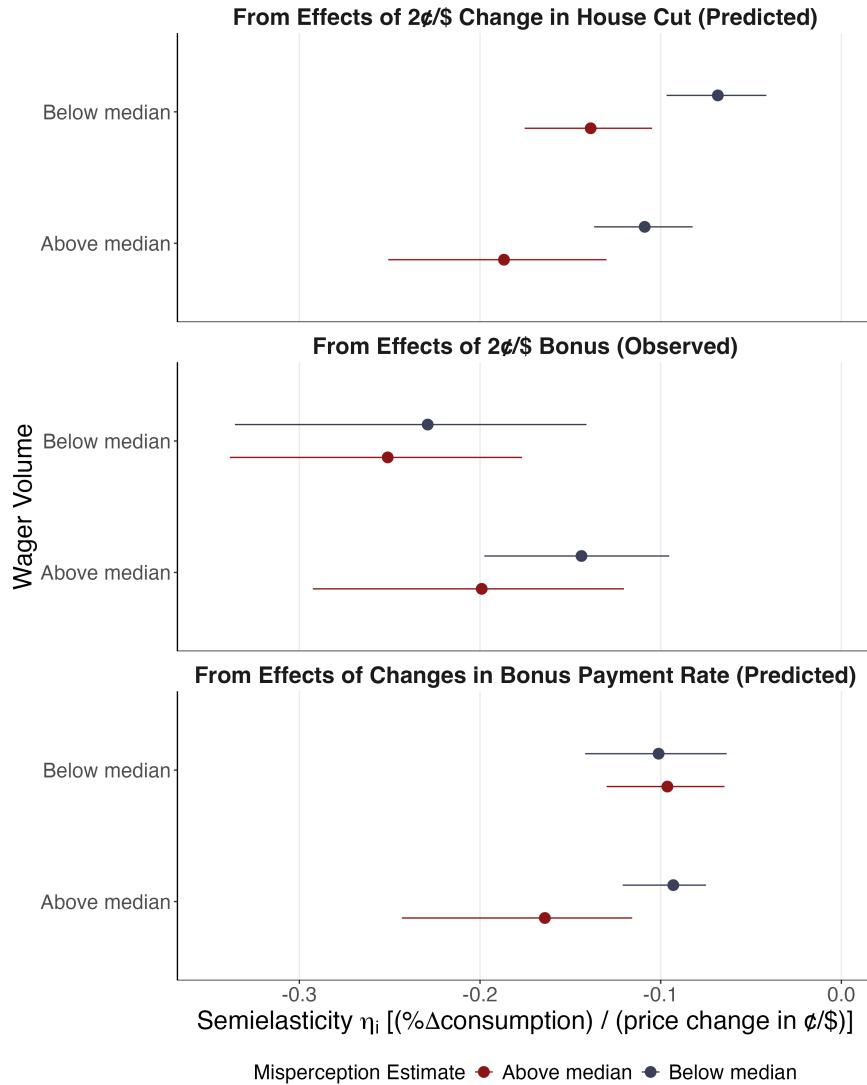
**Figure B.12:** Effect of Bet Less Bonus on self-reported untracked gambling

Notes: The figure presents coefficients from bonus treatment effect regressions as specified in equation B.4. For tracked sports betting, the dependent variable is normalized as described in that section. For untracked gambling, 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.



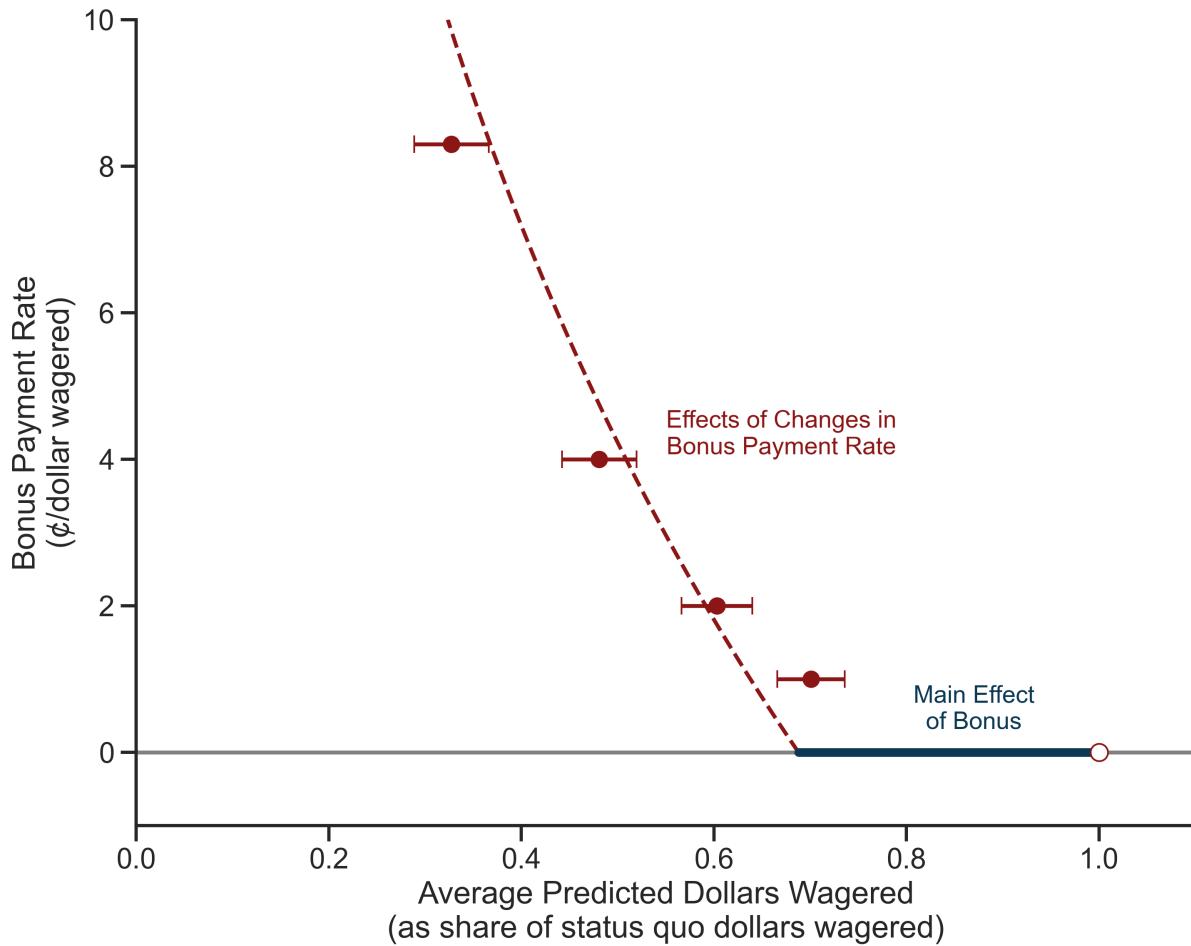
**Figure B.13:** Qualitative evidence on how participants interpreted the bonus

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



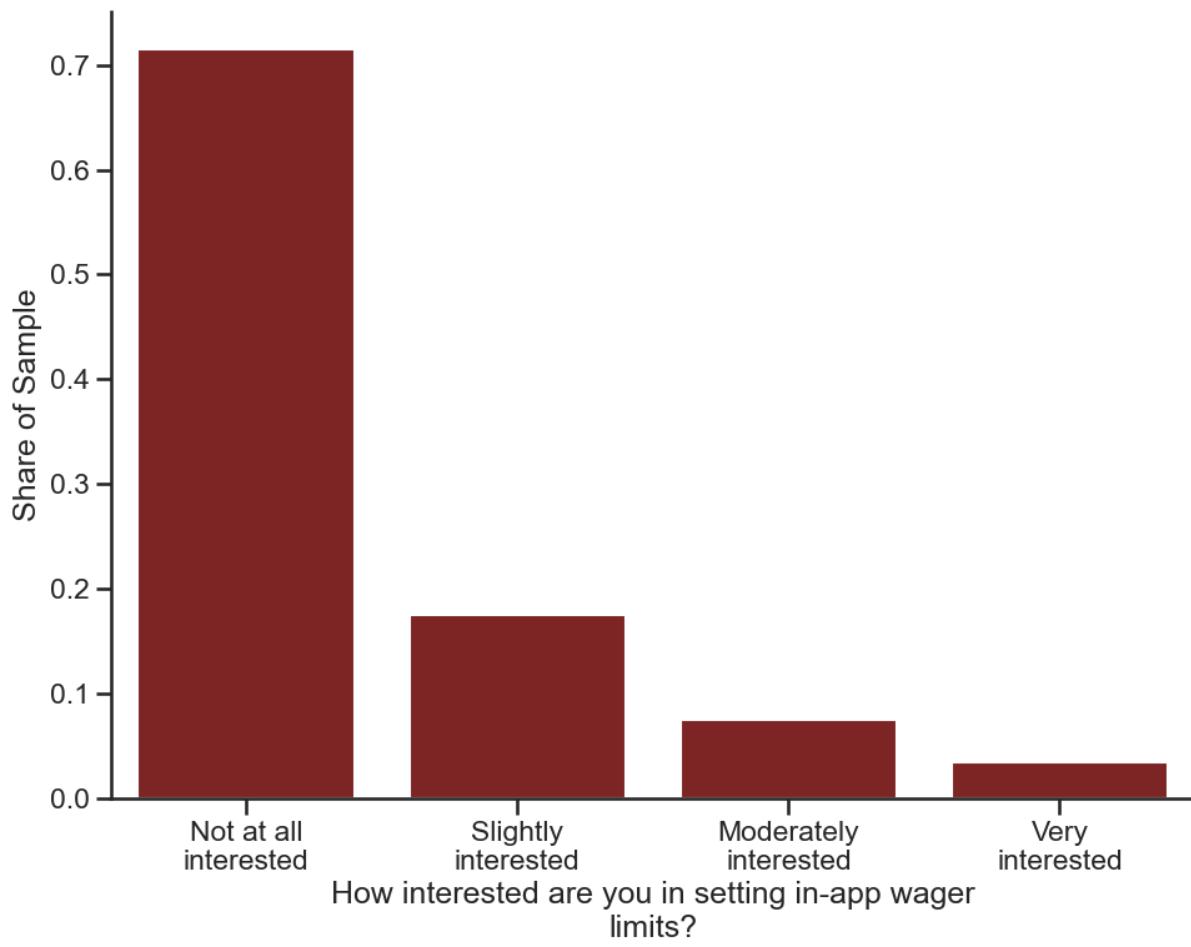
**Figure B.14:** Alternative Semielasticity Estimates

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.15). 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.



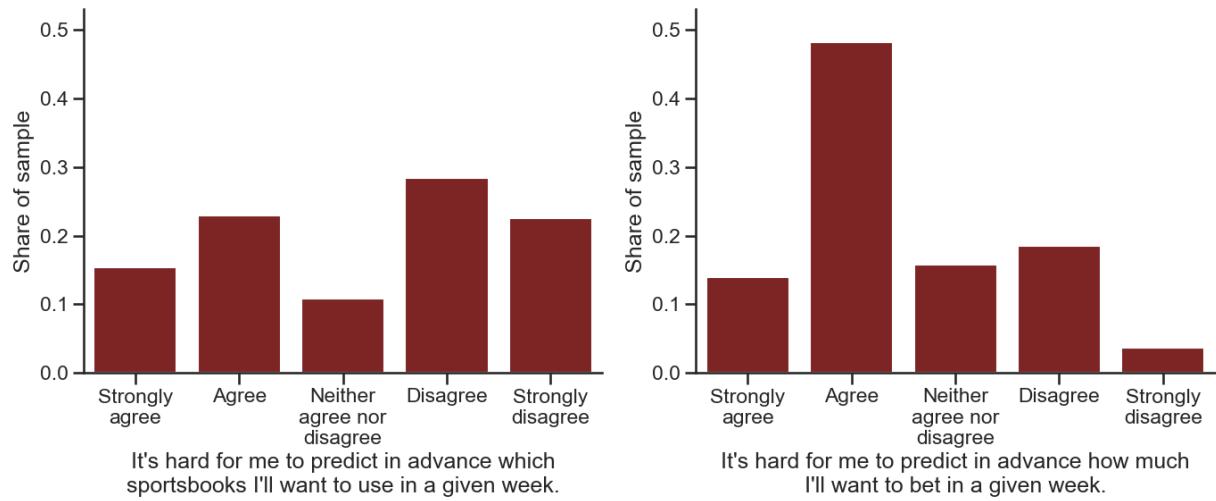
**Figure B.15:** Effects of varying the Bonus payment rate on predicted consumption

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 aggregated 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.



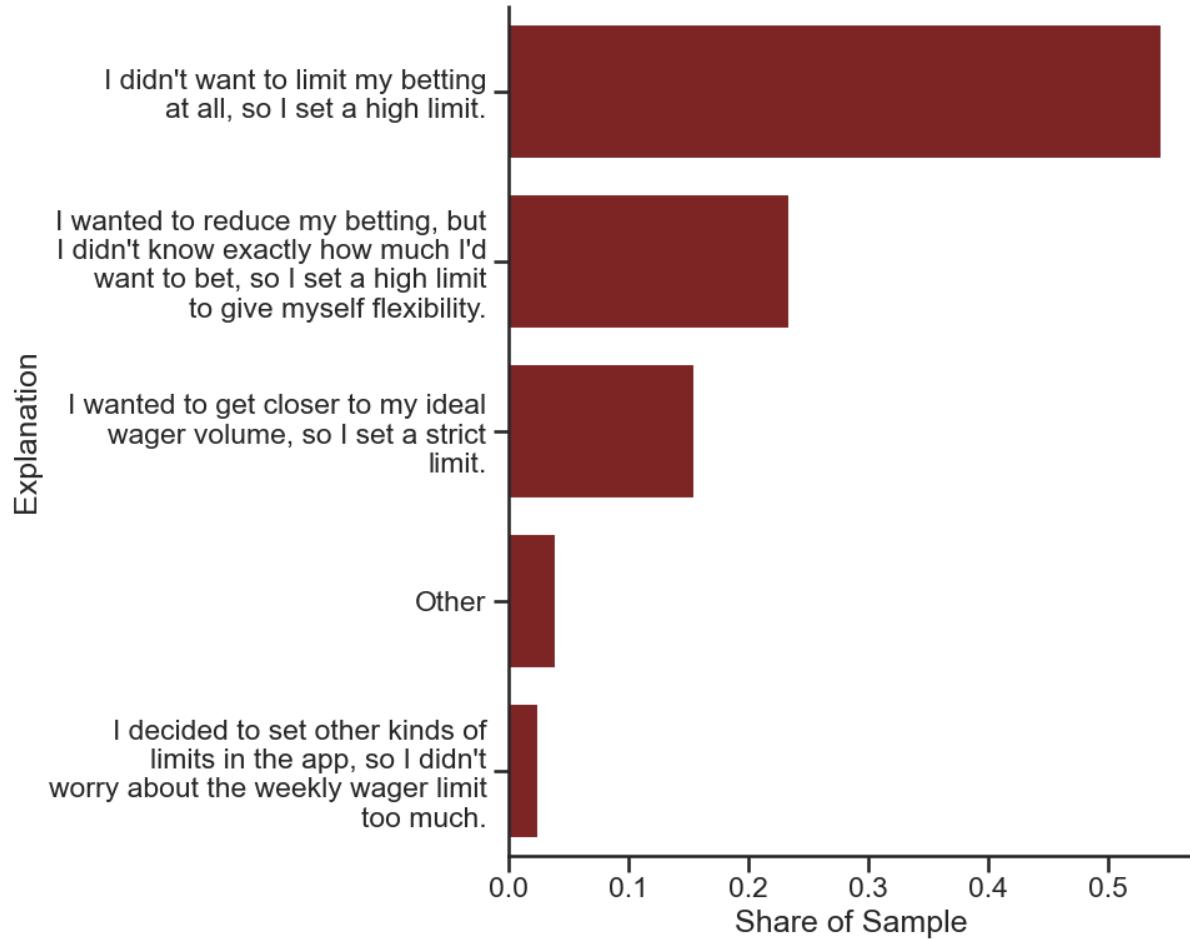
**Figure B.16:** Qualitative interest in limits

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).



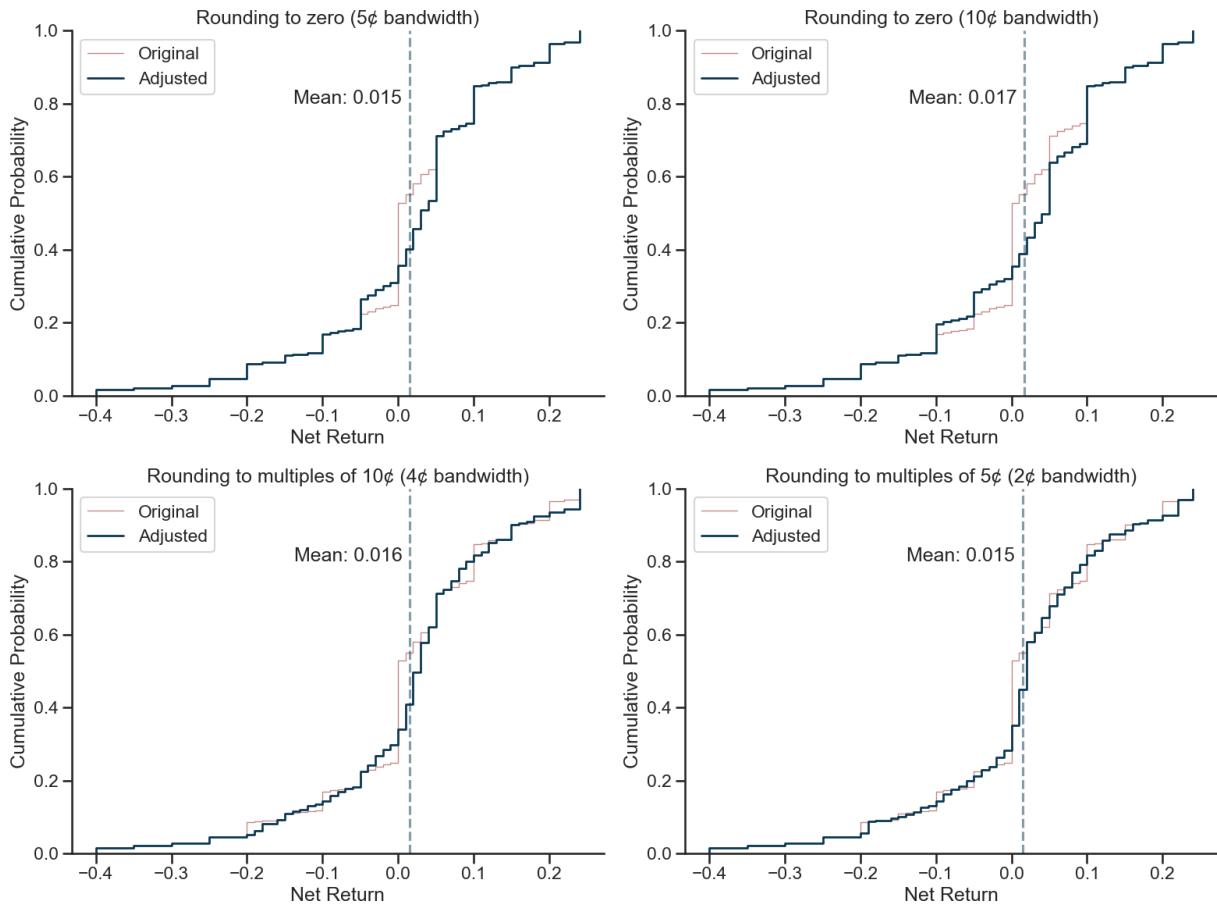
**Figure B.17: Qualitative evidence on uncertainty about future demand**

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



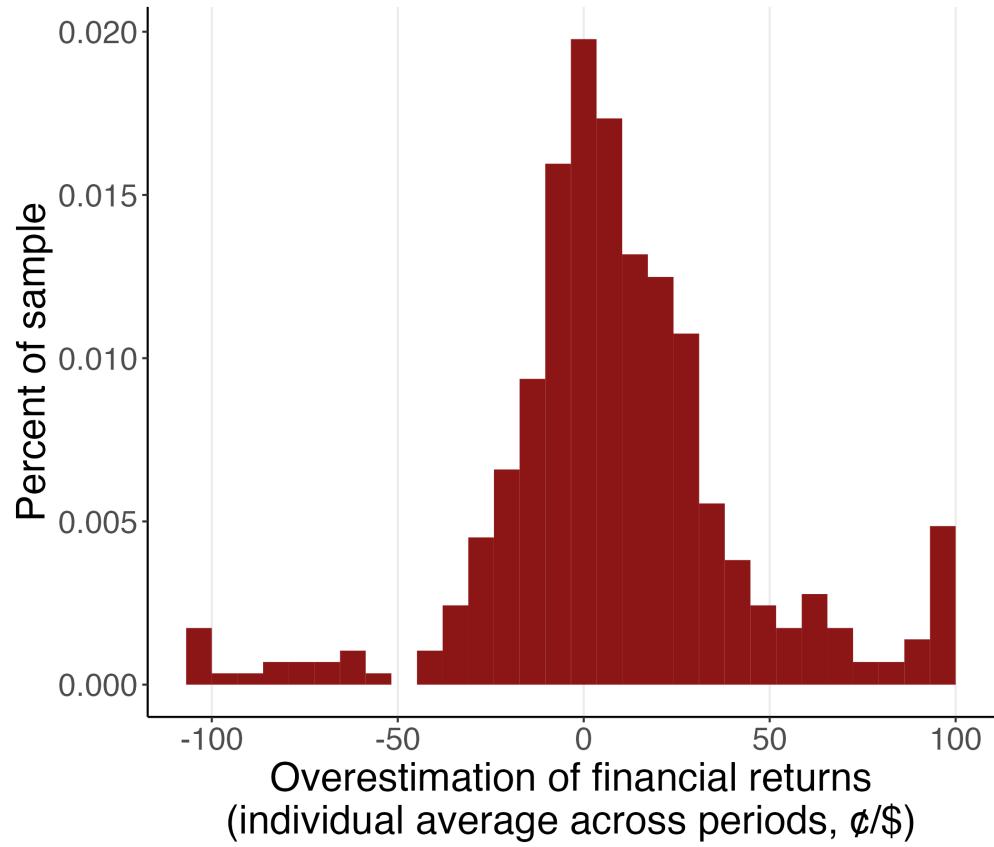
**Figure B.18:** Qualitative explanations of limit choices

Notes: The figure reports the frequency of responses 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).



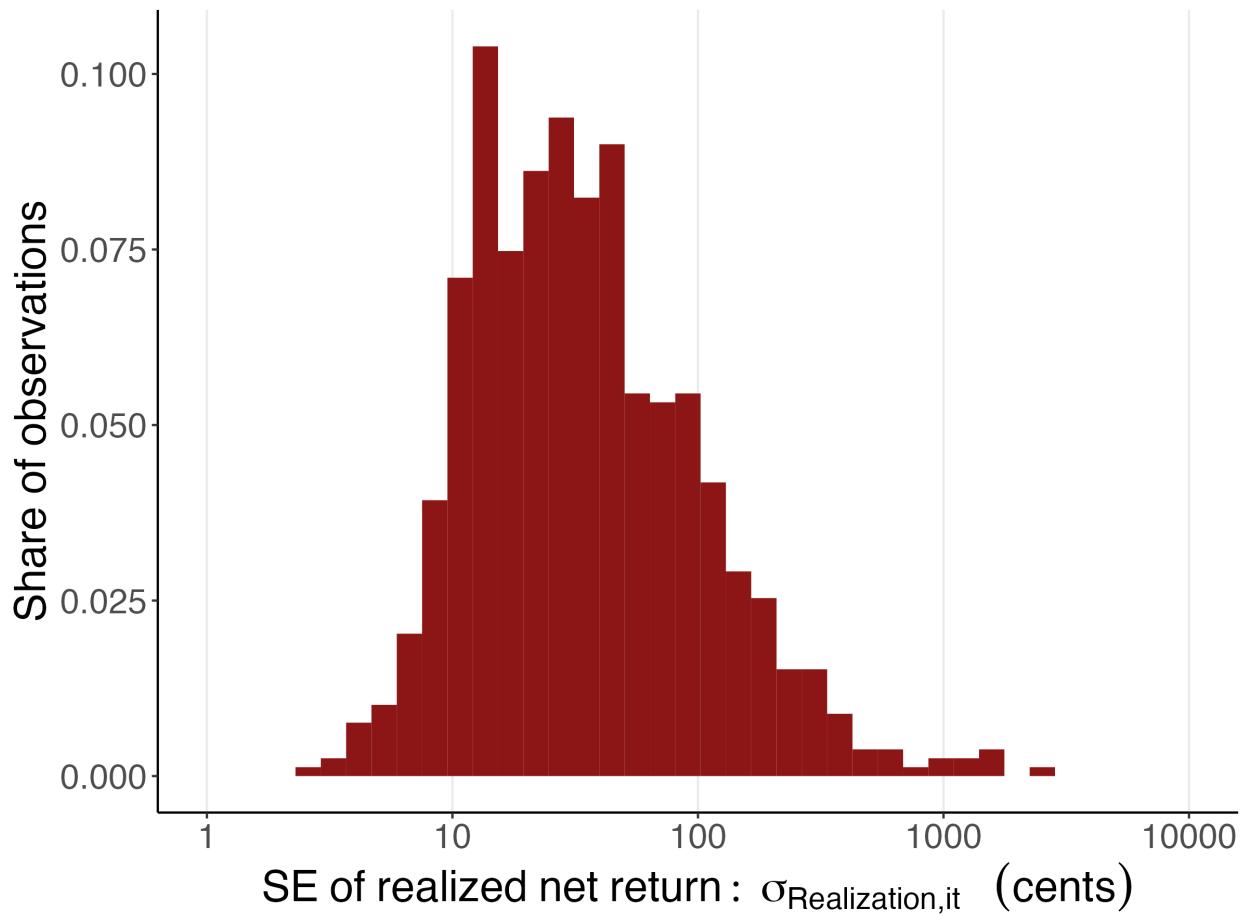
**Figure B.19:** Rounding-adjusted belief distributions

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



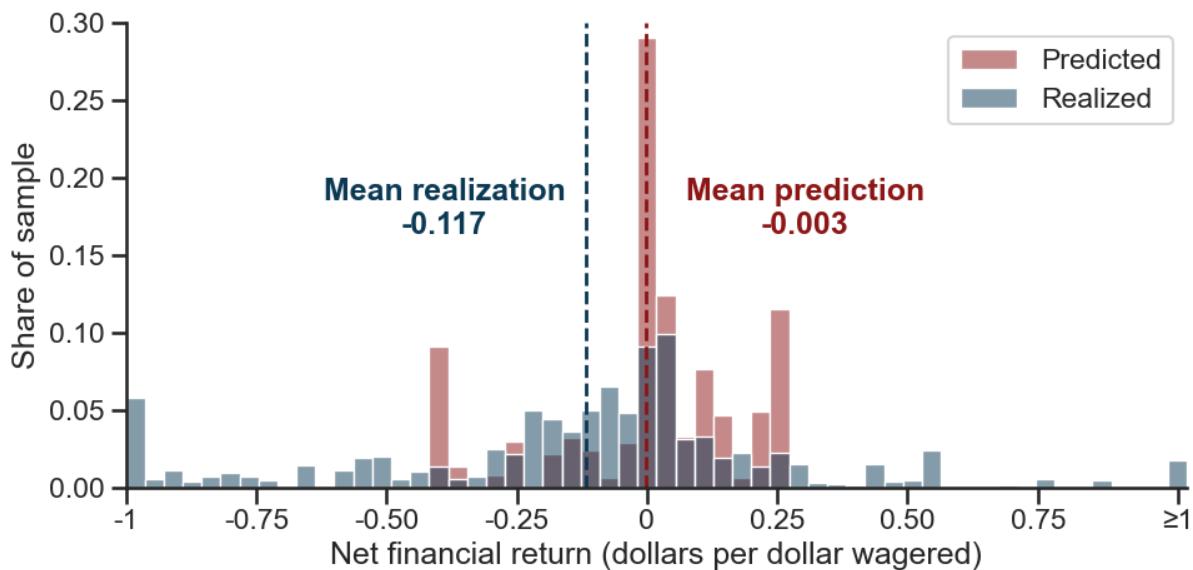
**Figure B.20:** Distribution of raw prediction errors about future returns

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.



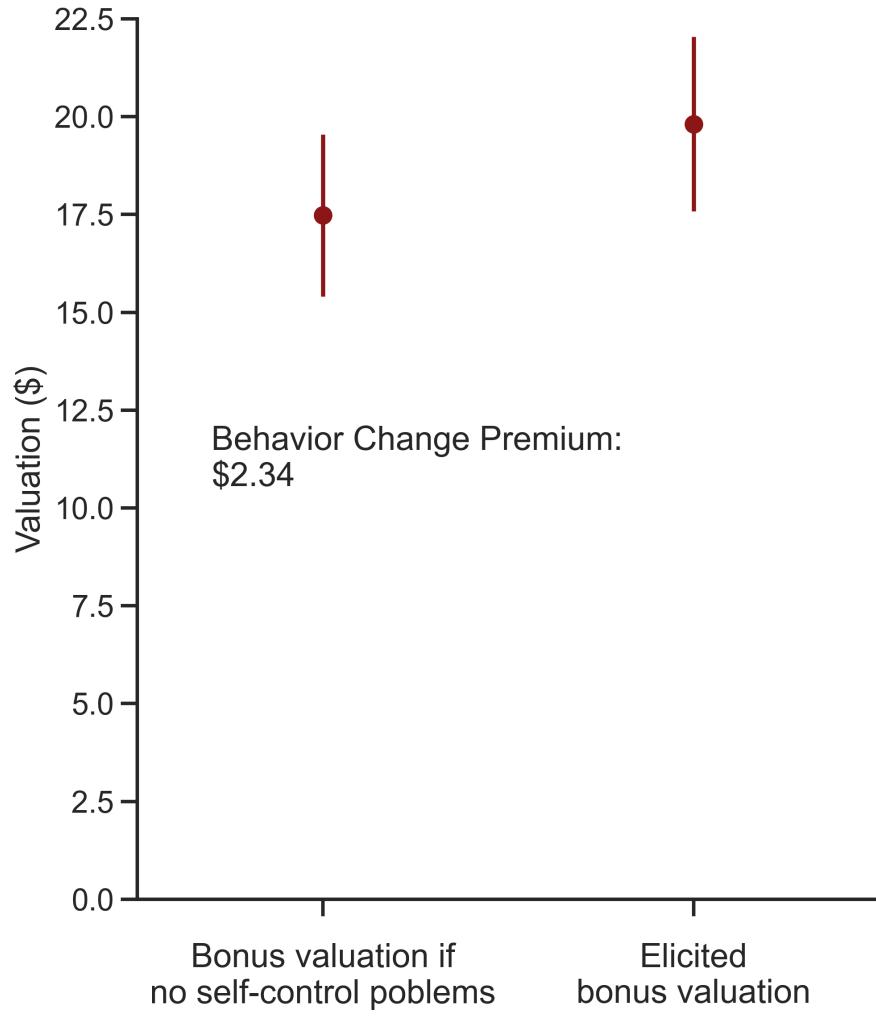
**Figure B.21:** Distribution of individual-period net return standard errors

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.2.



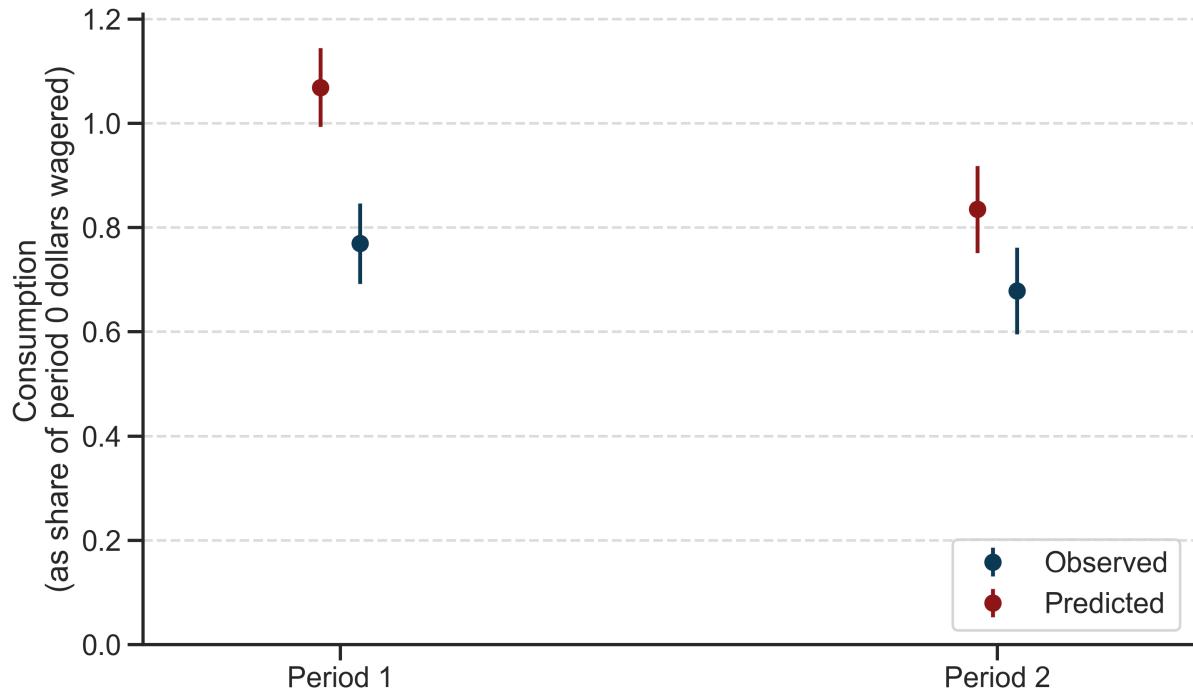
**Figure B.22:** Comparing predicted financial returns to realized financial returns (weighted sample)

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.



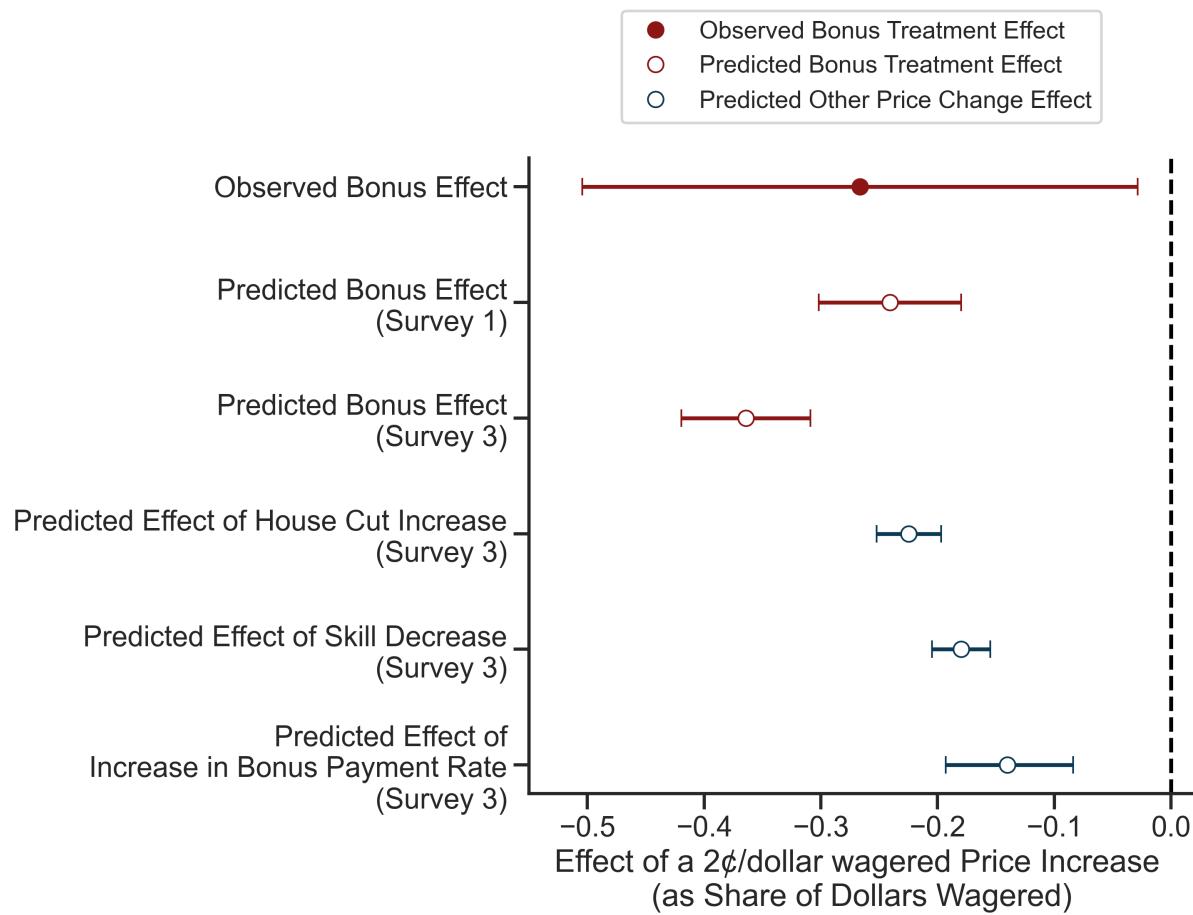
**Figure B.23:** The Behavior Change Premium (weighted sample)

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.



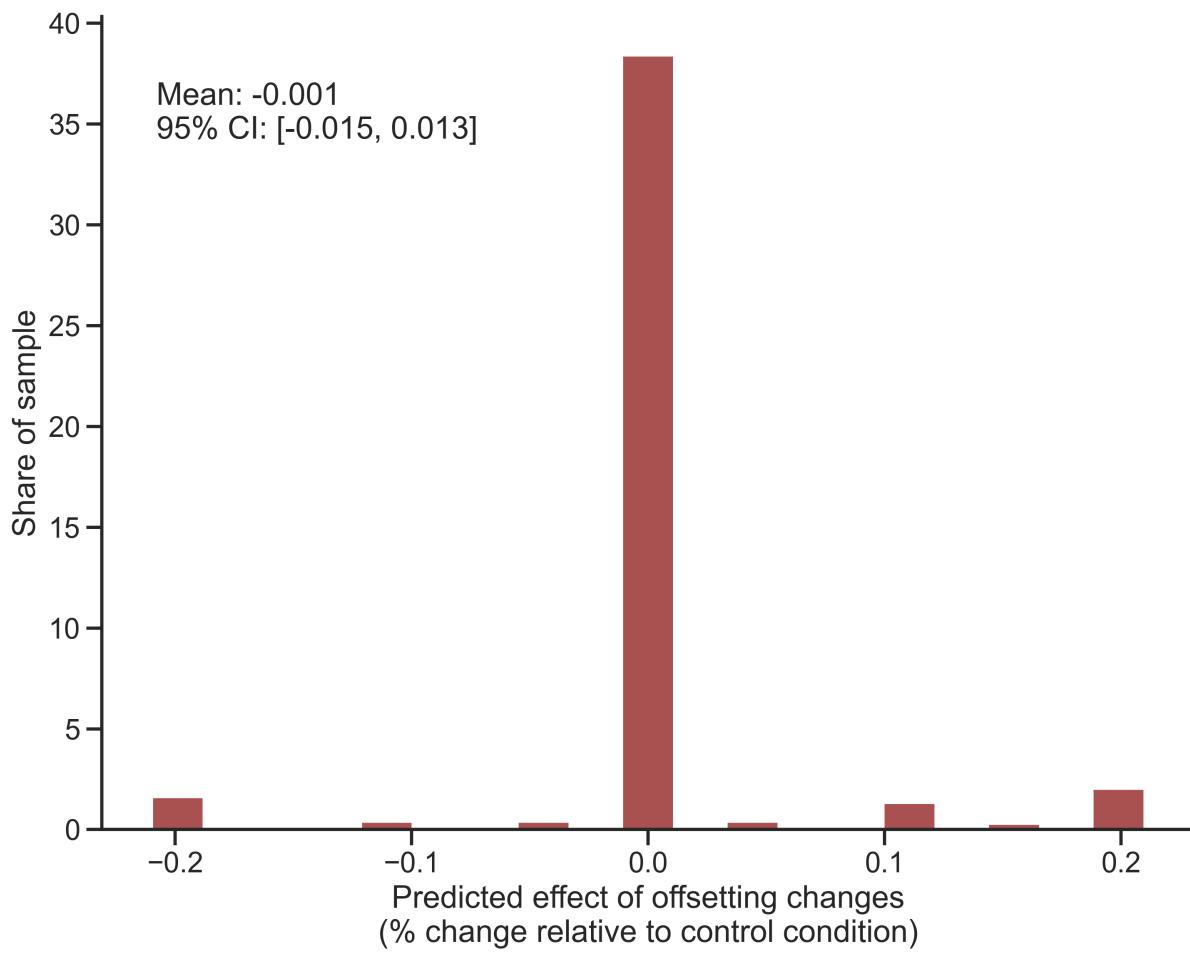
**Figure B.25:** Predicted effects of different kinds of price changes (weighted sample)

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

*Under construction. Will be complete by 10/25/24. [Link to latest version](#)*

### C.1 Tables and Figures



**Figure C.1:** 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  $[-0.2, 0.2]$ .

## 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 analyses 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.<sup>55</sup>

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

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<sup>55</sup>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.2, 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*** (0.114)	-0.823*** (0.165)	-0.781*** (0.156)	-0.629*** (0.126)	-0.416** (0.094)	-0.306*** (0.072)
Bonus × t=2	-0.132 (0.116)	-0.153 (0.168)	-0.149 (0.159)	-0.135 (0.129)	-0.122 (0.097)	-0.112 (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 <sup>2</sup>	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 version 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 eligibility criterion that participants must have placed wagers in Period 0). Columns differ in the restrictiveness of the winsorization bounds. Column 3 reports our pre-registered specification 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*** (0.115)	-0.604*** (0.178)	-0.524*** (0.151)	0.910*** (0.319)				0.208 (0.128)			
t=2 × Bonus	-0.128 (0.117)	-0.039 (0.152)	0.013 (0.164)	0.869*** (0.313)				0.170 (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*** (0.289)	0.477 (0.397)	0.953** (0.424)		0.189 (0.119)	0.087 (0.170)	0.220 (0.166)	
Above median t=0 wagers × Bonus					0.813 (0.584)				0.198 (0.238)		
Above median t=0 returns × Bonus						-0.147 (0.585)				-0.067 (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 <sup>2</sup>	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

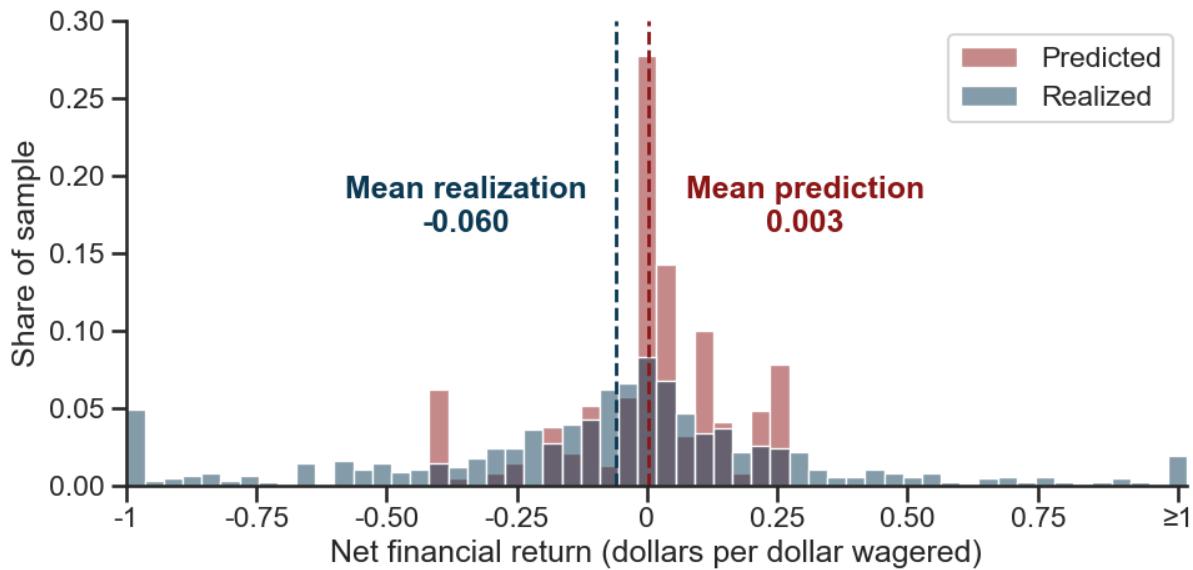
**Table D.2:** Pre-registered Bonus treatment effect regressions

Notes: The table contains all pre-registered Bonus treatment effects, as specified in section 3.4 of the pre-analysis 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), and responses to the “sports betting makes life better” survey question (defined in Section A), for columns (1-3), (4-7), and (8-11) respectively. All columns include the following covariates: a limit 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 measured on Survey 1 as an outcome. Columns (1), (4), and (8) implement regressions as specified in equation B.4 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 (0.107)	0.019 (0.153)	-0.047 (0.152)	0.093 (0.314)				0.223* (0.127)			
t=2 × Limits	0.140 (0.117)	0.086 (0.146)	-0.055 (0.169)	-0.275 (0.330)				-0.105 (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 (0.294)	0.202 (0.389)	-0.410 (0.426)		0.059 (0.119)	0.255* (0.149)	0.114 (0.170)	
Above median t=0 wagers × Limits					-0.560 (0.586)				-0.428* (0.240)		
Above median t=0 returns × Limits						0.612 (0.581)				-0.117 (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 <sup>2</sup>	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-analysis 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), and responses to the “sports betting makes life better” survey question (defined in Section A), for columns (1-3), (4-7), and (8-11) respectively. All columns include the following covariates: a limit 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 measured on Survey 1 as an outcome. Columns (1), (4), and (8) implement regressions as specified in equation B.4 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 contains 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

*Under construction. Will be complete by 10/25/24. [Link to latest version](#)*