Perceived cost of sampling new options predicts decision biases in economic contexts

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Data and code availability: <https://github.com/nicholasfurl/Model_fitting_hybrid_study>

Abstract

Considerable research has shown that people make biased decisions in “optimal stopping problems”, where options are encountered sequentially, and there is no opportunity to recall rejected options or to know upcoming options in advance (e.g., when flat hunting or choosing a spouse). Here, we use computational modelling to identify the mechanisms that best explain decision bias in the context of an especially realistic version of this problem: the full-information problem. We show that participants’ bias - The extent to which their sampling rates deviate from an optimality model - depends on the sequence length and the distribution of payoffs, while we find null effects for a variety of other hypothetical causes. We fit several Bayesian models of bias and a heuristic model and found participants’ biased sampling rates were best explained as participants perceiving option sampling as costly. We therefore propose a new theoretical viewpoint for the human solution to full information problems. Understanding the causes of such biases could enhance how we conduct real world sequential searches for options, for example how online shopping or dating applications present options to users.

Introduction

Often in everyday life, decisions must be made regarding options presented in sequence, like when attempting to find the best deal on a certain product or service. For such scenarios we can ask ourselves, when should someone stop evaluating new information and commit to a decision? This common real-life dilemma can be defined as an optimal stopping problem. For example, if one encounters a limited-time offer whilst shopping, should one accept it when it is available or miss it and wait for a better one? If a doctor needs a healthy organ for transplant, do they use what is available now or risk waiting for a healthier one? If an animal welfare charity is visiting homes to find a suitable environment to rehome an animal, do they accept the currently-visited home or continue to visit homes in hope of a better one? This general problem is often referred to as the "fiancé(e) problem", by analogy to decisions about whether to reject a current suitor in favour of meeting new ones in the future. We shall see below that solving many of these problems optimally is computationally challenging and that participants (when compared to the optimal solution) show systematic decision biases. Our aim here is to delineate the experimental contexts in which participants exhibit these biases and to fit theoretical models to participants’ choices to identify the computational mechanisms that give rise to these biases.

There are many types of optimal stopping problems and their potential computational solutions have been discussed in the fields of mathematics (Ferguson, 1989), behavioural ecology (Castellano et al., 2012; Castellano & Cermelli, 2011), economic decision making (Baumann et al., 2020; Seale & Rapoport, 1997, 2000), cognitive science (Lee, 2006) and neuroscience (Costa & Averbeck, 2015). The computational solutions considered for optimal stopping problems are closely related to probabilistic reasoning and explore/exploit foraging decisions (Averbeck, 2015) and other sequential tasks that involve prospective reward prediction (Kolling et al., 2018; Scholl et al., 2022). The availability of optimal computational solutions to these decision problems enables researchers to use them as “ideal observer models”, which can identify when people make suboptimal decisions, including decisions that reveal systematic biases.

We focus in the present study on a bias that arises for *“full information problems”*. This version of optimal stopping problem arguably most closely resembles real-world decision problems. Imagine an agent is searching for a new flat in a competitive market. The agent can sample a limited number of options in sequence (e.g., twelve flats can be viewed, one at a time) and decides, for each option, whether to stop sampling and choose that option, under the condition that rejected options cannot be returned to later (e.g., refused flats are then offered to others and so become unavailable). Full information problems can accommodate several further realistic complications that are not present in simpler optimal stopping problems. Flat hunters in full information problems can directly know the value of each option (e.g., how nice the currently-viewed flat is or how much it costs). Full information problems can also incorporate flexible payoff schemes (e.g., an agent might feel rewarded only if they achieve the best possible flat or their subjective reward might depend on the relative quality of whatever flat is chosen). Full information problems may involve a “cost to sample”. Each time a new flat is visited (i.e., a new option is sampled), our flat hunter may incur calculable costs such as time, money or effort, which may be subtracted from the final achieved reward value and so can limit how many options are sampled. Finally, full information problems allow agents to harness their prior belief about the probability distribution that is generating their decision options (i.e., the generating distribution). When flat hunting, consumers can use beliefs about the housing market to prospectively compute the probability that an even nicer flat might be sampled if the current one is refused.

Here, we will use computational modelling as well as experimental methods to test hypotheses about the causes of an “undersampling bias”, which to date has mainly been demonstrated and replicated for optimal stopping problems cast in economic scenarios in which options are represented as numbers (e.g., prices). When the sampling behaviour of ideal observers is compared to that of human participants, humans sample fewer options than is optimal (e.g., Baumann et al., 2020; Cardinale et al., 2021; Costa & Averbeck, 2015; Goldstein et al., 2020; Guan & Stokes, 2020). However, recently, new studies have reported full information problems associated with oversampling rather than undersampling (Furl et al., 2019; van de Wouw et al., 2022). These studies employed several different experimental and computational methods that hypothetically could affect bias. These include: the timing of option presentations, whether participants are able to leave the study earlier due to early stopping, the reward values assigned to differently-ranked choices, the number of options in each sequence and the use in the ideal observer of subjective values (participant ratings of prices). In later sections, we will describe all these features in further detail and will report the results of systematic manipulations of each to determine whether or not any of these features contribute to undersampling bias. To this end, we have adapted the economic task first reported by Costa and Averbeck (2015), such that participants attempt to choose high-ranking smart phone prices. To anticipate our findings: Undersampling bias became more pronounced when participants were rewarded only for the highest ranked options or when sequence lengths were longer. In both these cases, the ideal observer increased its sampling rate in response to these features to a greater degree than did participants. Indeed, although participants could detectably increase their sampling rates to some degree, they generally appeared more reluctant to increase their sampling rates than is optimal.

What computational mechanisms account for participants’ reluctance to increase their sampling rates? We created theoretical computational models, each with a free parameter designed to explain bias. We show (replicated across multiple studies and conditions) that participants’ sampling decisions on our economic task are best explained by a theoretical model with a biased cost to sample, compared to several other computational models that could theoretically explain bias. The reluctance that we observed for participants to increase their sampling rate when appropriate may therefore arise because participants perceive that sampling is an intrinsically costly activity.

General Methods

Paradigm summary

First, we briefly describe the features of our paradigms that are relevant for understanding the operations of our computational models. More specific methods for individual studies will be described in separate sections later.

We implemented full information optimal stopping problems in which participants attempted to choose the price of a mobile phone contract that they most preferred. Prices used for all studies reported herein were for flagship models by the top brands (e.g., iPhone, Samsung, Huawei), on an up to 5GB plan with unlimited texts and minutes. The 90 prices were actual prices (in GBP) of 2-year contracts offered by various UK retailers, as harvested from internet advertisements in the year before data collection. The use of these real-world prices was intended to maximise the likelihood that the distribution of option values used in our studies would approximate the “true” generating distribution of smartphone price options in the participants’ local market and thereby also approximate any prior expectations participants derived from their experience with smartphone contract prices.

In some conditions, the paradigm began with a “phase 1” ratings task, in which participants viewed the full distribution of prices that could appear as options later and rated each for its “attractiveness” or subjective value. As described below, some models operate over objective values / raw prices (OV) and other models operate on the subjective value of the prices (SV), derived from the ratings measured during phase 1. In phase1, participants also could learn the “generating” distribution of option values and thereby establish expectations about the probabilities with which certain option values might appear in any given sequence, later in the optimal stopping task. The distribution of these ratings could also be used to set the models’ prior on its generating distribution of option values (See *Ideal observer optimality model* section).

Next, in the optimal stopping task, participants engaged with several fixed-length sequences of option values, populated by prices sampled randomly, without replacement, from the Phase 1 generating distribution. In each sequence, participants sequentially encounter these prices and, for each, decide whether to reject that option value (rendering it no longer accessible) and sample a new one, or to take / choose that option value, which stops the search through the sequence and renders the upcoming new price samples no longer accessible. If the last price in a sequence is reached, that price became their choice by default.

Ideal observer optimality model

To analyse the optimal stopping task, we compared the number of options sampled by our participants before choosing an option to that of an ideal observer model. The ideal observer model is a benchmark of optimality, for which performance is Bayes-optimal. This finite-horizon, discrete-time, Markov decision process (MDP) model has been used in previous studies (Cardinale et al., 2021; Costa & Averbeck, 2015; Furl et al., 2019; van de Wouw et al., 2022). The Bayesian version of the full information problem optimality model builds on the classic Gilbert and Mosteller model (Gilbert & Mosteller, 1966). Expectations about option values are derived from the model’s belief about the distribution from which future options are assumed to be generated (i.e., the generating distribution). More precisely, the utility *u* for the state *s* at sample *t* is the maximal action value *Q*, out of the available actions *a* in *A*, which in turn depend on the reward values *r* and the probabilities of outcomes *j* of subsequent states (i.e., the generating distribution), weighted by their utilities.

The terms appearing inside the curly brackets are taken collectively as the action value *Q*. is the reward that will be obtained in state *s* at sample *t* if action *a* is taken. The model described here reduces r by costs incurred by sampling again using a “cost to sample” penalty term *C*. See formula for below. As there was no extrinsic cost-to-sample in any of our experimental designs herein, *C* was always fixed to zero for the ideal observer model. The integral is taken over the possible states subsequent to the current sample. Each of these states is weighted by the probability of transitioning into it from the current state, given by , as derived from the generating distribution.

The utilities for sampling again are computed based on backwards induction. The model first considers the utility for the final sample *N* in the sequence, which is simply the reward value associated with the *N*th state (because taking the option is the only available action for the final sample in a sequence).

Next, the model works backwards through the sequence, iteratively using the aforementioned formula for when computing each respective action value *Q* for taking the option and declining the option for each *t*. Whenever the reward value of taking the current option is considered, the reward function *R* assigns reward values to options based on their ranks. *h* represents the relative rank of the current option.

In contrast, the reward value of sampling again is simply the cost to sample *C*.

This customisable *R* function allowed us to examine how the ideal observer model changes its sampling strategy under the different reward payoff schemes used in our studies. In Pilot full, the full condition of Study 1, Study 3 and both sequence length conditions of Study 4, participants were instructed to try to choose the best price possible. To match these instructions, we implemented a continuous payoff function (resembling that of the classic Gilbert & Mosteller formulation), in which each relative rank would be rewarded commensurate with the value of its associated option. In Pilot baseline and the baseline, squares, timing, and prior conditions of Study 1, we implemented the payoff scheme to match participants’ instructions that they would be paid £0.12 for the best rank, £0.08 for the second best rank, £0.04 for the third best rank and £0 for any other ranks. Lastly, in the payoff condition of Study 1, we programmed the reward payoff function to match participants’ reward of 5 stars for the best rank, 3 stars for the second best rank, one star for the third best rank and zero stars for any other ranks.

Another feature added to our implementation of the ideal observer, compared to the Gilbert & Mosteller base model, is the ability to update the model’s generating distribution from its experience with new samples in a Bayesian fashion, instead of this generating distribution being specified in advance and then fixed throughout the paradigm. Our Bayesian version of the optimality model treats option values as samples from a Gaussian distribution with a normal-inverse-*χ2* prior. Before experiencing any options, the prior distribution has four initial parameters: the prior mean *μ0*, the degrees of freedom of the prior mean *κ*, the prior variance *σ*20 , and the degrees of freedom of the prior variance *ν*. This initialised distribution plays the role of a prior generating distribution when the first option value is sampled. The *μ0* and *σ*20 parameters of the generating distribution are then updated by the model following presentation of each newly sampled option value as each sequence progresses.

Here, we set the prior values of *μ* and *σ*2 in two possible ways (IO OV and IO SV, as described below). In previous studies, the mean and variance of the generating distribution has been fixed in advance by the mean and variance of the empirical option value distribution (e.g., Baumann et al., 2020), sometimes under the assumption that participants will have experience with this distribution prior to the study (Cardinale et al., 2021; Costa & Averbeck, 2015). We implemented the ideal observer objective values model (IO OV) procedure to all the study conditions reported herein, whether or not participants were familiarised with the distribution of potential price options in an initial phase. This OV procedure assumes that the raw prices can be treated as a proxy for participants’ subjective value of the prices, and that all participants have equivalent subjective price valuations, and so an IO model that optimises only the raw prices when making decisions would therefore be an appropriate basis for comparison with participants.

However, we also had access to subjective values of options in some conditions, due to the presence of the initial rating phase (Pilot full, Study 1 full condition, Study 1 ratings condition, Study 2 and both sequence length conditions of Study 3). We considered here that participants’ subjective valuation of prices may not exactly equal the raw price values, especially in their scaling, which may be relevant to full information problems. For conditions which had a ratings phase, we therefore also computed a second version of the ideal observer, IO SV. In the conditions for which we had subjective values from the initial phase available, we used each participants’ individualised ratings (subjective valuations) of the prices as option values input to IO SV, and we used the mean and variance of individual participants’ ratings distributions when initialising the prior of the generating distribution of the ideal observer model.

Because conditions with an initial rating phase had two versions of the ideal observer model, each providing separate optimality estimates (IO OV and IO SV), we were able to ascertain whether use of objective or subjective values affects the strategy taken by the optimality model and, consequently, whether it changes the assessment of participant bias. We ensured for both OV and SV models that better options were always more positively-valued such that the models were always solving a maximisation problem. We further ensured that estimated parameters for OV and SV models would be on the same scales. We achieved this by reflecting the prices around their mean. Then we rescaled the values to span 1 (the highest / worst price) to 100 (the best price). These reflected and rescaled objective values were then used in OV models when computing the prior generating distribution, and when inputting price values to the model as option values,

Theoretical models

The purpose of the ideal observer model described above was to assess bias, not to theoretically explain participants’ bias. By definition, the parameter values of an ideal observer model is fixed to ground truths established by the experimental design. Because of this feature, however, ideal observer models are not appropriate for use as theoretical models of potentially-biased human sampling and choice behaviour, without modification added to account for sources of individual variability in bias. That is, the ideal observer only models the computations leading to accurate choices but not to systematic sources of error, like oversampling or undersampling. To better understand which computations might be responsible for participants’ biased choices, we formulated a number of theoretical models and fitted them to participants’ take option versus sample again choices. As mentioned above with respect to the ideal observer model, some previous studies have implemented models which aim to optimise the objective values of choices (e.g., Baumann et al., 2020; Cardinale et al., 2021; Costa & Averbeck, 2015; Lee, 2006) while other model implementations optimise subjective values of those options, obtained via a separate rating task (Furl et al., 2019; van de Wouw et al., 2022). Because there is no obvious determination of which procedure is correct, we implemented both objective values (OV) and subjective values (SV) versions of all our theoretical models, whenever a study condition involved a preceding rating task that enabled both model implementations. Then, we could assess using model comparison whether OV or SV models best fit human participant choices, or whether OV and SV models are relatively interchangeable (as we in fact discovered, see Results).

For every sample, the probabilities of the two available choices (take current option versus sample again) were computed by transforming action values from each model to probabilities using Softmax and then summing negative log likelihoods over choices for each participant. In each model, we freed one theoretically interpretable key parameter (these free parameters and their models are described below) and the inverse temperature parameter beta from the Softmax function (the starting value for beta was always 1 and the fitting of beta was bounded between 0 and 100). Variability in each of the key theoretical parameters was confirmed during parameter recovery to be capable of modulating the sampling rate (Supplementary Procedures Text A and Supplementary Figure S2 and upper panel of S3). The two free parameters per model were fitted using fminsearch.m in MATLAB (Mathworks, Natick MA). Parameter recovery analyses of all the models described below showed at least adequate correlations between configured and recovered parameters (Figure S1), although strong correlations were observed for some models, including the cost to sample model, which is the model that will form the basis of our main conclusions. We also found strong correlations between sampling rates associated with configured parameters and sampling rates associated with recovered parameters (Supplementary Procedures Text A and Supplementary Figures S2 and S3). We implemented two parallel model comparison methods based on negative log likelihood values converted to Bayesian information criterion (BIC) values. For the first model comparison method, we submitted the BIC values to repeated measures pairwise statistical tests using Bayes factors to ascertain whether pairs of models differed or had equivalent BIC values on average over participants. The best model would then show the (statistically) lowest BIC mean value. For the second model comparison method, we computed which model had the lowest (best) BIC for each participant and then plotted histograms to ascertain which model(s) dominated the others in terms of participant “wins”. The model that best-fit the most participants presumably was the sampling strategy most often used by participants in our sample.

The objective and subjective values versions of the *“cut off” heuristic (CO OV and CO SV)* is a heuristic derived from the mathematically-optimal solution to the “Secretary problem” (Ferguson, 1989), an optimal stopping problem with a relatively simple mathematical solution due to the fewer assumptions made compared to the full information problem. The secretary problem solution assumes participants use no prior knowledge of the generating distribution, considers only relative ranks of option values and agents are rewarded only when choosing the top-ranked option. Although this heuristic derives from the optimal solution to a different optimal stopping problem than the full information problem we consider here, Todd & Miller (1999) propose that this heuristic might be robust to violations of the secretary problem assumptions and, as a heuristic, would be relatively simple for humans to compute on the fly in realistic settings. More specifically, Todd & Miller (1999) propose that such a CO model can explain undersampling bias as the model can perform nearly-optimally (on secretary problems) while incurring fewer samples, which “satisfices” under conditions where there is a cost to sample (note that the CO model has no cost to sample parameter). This heuristic has previously been fitted to human behaviour on full information optimal stopping problems, although little evidence was found favouring it in that study (Baumann et al., 2020). In the theoretical CO models we implemented, the model chooses to sample again for every option until it reaches a cut-off, where the sequence position of the cut-off is fitted as the key theoretical free parameter. Then, the model continues to sample until it reaches the next option with the highest relative rank. Here, we used the optimal cut-off value (37% of the sequence length, rounded to the nearest integer) as the starting value during model fitting and the parameter search was bounded between 2 and the sequence length minus 1 (as the learning period defined by the cut-off must contain at least one sample and be followed by at least one sample available for choice). Cut-off values below the optimal one lead to undersampling and cut-off values above the optimal value lead to oversampling.

We also considered objective and subjective values versions of *the cost to sample model (CS OV and CS SV)*. Like all the other models described below in this section, CS OV and CS SV use the Bayesian ideal observer described above as a base, but they assume that participants’ otherwise rational Bayesian computations can be biased by a free parameter value. In the case of the CO OV and CS SV models, the fitted parameter to account for such bias was the cost to sample value *C* (See computation of in the Ideal Observer Optimality Model section above. In such a model, participants would undersample if they intrinsically perceived sampling as costly and so adopted a negatively-valued *C,* and would oversample if they perceived sampling as rewarding as so adopted a positive *C*. We initialised model fitting with a starting *C* value of 0 (i.e., the optimal value) and, during fitting, bounded *C* to be between -5 and 5.

We used a similar approach when building *the optimism model (O OV and O SV)*. In this model, we added a new free parameter to *μ*, the mean of the posterior generating distribution. This additional constant alters the mean value after it is updated by the current sample value and before the use of this posterior generating distribution to compute utilities . Negative values of this parameter can bias an agent to compute pessimistic estimates of future option values by shifting the posterior mean (i.e., expectation) to be lower. This can lead to undersampling by making the current option appear more appealing compared to the artificially-deflated expectation of option values resulting from continued sampling. Conversely, positive values of this parameter encourage oversampling, as the agent would have too optimistic an expectation of future option values to be gained by continued sampling. We initialised model fitting with a starting value of 0 (i.e., the optimal value) and the optimism parameter was bounded during fitting to be between -100 and 100.

In the *biased values model (BV OV and BV SV)*, we considered the possibility that, although participants may use the optimal solution to solve the task, they might instead be biased to misperceive the magnitudes of the option values that are input into this optimal solution. This might especially be the case if participants perceive only the very most valued options as worthy of consideration at all, as might be the case in “high threshold” models of optimal stopping in mate choice (Furl et al., 2019; Valone et al., 1996). Here, we passed the option values through a logistic function prior to input as option values to the ideal observer, which effectively thresholds the option values such that option values less than the midpoint parameter of the logistic function are roughly minimal and option values above this midpoint are roughly maximal, leaving only option values above an input value threshold as eligible for consideration by the ideal observer. We fixed the logistic slope to equal .2 (on the basis of successful exploratory parameter recovery using this value) while freeing and fitting the midpoint parameter / threshold of the logistic function. We picked the centre of the input value range (i.e., 55)as the starting value for the free logistic midpoint parameter when fitting to participants’ choices and bounded the parameter fitting to be within the option value range 1 to 100.

The biased rewards model (BR OV and BR SV) is based on similar logic as BV. However, instead of assuming participants place a threshold on the option values being input to the model, we instead assumed such a threshold on the reward function *R* (See formula for above). Recall that this function assigns reward payoffs to choice relative ranks. As with BV, we passed the option values through a logistic function, with slope = 1 (based on experience with parameter recovery), with the logistic midpoint parameter as the free parameter. Then, the transformed values were assigned as reward payoff values in place of the ones otherwise suitable for the model (See the *Ideal Observer Optimality Model* section of the *General* *Methods* for more information on how reward payoffs are otherwise implemented in these models). Increasing this midpoint parameter value / reward threshold leads to increased sampling while decreasing this value leads to decreased sampling. As in the BV model, the starting value of the midpoint parameter was initialised at 55 and fitting was bounded between 1 and 100.

Pilot Studies Methods

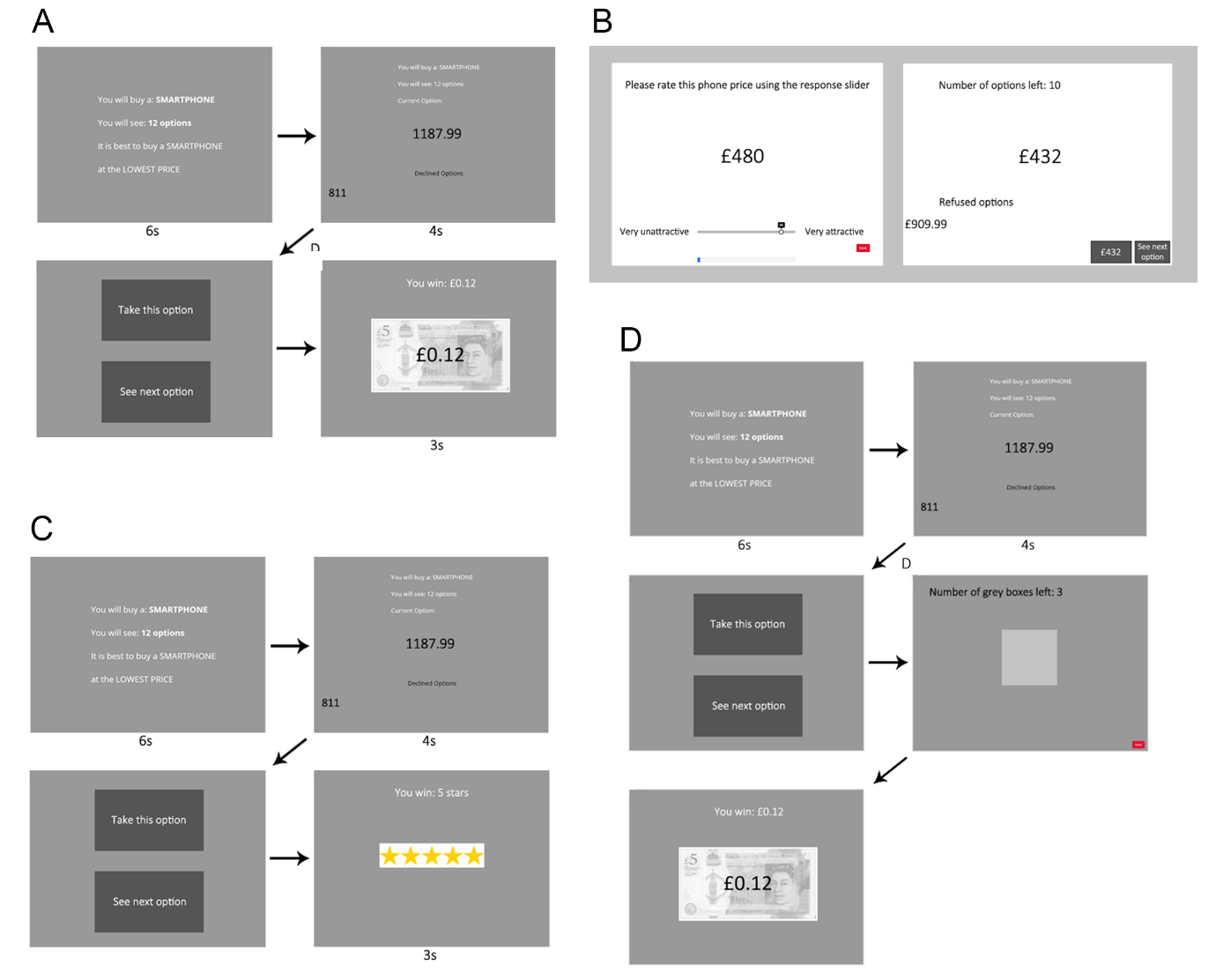
Participants

We recruited participants in both our pilot studies from the United Kingdom using the online data collection platform Prolific (Prolific, 2014). We enrolled 50 participants into Pilot Baseline and 51 participants into Pilot Full.

Procedures

Gorilla Experiment Builder (Anwyl-Irvine et al., 2020) was used to create and host the Pilot baseline and Pilot full studies. For the Pilot baseline study, we were interested in whether we could replicate previously observed participant undersampling bias (Cardinale, et al., 2021; Costa & Averbeck, 2015), in which participants sampled fewer options before decision relative to the Bayesian ideal observer model. Therefore, we designed a paradigm that matched Costa and Averbeck (2015) in its methods particulars as closely as was practical, while concomitantly adapting the paradigm for an online data collection setting. Consequently, there was no phase 1 ratings task in Pilot baseline. In the optimal stopping task (Figure 1A), participants attempted to choose one of the top three ranked smartphone prices out of each option sequence. The option value screen also presented the previously-rejected option values and the number of options remaining in the sequence. Each of the seven sequences used an order of 12 option values, which was fixed in advance, so a given sequence’s option values and their order within the sequence was identical for every participant (and corresponding models), although the sequences themselves were intermixed randomly.

Figure 1. Paradigm designs used in pilot studies and Study 1. (a) Pilot baseline study and Study 1 baseline condition. (b) Pilot full and Study 1 full condition. (c) Study 1 payoff condition. (d) Study 1 squares condition.



Like Costa and Averbeck (2015), we also rewarded participants financially for choosing one of the top three options in the sequence. Participants in Pilot baseline earned £0.12 per sequence if they chose the best price in the sequence, £0.08 if they chose the second best price, £0.04 if they chose the third best price, and £0 if they chose any other option. These performance-based bonus payments were earned on top of a flat fee, which for all our studies was set in line with Prolific’s recommended pay of £7.50 per hour (participants typically finished the study in considerably less time than an hour). Once a choice was made, participants viewed a feedback screen that informed them of their winnings for that sequence. The paradigm utilised fixed screen timings, meaning that participants automatically advanced through the screens, except when asked to make a decision (‘Take this option’ or ‘See next option’). Participants were warned about this feature in the instructions preceding the task.

For Pilot Full, we were interested in whether participant undersampling bias would continue to replicate using the same economic smartphone price task, but when implementing the “full” complement of methods particulars adapted from studies that revealed oversampling bias instead of undersampling bias (Furl et al., 2019). The logic is that, if any of these methods features is responsible for the oversampling bias seen in these earlier papers, then Pilot full should produce an oversampling bias, which would contrast with the undersampling bias we expected to see in Pilot baseline.

Pilot full added an initial ratings phase (Figure 1B), in which participants rated the “attractiveness” of the price, defined in the instructions as a willingness to purchase a phone at that price. Ratings were made by mouse click on a sliding scale from 1 to 100, with the slider only appearing after the first click - to avoid slider biases (Matejka et al., 2016) - with the selected rating value shown above the slider. Participants rated 180 prices, presented one at a time in a random order, and comprising the 90 unique prices, each rated twice. The average over the two ratings for each price was then used as the subjective value input to the SV versions of the models. In Pilot full, the mean (over participants) Pearson’s correlation coefficient between the two ratings was .83. A blue progress bar was shown continuously at the bottom of the screen to visualise participants’ progression through the ratings phase.

The optimal stopping (second) phase of Pilot full (Figure 1B) included five sequences of 12 option values each. As in Pilot baseline, the option values in each sequence were fixed in advance but the sequences’ order was randomised. Unlike Pilot baseline, once participants chose one of the options, they then had to advance by button press through a series of grey squares that replaced the remaining options in that sequence. This was intended to discourage participants from finishing the study early by choosing earlier options. Also unlike Pilot baseline, the optimal stopping task was entirely self-paced - participants advanced by using their mouse to click on the buttons on the screen. After finishing a sequence, participants were directed to a feedback screen displaying their chosen price and the text: "This is the price of your contract! How rewarding is your choice?". Participants responded to this question using a slider scale ranging from not rewarding (1) to very rewarding (100). The purpose of this rating activity was only to provide feedback to the participants about the quality of their choices, in lieu of the bonus payoff screen in Pilot baseline, and to encourage participants to reflect upon the choice’s reward value before moving on to the next sequence. These ratings do not provide hypothesis-relevant data and were not analysed. Participants were reimbursed a flat fee only - no bonus monetary payoff was awarded.

Pilot Studies Results and Discussion

As the two pilot studies are separate studies, with data collected at somewhat different times, we will descriptively, rather than statistically, compare them. Figure 2 shows the mean number of samples to decision made by human participants for both of the pilot studies, which yielded similar numbers of samples, with a slight numerical increase for Pilot full.

Figure 2. Human participants’ numbers of samples to decision for all studies. Significant pairwise differences between condition means within a study are shown as green horizontal lines (*p* < .05, multiple comparison corrected for the number of pairs in that study), which appear only for the Study 3 sequence length conditions. Magenta horizontal lines connecting pairs of bars show conditions within each study where *BF*01 > 3 (i.e., moderate evidence favouring a null model with equal means). No pairs with *BF*10 > 3 were found.

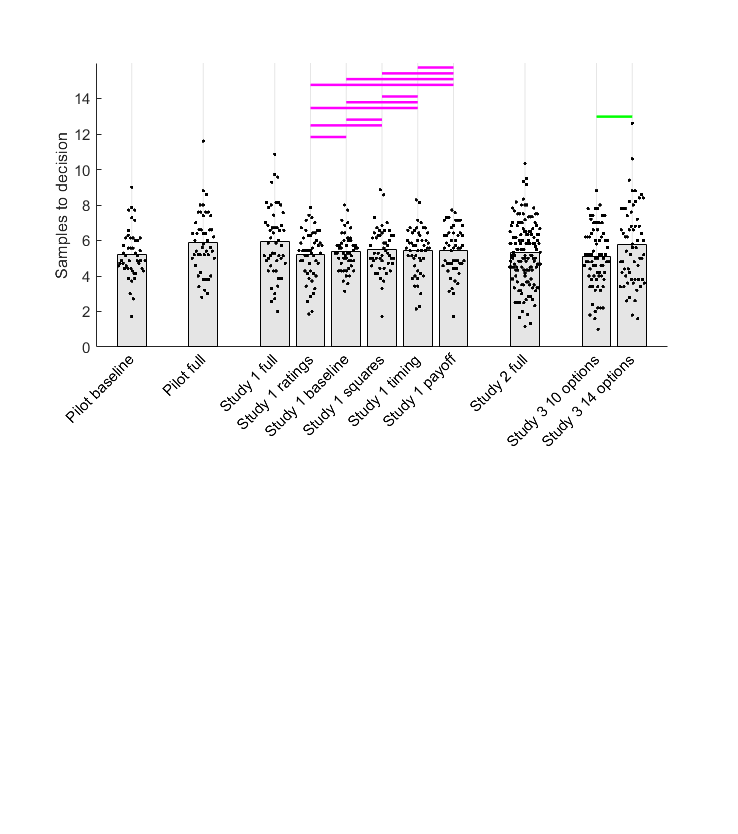


Figure 3. Model comparison for Pilot baseline (left column) and Pilot full (right column). First and second rows show individual participants as points and bars show their mean values In the first row, horizontal lines above human and IO samples data indicate in thin black when *BF01* > 3 (moderate evidence for equal means) or in thick grey when *BF10* > 3 (moderate evidence for different means). Human participant data are the same as in Figure 2. The second row shows BIC values (lower values indicate better model fit) for participants (points) and their mean values (bars). Horizontal lines are shown in black when *BF01* > 3 or in the colour corresponding to the better model when *BF10* > 3. For Pilot baseline, the abundant light green lines suggest that CO is the superior model. For Pilot full, the abundant orange and light green lines suggest that CO and CS are the best models. The third row plots the counts of participants for which each model was the best-fitting, which corroborate the results in the second row. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

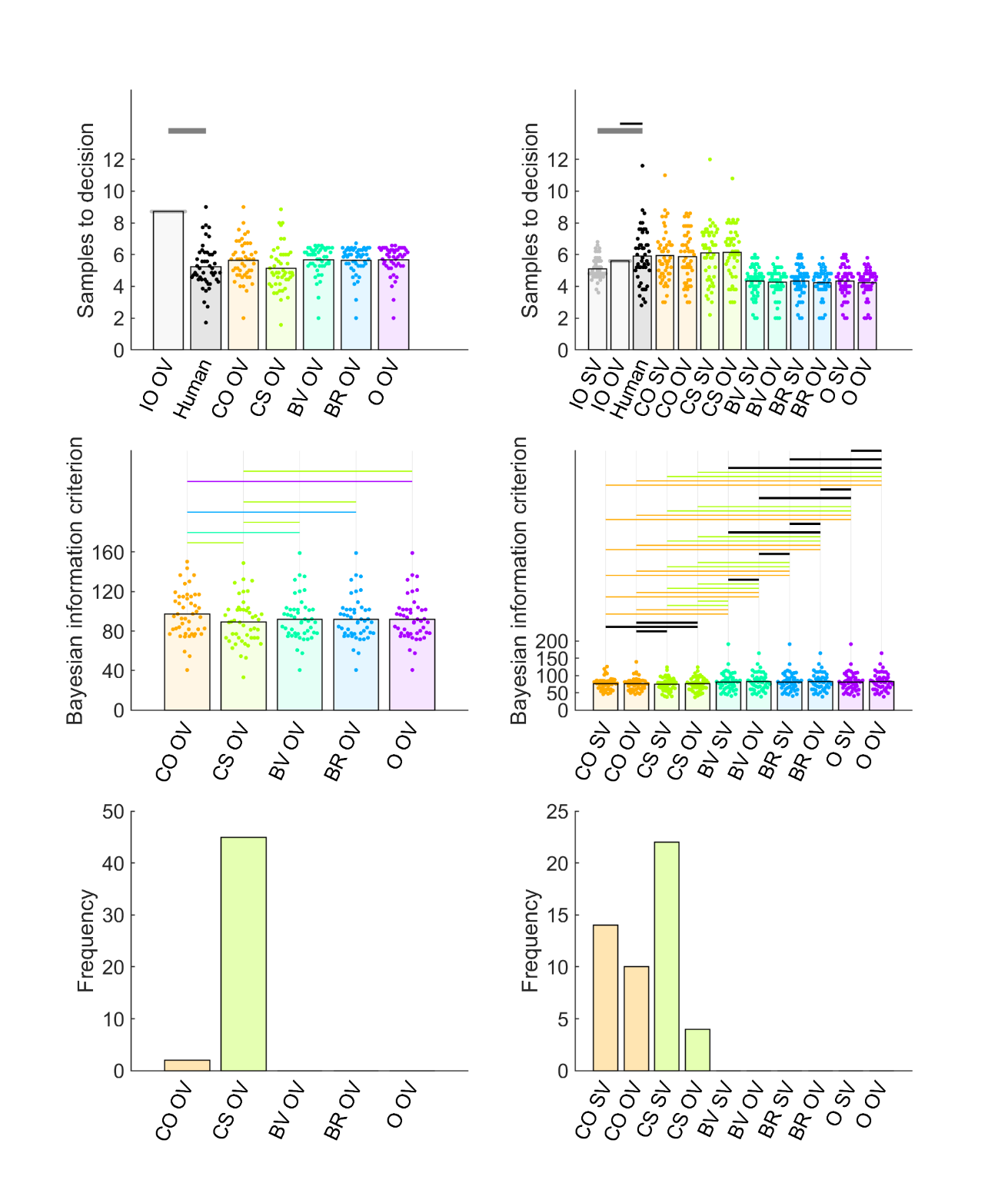


Figure 3 shows results from the comparison of human participants’ sampling (reproduced from Figure 2) with sampling of the ideal observer and theoretical models. As expected, we successfully replicated undersampling in the Pilot baseline condition (Figure 3, left column), where participants sampled fewer options than the ideal observer (Cohen’s *d* = -2.52). All the theoretical models, after fitting to Pilot baseline data, resembled the participants to some degree, as they all showed some degree of undersampling, compared to IO OV. Nevertheless, the CS model most closely approximated the human participants’ exact mean samples (Figure 3, left column, first row). Moreover, CS was the best predictor of individual participants’ sampling rates (Figure S5). Model comparison using BIC scores provided strong evidence that the CS model better fit participants than other models. Bayesian pairwise tests (Figure 3, left column, second row), showed that CS had significantly lower (better) BIC values than any other model. And CS was the best fitting model in almost every individual participant (Figure 3, left column, third row).

For Pilot full, in contrast to Pilot baseline, there was no clear undersampling bias. Instead, the ideal observer sampled at a reduced rate, compared to Pilot baseline, while participants sampled at approximately the same rate (Compare light grey bars across first row of Figure 3). Participants’ sampling was consequently statistically equivalent to optimal sampling, when compared to IO OV (Cohen’s *d* = .17), the same optimality standard used in Pilot baseline. Moreover, when compared to IO SV, there was even a weak (Cohen’s *d* = .45) oversampling effect (Figure 3, top right). An explanation for the IO models reducing their sampling rates in Pilot full compared to IO sampling in Pilot baseline is the difference in the reward payoff function. All relative ranks of choices were rewarded to some degree in Pilot full, depending on the magnitude of the option value, but only the top three ranks were rewarded in Pilot baseline. None of the other methods differences between Pilot baseline and Pilot full (e.g., the presence of a first phase, grey squares, timed screen advances, etc.) change how IO models implement their computations and so cannot change the IO model’s sampling rate. Moreover, neither OV nor SV versions of the IO in Pilot full sampled as much as the IO OV model Pilot baseline, nor did IO OV produce undersampling in Pilot full as it did in Pilot baseline, suggesting that the use of subjective versus objective values when modelling is not sufficient to change model behaviour enough eliminate the undersampling bias either. In study 1, wthismore rigorously: that the reward payoff scheme modulates undersampling bias by affecting the sampling rate of the ideal observer, while the participant sampling rate may be less affected

The weight of the evidence in Pilot full suggests that, as in Pilot baseline, CS is the theoretical model that best explains participants’ variability in bias. Although both CS and CO models outperformed the other models in terms of approximating participants’ sampling rates (Figure three, top right) on average BIC (Figure 3, middle right) and the number of best-fitting participants (right panel in the third row of Figure 3), the CO models failed to approximate participants’ ranks achieved (Figure S4 in Supplementary Materials). Subsequently studies reported herein will and, to foreshadow,

Study 1

The paradigm design that we adapted to use in Pilot baseline was taken from Costa & Averbeck (2015) and resulted in findings of undersampling like that study. However, we adapted many of the design features for Pilot full from a study that showed *over*sampling (Furl et al., 2019) and we found that this design eliminated the undersampling bias by changing the sampling rate of the IO optimality model. This pattern raises a distinct possibility that at least one of these methods differences might affect the nature of sampling bias.

Study 1 was therefore designed to put this possibility to the test by using six conditions to systematically vary the aforementioned methods differences and then analysing whether they affect (1) sampling performance of participants and (2) of the OV and SV versions of the IO model. Our first hypothesis was, because participants’ samples to decision were so similar between Pilot baseline and Pilot full, that these methods differences would not substantially change participants’ number of samples to decision in Study 1. Because of the possibility that we might need to interpret null effects (where participants sample at equal rates in different conditions), we implemented Bayesian tests using null (equal means) models (Figure 2). Our second hypothesis was that only the reward payoff scheme (which was different in the full condition than the other conditions) would affect IO sampling rates and that none of the other manipulations of task features would. And finally, for our third hypothesis, the theoretical model that best explains participants’ sampling biases should continue to be CS.

Study 1 Methods

Participants

As in the pilot studies, participants in Study 1 were enrolled from Prolific’s pre-screening facility to ensure that all participants were residents of the United Kingdom, to maximise familiarity with current UK smartphone market prices, denominated in GPB. We enrolled independent participant samples into each of six conditions (See Procedures), targeting fifty participants in each condition (chosen on the basis of our pilot studies, whose sample sizes proved sufficient to discriminate participant and IO sampling rates). However, because of a technical difficulty with the participant recruitment platform, we overshot our data collection target by two participants, one in the timing condition and one in the ratings condition.

Procedures

The study was developed using the experiment hosting software Gorilla Experiment Builder (Anwyl-Irvine et al., 2020). We implemented six conditions in Study 1, which systematically manipulated the presence or absence of four key task features. These features are summarised in the rows of Table 1 and Figure 1 visualises the paradigm designs for Study 1 baseline (Figure 1A), full (Figure 1B), payoff (Figure 1C) and squares (Figure 1D) conditions. Next, we will cover each condition in turn.

The *baseline condition* (Figure 1A) was nearly identical with the Pilot baseline study, except that it implemented seven sequences instead of five. That means that, like Pilot baseline, Study 1 baseline adapted its methods from Cardinale et al. (2021) and Costa and Averbeck (2015). It is “baseline” in the sense that it possesses none of the new methodological features from Furl et al. (2019) under test here, and it will serve as the basis for comparison against the other conditions, which each add one or more of the methodological features. Similar to Pilot baseline, we fixed in advance the option values and their order within each of the sequences, and then these fixed-option sequences were presented in random order. However, in this case, to avoid as homogenous a set of sequences as was used in Pilot baseline, we created 10 such fixed sets of sequences and each participant was randomly assigned to one of these sets. This procedure was implemented in Study 1 baseline and in all the conditions based on it, described below (i.e., ratings, payoff, squares, timing). The *full condition* was identical to the Pilot full study (Figure 1B), except that it used seven sequences instead of five. The mean (over participants) Pearson’s correlation coefficient between the two ratings for each price collected in the first phase was .87. The *ratings condition* was the same as the baseline condition with the exception that it added the same initial rating phase as in the Pilot Study and full condition (Figure 1B), while using the same optimal stopping task as the baseline condition (Figure 1A). In this condition, the correlation between the two ratings for each price (on average over participants) was .81. The *payoff condition* (Figure 1C) was the same as the baseline condition with the exception that participants did not receive the monetary incentivisation that they did in the baseline condition. Participants were instructed to make choices to maximise the number of stars. Then, instead of receiving feedback regarding their earned bonus payments on the feedback screen (as in the baseline condition), participants were shown pictures of the number of stars that they earned for their choice: either five stars, three stars or one star, if they chose respectively the best, second best, or third best price in the sequence. The *squares condition* (Figure 1D) was the same as the baseline condition with the exception that, once participants had chosen an option that was not the last option, they had to press a key to advance through grey squares that replaced each forgone option until the end of the option sequence. The *timing condition* was the same as the baseline condition with the exception that it incorporated a ‘next’ button in the top right corner of every option screen. This ensured that the entire paradigm was self-paced.

Table 1. Summary of conditions for Study 1

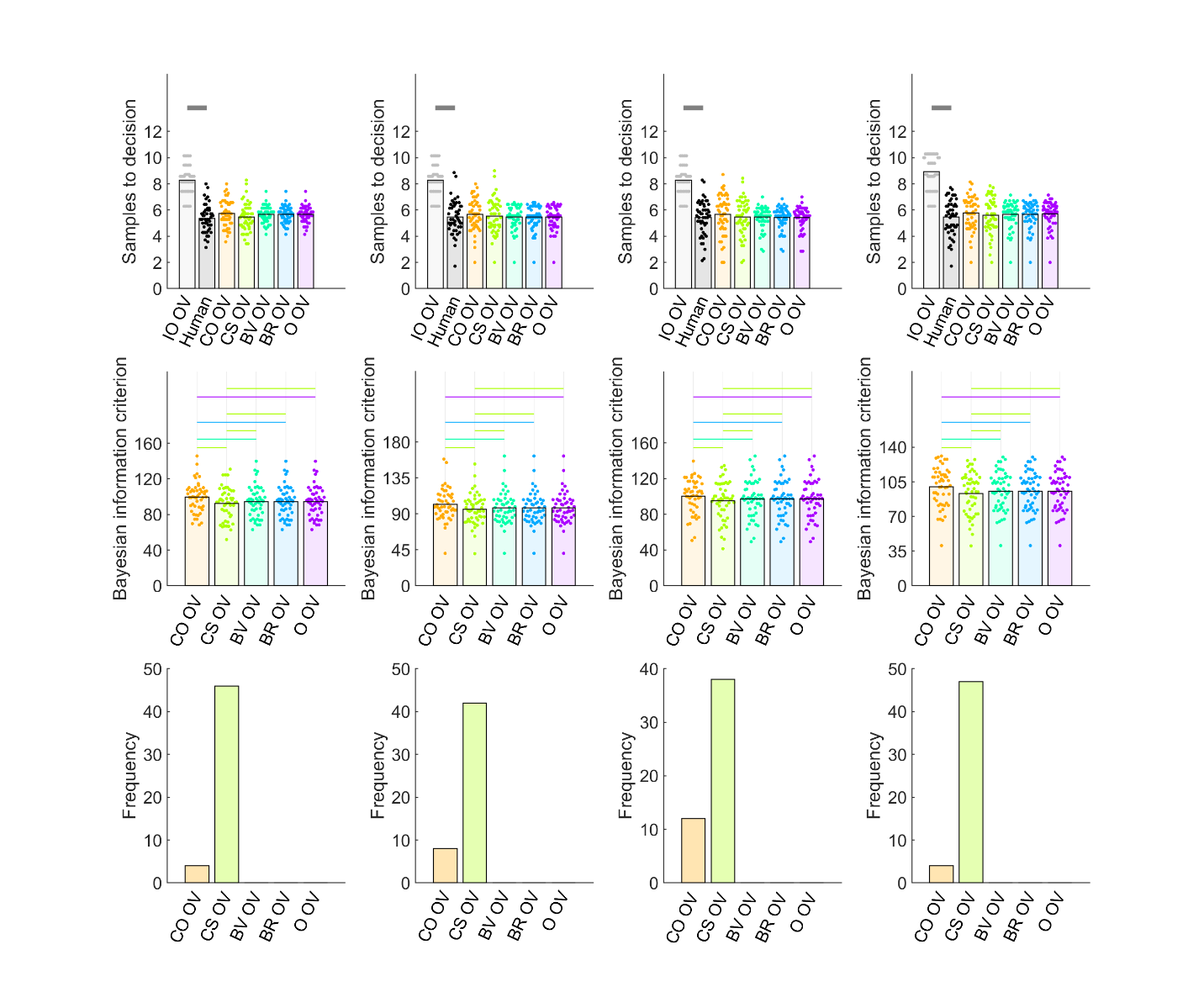
|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  |  | Study 1 condition name | | | | | |
|  |  | Baseline | Full | Squares | Payoff | Timing | Ratings |
| Task feature | Grey squares |  | × | × |  |  |  |
| No monetary payoff |  | × |  | × |  |  |
| Self-paced timing |  | × |  |  | × |  |
| Rating phase |  | × |  |  |  | × |

Study 1 Results and Discussion

First, we tested our first hypothesis: that none of the conditions would affect participants’ number of samples to decision. Similar to what we found with our pilot studies (Figure 2), there was a slightly higher number of participants’ samples in the full condition than any of other conditions. However, neither pairs of conditions including the full condition, nor any other pair showed a “significant” statistically-substantiated mean difference either by frequentist tests (using threshold *P* < .05, after multiple comparison corrected for the 15 condition pairs) or by Bayesian *t*-tests (using threshold *BF10* > 3, moderate evidence in favour of mean difference). According to these Bayesian *t*-tests, nearly every pair of conditions showed statistically equivalent means, (all *BF01* > 3, moderate evidence in favour of null model and shown as magenta horizontal lines in Figure 2), with the only exceptions being the comparisons with the full condition, which were statistically inconclusive. Cohen’s *d* values for these comparisons are visualised in Figure S7 in the Supplementary Materials. These results confirm our first hypothesis.

We next compared participants’ number of samples against those of the IO optimality models, to evaluate decision bias. The first row of Figure 4 shows Bayesian pairwise tests (threshold *BF10* > 3, moderate evidence for different means) from the studies without any first phase, comparing participants’ sampling (black points) against that of the IO OV model with a payoff structure that rewards only the top three ranks (grey points). We found nearly-identical undersampling bias in the baseline (Cohen’s *d* = -2.01), squares (Cohen’s *d* = -171), timing (Cohen’s *d* = -1.74) and payoff (Cohen’s *d* = -1.96) conditions. Thus, as we predicted as part of our second hypothesis, neither participant performance nor IO performance nor the undersampling bias appears sensitive to the presence or absence of grey squares, self-advanced timing or extrinsic monetary reward.

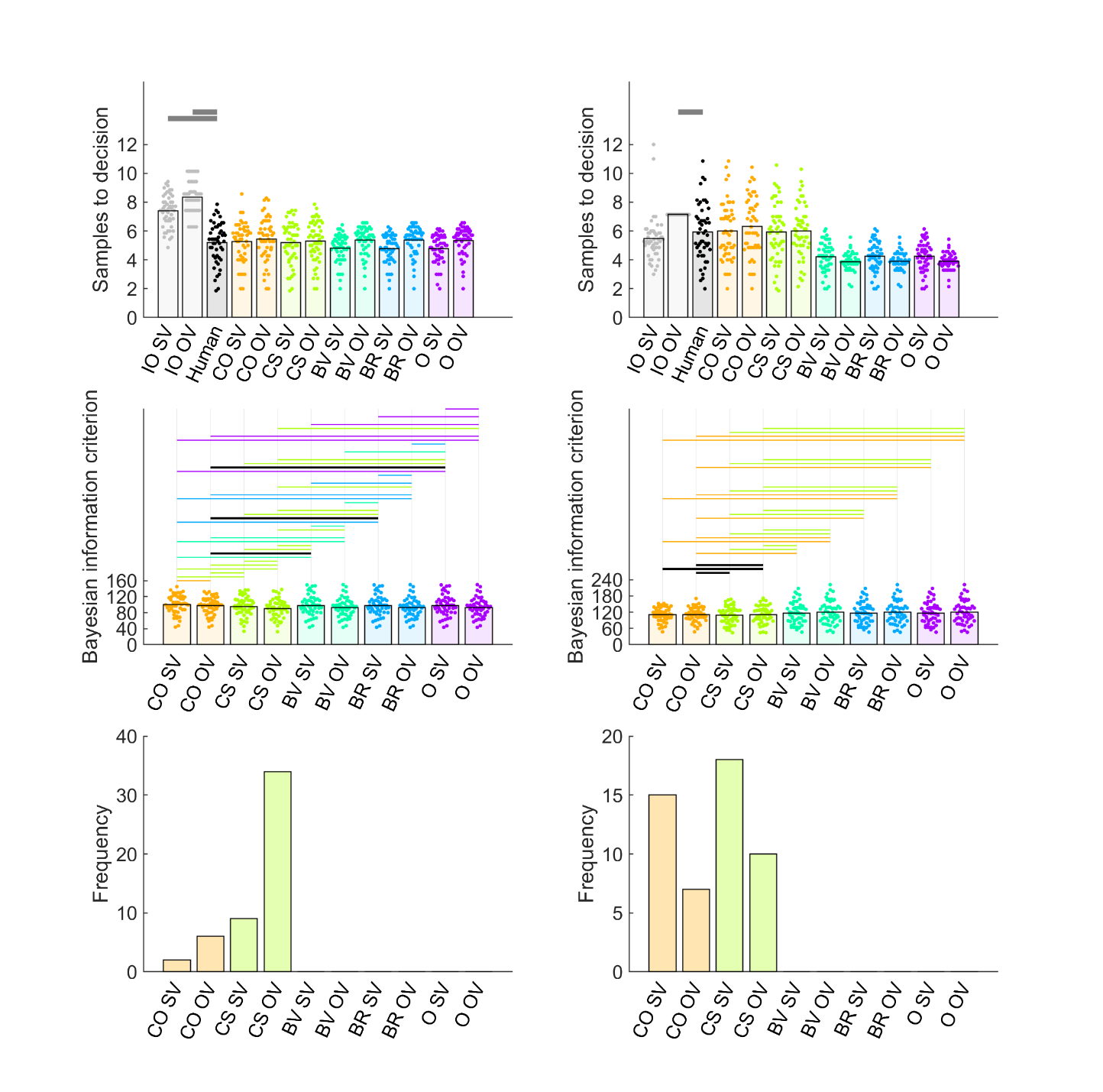
Figure 4. Model comparison for (columns from left to right): Study 1 baseline, squares, timing and payoff conditions. In first and second rows, points show participants and bars show mean values. In the first row, above human and IO samples data, horizontal lines indicate in thin black when *BF01* > 3 (moderate evidence for equal means) or in thick grey when *BF10* > 3 (moderate evidence for different means). Human participant data are reproduced from Figure 2. The second row shows BIC values (lower values indicate better model fit) for participants (points) and mean values (bars). Horizontal lines are shown in black when *BF01* > 3 or in the colour corresponding to the better model when *BF10* > 3. The abundant light green lines suggest that CS outperforms other models. The third row shows that the count of participants for which CS was best-fitting is higher than for other models. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values.



The first row of Figure 5 shows, for the two conditions with an initial rating phase (ratings and full), Bayesian test results comparing participant sampling with model sampling (IO SV and IO OV). Sampling bias in the rating condition (left column) showed undersampling for both IO OV (Cohen’s *d* = -1.72) and IO SV (Cohen’s *d* = -1.20). However, the participant versus ideal observer comparison for the full condition more closely resembled the results of Pilot full (right column), in which the ideal observer did not sample as much as in the other conditions. Even though IO OV sampled about one option less on average in the full condition than in the ratings condition, participants still statistically undersampled in full compared to IO OV (Cohen’s *d* = -0.61). In contrast, the difference in sampling between participants and IO SV was small enough to be statistically inconclusive (Cohen’s *d* = 0.19). These findings taken together lend further confirmation to the second hypothesis: That IO sampling was greater in all the conditions that rewarded only the top three highest ranked choices, compared to the only model that used a payoff scheme that rewarded all choices commensurate with the chosen option value (the full model), whereas no other task feature manipulation affected IO sampling rates.

Note that the results of Pilot full and Study 1 full are inconsistent enough that collectively they cannot clearly indicate the direction of bias (if any). The number of samples for IO models is much more similar to that of human participants in these conditions, and the mean IO sampling rate appears to vary in the vicinity of those of the participants from study to study. We will resolve this issue as well as measure bias more precisely in Study 2 by implementing a full condition with some improved design elements and a statistically better-powered sample size.

Figure 5. Model comparison for Study 1 rating (left column) and full (right column) conditions. In the first and second rows, points show participants and bars show mean values. In the top row, horizontal lines above human and IO samples data indicate in black when *BF01* > 3 (moderate evidence for equal means) or grey when *BF10* > 3 (moderate evidence for different means). Human participant data are reproduced from Figure 2. The second row shows BIC values (lower values indicate better model fit) for participants (points) and their mean values (bars). Horizontal lines are shown in black when *BF01* > 3 or in the colour corresponding to the better model when *BF10* > 3. The abundant light green lines suggest that CS outperforms other models. The third row shows that the count of participants for which CS was the best-fitting was better than for other models. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.



To address our third and final hypothesis, we evaluated computational theoretical models that could explain biases in individual participants. All the conditions produced similar results. CS models closely approximated participants’ mean number of samples to decision (Figures 4 and 5, first row and Figures S9-S12, S14, S15) and and their mean rank of chosen price (Here, CO model performance was especially poor , see Figures S8 and S13, first row). CS OV and CS SV models resulted in better BIC values (Figure 5, second and third rows) than other models on average (BF10 > 3) and best-fit more participants than any other models. The results in Study 1 full were similar, though the analysis of the number of participants who were best-fit by each model (Figure 4, third row, right panel) showed somewhat more contribution of the CO heuristic than for the other Study 1 conditions.

Study 2 Introduction

The Pilot full study and the Study 1 full condition showed that an optimal stopping task in which all choices are rewarded according to their value leads to reduced IO sampling, compared to a variety of different conditions in which only the top three ranking choices were rewarded. Consequently, the mean number of samples of the IO SV and IO OV models were more similar to participants’ sampling rates in Pilot full and Study 1 full (Figures 3 and 5), making a direct assessment of bias in this condition difficult to determine with high precision. Our second study aimed to obtain a higher quality estimate of participant sampling bias in the full condition by overcoming a number of limitations of previous full condition designs. We increased the target sample size from approximately 50 (in Pilot full and Study 1 full) to 151 in Study 2 full. Additionally, we generated a new set of sequence option values for every participant, whereas in Pilot full and Study 1 full, all participants engaged with sequences that were fixed in advance.

Study 2 Methods

Participants

One hundred fifty one participants based in the UK enrolled, using the participant recruitment platform Prolific.

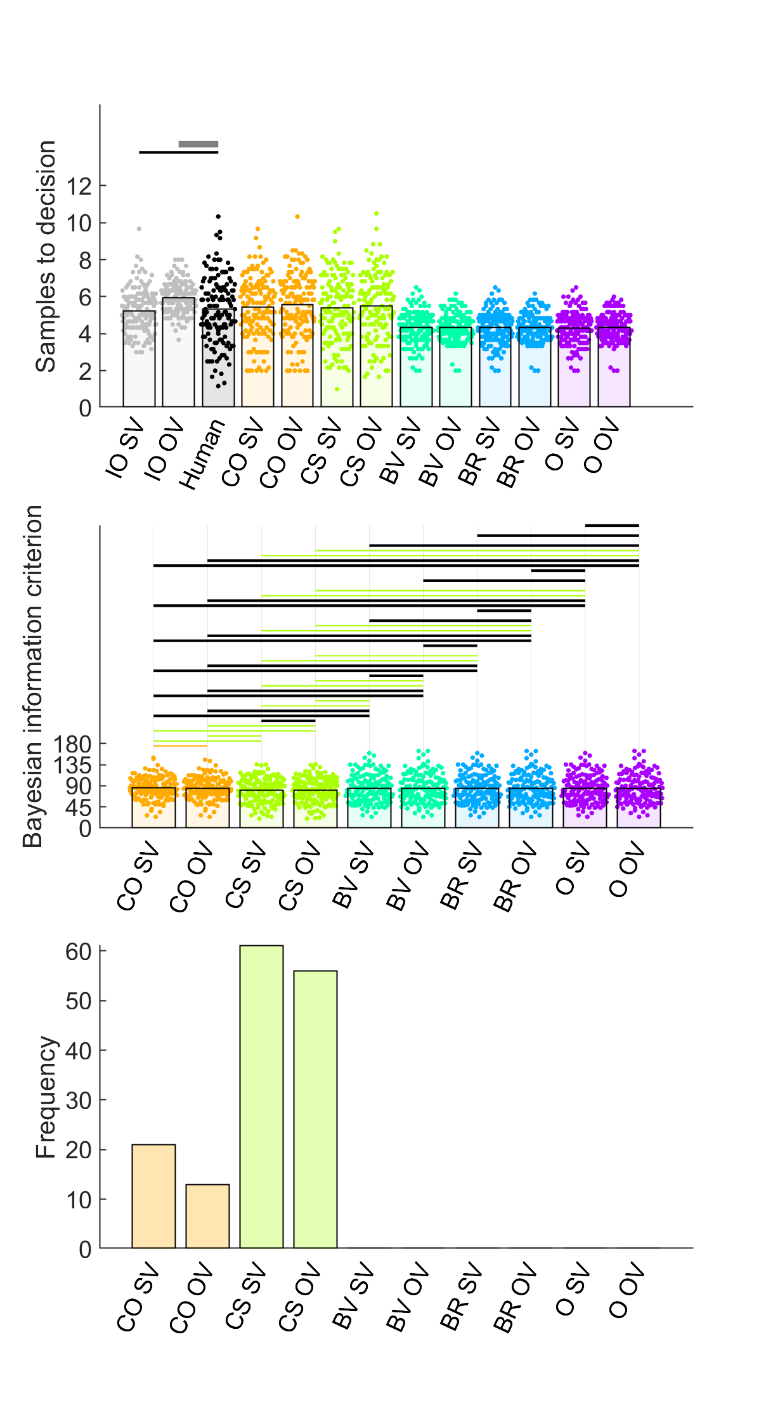
Procedures.

The study was developed in Javascript jsPsych 7.3.1 (de Leeuw et al., 2023), which was used to reproduce most of the methods of Pilot full and Study 1 full. In Phase 1, two lists of the 90 prices (the same smartphone prices used in our three studies reported above) were concatenated and then its elements randomised and presented to participants sequentially above a 1 to 100 scale, in which they indicated the “attractiveness” of each price via mouse click. The mean (over participants) Pearson’s correlation coefficient between the two ratings for each price was .85. Next, participants performed an optimal stopping task with six sequences of 12 price option values, randomly sampled without replacement from the 90 phone contract prices. The study implemented self-paced screen timing. There were no grey squares. Instead, upon choice, the paradigm proceeded directly to the feedback screen. The feedback screen appeared as described above for Pilot full and Study 1 full. Participants were instructed to choose the best possible price.

Study 2 Results and Discussion

Participants appeared to sample about as many prices in Study 2 as in the previous studies reported herein (Figures 1 and 6). From Figure 6, which shows Bayesian pairwise test results comparing participants’ sampling to that of the two ideal observers (threshold *BF01* > 3, moderate evidence for null model), it can be seen that participants sample statistically equivalently to IO OV (Cohen’s *d* = .05) and undersample compared to IO SV (Cohen’s *d* = -0.32). These are somewhat different results from those observed in Pilot full and Study 1 full, which used a more limited set of sequences and a third of the sample size. It is also noteworthy that Study 2 found no evidence for *over*sampling, either when comparing to IO SV or IO OV.

Figure 6. Model comparison for Study 2. In the first and second rows, points show participants and bars show mean values. In the first row, human and IO samples are demarcated by thin black horizontal lines when *BF01* > 3 (moderate evidence for equal means) or thick grey lines when *BF10* > 3 (moderate evidence for different means). Human participant data are reproduced from Figure 2. The second row shows BIC values (lower values indicate better model fit) for participants (points) and their mean values (bars). Black horizontal lines indicate when *BF01* > 3. When *BF10* > 3, the horizontal line is coloured the same as the bar of the better model. The abundant light green lines suggest that CS outperforms other models. The third row shows that CS was the best-fitting model for the most participants. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.



Our model fitting results also provide a theoretical explanation for whatever variability in participant bias that exists for the economic / price domain. Study 2 confirms the results we found throughout our studies reported herein, with the best evidence favouring CS as the best model for the average participant. The SV and OV CS models both: 1) reasonably reproduced participants’ mean number of samples to decision (Figure 6, first row) and ranks (Figure S6); 2) simulated sampling behaviour that was more highly correlated with individual participants’ number of samples than any other model (Figure S17); 3) showed statistically better BIC values than every other model (Figure 6, second row); and 4) best-fit more individual participants than another model.

Study 3 Introduction

Figure 2 suggests that participants are loath to change how much they sample. They are not sensitive to the presence or absence of the various methods features listed in Table 1. And, even though rewarding only the top-three options leads the IO model to increase the number of options it samples, participants do not correspondingly increase how much they sample under this incentivisation scheme. Our theoretical model fitting results so far suggest that participants’ sampling is controlled by a perceived intrinsic cost to sample, which would indeed limit participants from increasing how much they sample, relative to an ideal observer, which operates only under the ground truth of no extrinsic sampling cost and so is freer to increase its sample rate when appropriate.

The goal of Study 3 was to ensure that our implementation of the optimal stopping task was methodologically viable and that it is in practice possible to experimentally modulate how much participants sample at least to some degree. Costa & Averbeck (2015) manipulated the sequence length (i.e., how many options were available in each sequence) and found participants were willing to increase the number of samples for longer sequences. Nevertheless, Costa & Averbeck found that undersampling was more pronounced at higher sequence length. Participants in their study appeared reluctant to increase how much they sampled, whereas the ideal observer increased its sampling rate to adapt to the longer sequence lengths without constraint – a pattern that appears consistent with the reluctance with which participants increase their sampling rates in our studies reported herein. Here in our third study, we replicated this effect of sequence length on participants’ average number of samples, using sequence lengths of 10 and 14 options. Moreover, we predicted that an intrinsic cost to sample will continue to best-explain participants’ sampling bias for both 10 option and 14 option sequences.

Study 3 Methods

The preregistration of Study 3 can be found at <https://osf.io/vcf7u>. We enrolled 140 participants from the UK using Prolific, where half the participants engaged with sequences of length 10 and the other half engaged with sequences of length 14. As explained in the pre-registration, the sample size was intended to double that of Costa & Averbeck (who used a more powerful repeated-measures design and who were able to use more trials per participant in-lab, while we needed a shorter online study). The procedures were identical to Study 2, using the same jsPsych code, merely changing the sequence length of the optimal stopping phase of the study. The averages (over participants) of the Pearson’s *r* values computed between the two phase 1 ratings to each price were .88 for the 10 option condition and .84 for the 14 option condition.

Study 3 Results and Discussion

Figure 2 (rightmost bars) shows that our hypothesis was confirmed: participants sampled significantly more for longer sequences, replicating the findings in Costa & Averbeck (2015). As in Costa & Averbeck, undersampling was not observed for the shorter sequence length. In our study, the Bayesian tests (shown in Figure 7) suggest that at a sequence lengths of 10 options, participants slightly *overs*ampled (rather than undersampled) compared to IO OV (Cohen’s *d* = 0.33), while the difference with IO SV remained inconclusive (Cohen’s *d* = 0.26). In contrast, participants showed clearer evidence for an undersampling bias at sequence lengths of 14, as they sampled statistically *less than* both IO OV (Cohen’s *d* = -.63) and IO SV (Cohen’s *d* = -0.44).

Our model-fitting also confirmed our hypothesis that participants’ sampling biases could be explained by an intrinsic / perceived cost to sample. The model-fitting results for the two sequence length conditions of Study 3 closely resembled those of Study 2 and of each other. Although CO and CS models both reasonably approximated how much participants sampled (Figure 7, first row), CO poorly reproduced participants’ ranks (Figure S18, first row) and the two CS models outperformed all other models when predicting individual participant sampling (Figures S19 and S20). The CS models exhibit better BIC scores (Figure 7, second row) and better fit more individual participants (Figure 7, third row) than all other models. There was little differentiation between the results of CS SV and those of CS OV.

In summary, participants can and will change their sampling behaviour to some degree in some contexts. However, at least on tasks using the economic domain that we studied here, participants’ number of samples are “held in place” by their perception of an intrinsic cost of sampling, which discourages them from increasing their sampling to an optimal degree, which leads to increasing undersampling bias as sequences lengthen.

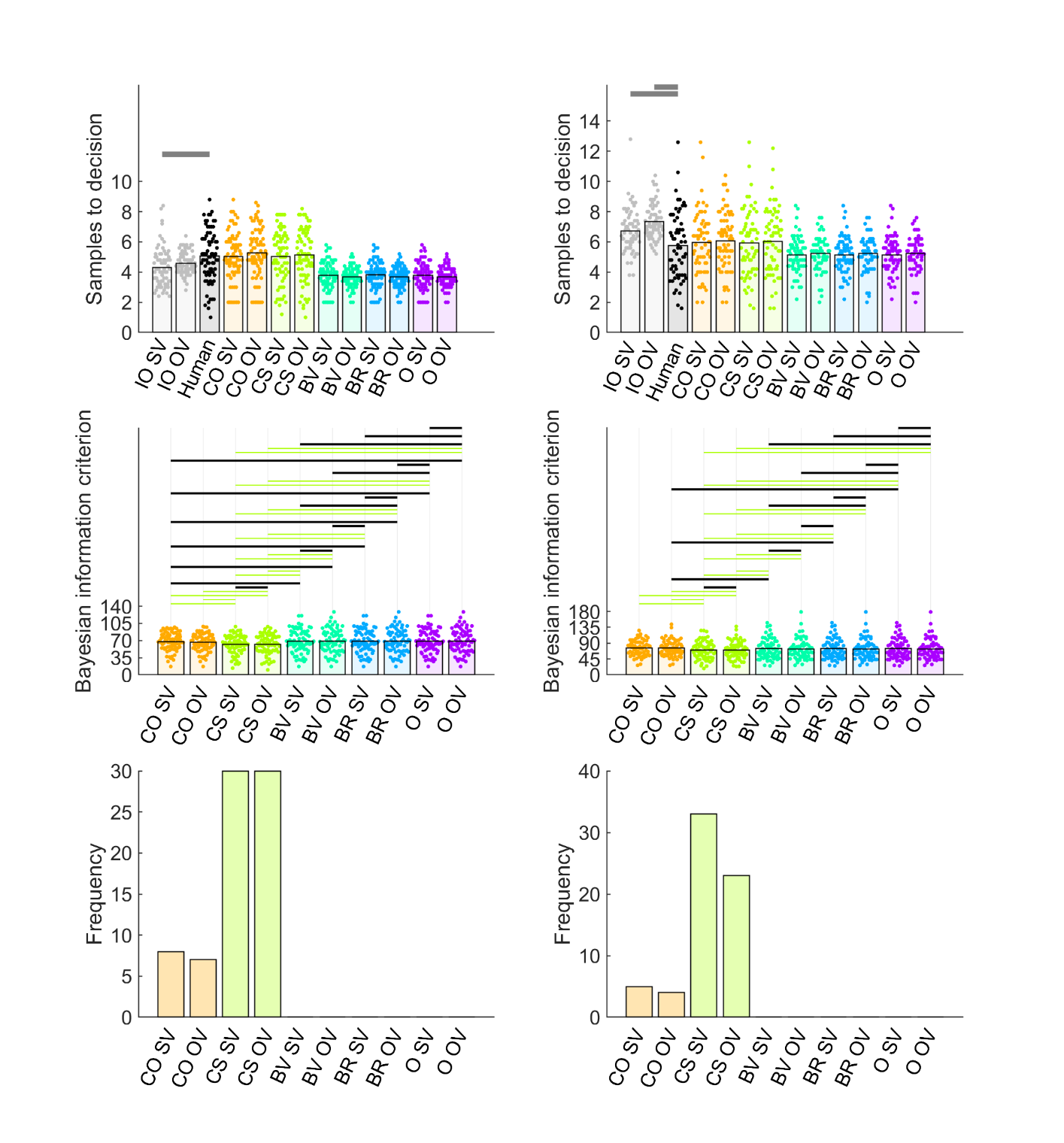


Figure 7. Model comparison for Study 3 10 options (left column) and 14 options (right column) conditions. Points in the first and second rows show participants and bars show mean values. In the first row, human and IO samples are demarcated by black horizontal lines when *BF01* > 3 (moderate evidence for equal means) or grey lines when *BF10* > 3 (moderate evidence for different means). Human participant data are reproduced from Figure 2. The second row shows BIC values (lower values indicate better model fit) for participants (points) and their mean values (bars). Black horizontal lines indicate when *BF01* > 3. When *BF10* > 3, the horizontal line is coloured the same as the bar of the better model. The abundant light green lines suggest that CS outperforms other models. The third row shows that the count of participants for which CS was the best-fitting was higher than for other models. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

General Discussion

In our pilot studies, we first established that we could replicate an undersampling bias (Baumann et al., 2020; Cardinale et al., 2021; Costa & Averbeck, 2015) by adapting a previous implementation of an economic full-information problem (Costa & Averbeck, 2015). In addition to this replication, we also tested novel variables (used in other studies like van de Wouw et al., 2022) that hypothetically might modulate undersampling bias. We were able to modulate the size of the undersampling bias in two ways: by manipulating the payoff scheme and by manipulating sequence length.

The undersampling bias was nearly eliminated in the “full” condition, a finding we were able to replicate (Pilot full, Study 1 full, Study 2, Study 3). Yet this is not because participants changed their behaviour much in this full condition, compared to a variety of other conditions in Study 1, which manipulated the presence or absence of various task features from the full condition (van de Wouw et al., 2022). Instead, the IO model sampled more than participants did in every condition except for the full condition, where it reduced its sampling rate to become more like participants’ (lower) sampling rate. This full condition was the only condition where participants were instructed to simply maximise the option value of their choices, as opposed to obtain one of the top-three ranked options. Indeed, we were able to experimentally eliminate as alternative possible causes the other task features in the full condition, including screen timing, grey squares, extrinsic monetary reward, the presence of a first rating phase and the use of subjective values in the IO model. Thus, we must conclude that only the reward payoff scheme used in the full condition could have reduced the IO model’s sampling rate and modulated undersampling bias. Indeed, because the IO model is willing to increase its sampling rate to the one appropriate for its payoff scheme, and participants are not so willing, the size of the undersampling bias is correspondingly modulated by the payoff scheme.

We observed a similar phenomenon in Study 3 for sequence length. Although both participants and the IO model increased their sampling rates for longer sequences, the IO model showed a greater sampling increase for longer sequences than participants did, and thus the undersampling bias correspondingly increased – a finding replicated from Costa and Averbeck (2015). It appears that, while sometimes participants can, albeit reluctantly, increase their sampling, they generally prefer to limit how much they sample, even when it is optimal to increase sampling rate more than they do.

Crucially, we were able to theoretically explain, in terms of a computational mechanism, participants’ sampling bias. Our model fits suggest that participants’ reluctance to increase sampling rates when it is optimal to do so arises because participants subjectively regard increased sampling as intrinsically costly. Our model comparison also eliminated several other competing theoretical accounts for participants’ biased sampling.

Whence does this does this suboptimal cost to sample arise? Although this is still an open question for future research, the data we report allows us to reject some possibilities. Our studies imposed no extrinsic penalty, monetary or otherwise, for extra sampling. Attempting to save time by stopping searches earlier is unlikely to have been a motivator either. Some conditions tested herein used a task feature designed to prevent participants from shortening the study by sampling less: the remaining options after choice in each sequence were replaced with grey squares, which participants were required to page through to finish each sequence, even after choosing an option. Moreover, some conditions also manipulated the screen timing: fixed screen timings add several seconds extra time cost per sample, compared to a condition with fixed screen timing, where a participant can quickly sample again at the tap of a key without waiting. Neither the grey squares nor the screen timing manipulations affected participants’ sampling rates (nor that of the IO model), null findings which are bolstered here by evidence from Bayesian null effects tests. Working memory is not directly a cause either. Participants had all rejected options available on the screen in front of them and so were prevented from forgetting rejected options.

Nevertheless, there are still open explanations, about which we may speculate. Although participants cannot “forget” rejected option values, they may nevertheless suffer limited bandwidth for integrating new evidence from samples. That is, participants might find that performing predictive computations based on more than four or five options to be effortful, even if the values themselves are not forgotten. Moreover, inspection of the ranks that participants achieved with their choices (a measure of their choice accuracy), shown in the first rows of Figures S4, S8, S13, S16 and S18 suggests that participants’ choices closely approximated performance of IO, even though participants exerted less effort to sample. Others (Todd & Miller, 1999) have argued that there is adaptive value for participants to reduce sampling to minimal levels if they can maintain “satisficing” levels of choice outcome performance, with less exertion or intrinsic sample cost – a form of heuristic decision making. Such might be the case here.

Future research may wish to ascertain how and why participants adopt certain intrinsic cost to sample values by fitting and comparing theoretical computational models in paradigms in which participants oversample, rather than undersample. Although we were unable to reliably induce participants to oversample in the present work (and instead identified variables that modulate the size of undersampling bias), others like van de Wouw et al. (2022) have demonstrated and replicated oversampling bias. Their work, rather than presenting options as numeric prices as we did here, communicated option values using images, such as the attractiveness of faces, foods and holiday destinations. Our manipulations of task features in Study 1 have already tested and rejected other task differences used in their paradigm that might give rise to oversampling (e.g., grey squares, timing, etc), leaving the pictorial stimulus domains as the most likely instigator of oversampling in van de Wouw et al. (2022). Thus, we hypothesise that a CS model might explain sampling bias in paradigms using images to present option values if participants find sampling through numbers to be intrinsically costly, but searching through images intrinsically rewarding, like the supposedly “addictive” activity of sequentially accepting or rejecting images on Tinder, a popular mobile application for dating. If it is participants’ attitudes towards their experiences of the options themselves that controls their sampling bias (and thereby the reward value of their choices), then this would have profound implications for how people engage with sequential searches in real-world contexts. For example, sellers in some domains may choose how they present shopping options to consumers to encourage fast yet satisficing searches.

Our model comparison is the first time theoretical models that specify the computations humans use to solve full information problems have been compared so comprehensively. Costa and Averbeck (2015) introduced the parameterised cost to sample model that we consider here and fitted that model to participants’ sampling choice on an economic full information task. However that study did not perform a model comparison with alternative models. Moreover, our current study for the first time provides a comprehensive parameter recovery analysis for this model and a number of other parameterised versions of this model (Supplementary Text A and Figures S1-S3). Our work also builds on the approach recently taken by Baumann et al. (2020), who compared the CO OV model we consider here with “threshold models” (Lee, 2006). Although these threshold models are useful tools for directly estimating participants’ choice thresholds at each sequence position from participants’ behaviour data, we took a different approach for our model comparison. Our approach was to compare models that are “computational” in the sense that they specify the computations that participants might theoretically be using to accurately solve the task, including specification of how participants compute their decision thresholds. In the parameterised Costa & Averbeck (2015) models we considered, the action value for sampling again (See Methods) acts as the effective decision threshold, which varies over trials depending on the perceived prospect of sampling a better option value, and which the value of the current option needs to exceed before the model will commit to a choice. Using these models, there is no need to explicitly parameterise the threshold, as it arises naturally from the computations within the model. Moreover, we obtain the added capability of parameterising bias terms (e.g., the cost to sample) and then simulating how these bias terms influence the computation of thresholds, which cannot be done using threshold models, at least as they have been implemented in the past. Nevertheless, our results largely agree with a key finding from the model comparison in Baumann et al., who showed that models that change their decision threshold across samples better fit participants’ data than the CO OV model, in which the decision threshold is established after the cut off sequence position and henceforth remains fixed. Similarly, our model comparison agree that variable-threshold models (i.e., the CS OV and CS SV models) better fit participant choices compared to the relatively rigid thresholds of the CO OV heuristic.

In summary, we show that, participants’ sampling behaviour on optimal stopping tasks is relatively insensitive to most methodological manipulations. In contrast, the ideal observer (which reflects optimal performance) is relatively more sensitive than participants to at least the payoff scheme and the sequence length, such that these two factors can modulate the degree of undersampling bias. We explain participants’ sampling behaviour using a theoretical model by which participants implement optimal Bayesian computations to solve the task accurately, but a systematic undersampling bias develops when participants perceive that continued sampling can become intrinsically aversive.

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Supplementary Procedures

Text A: Parameter recovery

To ascertain the ability of our models to derive the correct parameters from individual participant data, we performed parameter recovery analyses, in which we simulated model choices (take option or sample again) in response to randomly-generated option values. We wished to ensure that our fits to human behaviour would provide more reliable fits than those simulated during parameter recovery, and so we simulated 20 participants (our empirical studies recruited at least approximately 50 participants) with only five sequence per participant with twelve options per sequence. To parallel the structure of our empirical paradigms, we created a generating distribution (separately for each simulated participant) of 426 option values, randomly-produced from a Gaussian distribution with mean 50 and standard deviation of 5 and within the range of 1 to 100 (recall that we normalised all our prices to this same range when fitting models to human participants). Then, we populated the sequences of input option values for the optimal stopping task from this participant-specific generating distribution. We configured our models with ranges of the key theoretical parameters (Figure S1, x axis) that produced sampling rates between roughly two and ten samples to decision (Figures S2 and S3). The aforementioned randomly-generated option values were then presented to every configured model to extract simulated sampling rates associated with each configured parameter value. Varying the configured parameters in this way led to systematic variation in the sampling rate, as expected (Fig S3, top panel). We then fitted the models to these simulated take option / sample again decisions in the same way as we fitted human participants to obtain parameter estimates of the configured parameters. Configured and estimated parameters tended to correlate (Figure S1 and lower panel of Figure S3), especially for the model on which we base our final conclusions, CS. Also, the sampling rates simulated using configured parameters highly correlated with sampling rates simulated using the estimated parameters (Figure S2 and middle panel of Figure S3).

Text B: Attention check

Attention checks were added to phase one (i.e., the ratings phase) of Pilot full and the Study 1 full and Study 1 ratings conditions, to compensate for the unsupervised nature of online data collection. Every attention check showed a cross, a ‘next’ button, and the text "press ‘next’ when the cross disappears". The cross disappeared at a random time interval between one and five seconds. The ‘next’ button was active the whole time. If participants were paying attention, they would not press the ‘next’ button as soon as it appeared, but would instead read the text and respond only after the cross had disappeared. Thus, if participants’ response time exceeded the cross display time, they passed the attention check. Nevertheless, we found high correlations between phase 1 and phase 2 ratings (i.e., > .8) across our studies (see average correlations between phase1 ratings reported in Methods to pilot studies and Study 1) and so we elected to not remove participants based on attention check data in Pilot full, Study 1 full and Study 1 ratings and we discontinued the use of attention check trials in Studies 2 and 3.

Figure S1. We compared configured parameter values (horizontal axes) for 20 simulated participants (each shown as an individual scatter point) against parameter values estimated (vertical axes) after fitting models to decisions simulated using configured parameter values (horizontal axes). Correlations can achieve > .9. The grey diagonal indicates when configured and estimated parameters would be exactly equal. The coloured line indicates the regression line relating configured and estimated parameter values. Note that an extreme outlying point was removed from the lowest configured parameter in CO plot and *r*-value to increase visibility (estimated value = -219.85, when including outlier *r* = .28). Importantly, the model we later conclude to best explain human sampling choices, CS, shows highly accurate parameter recovery. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism.

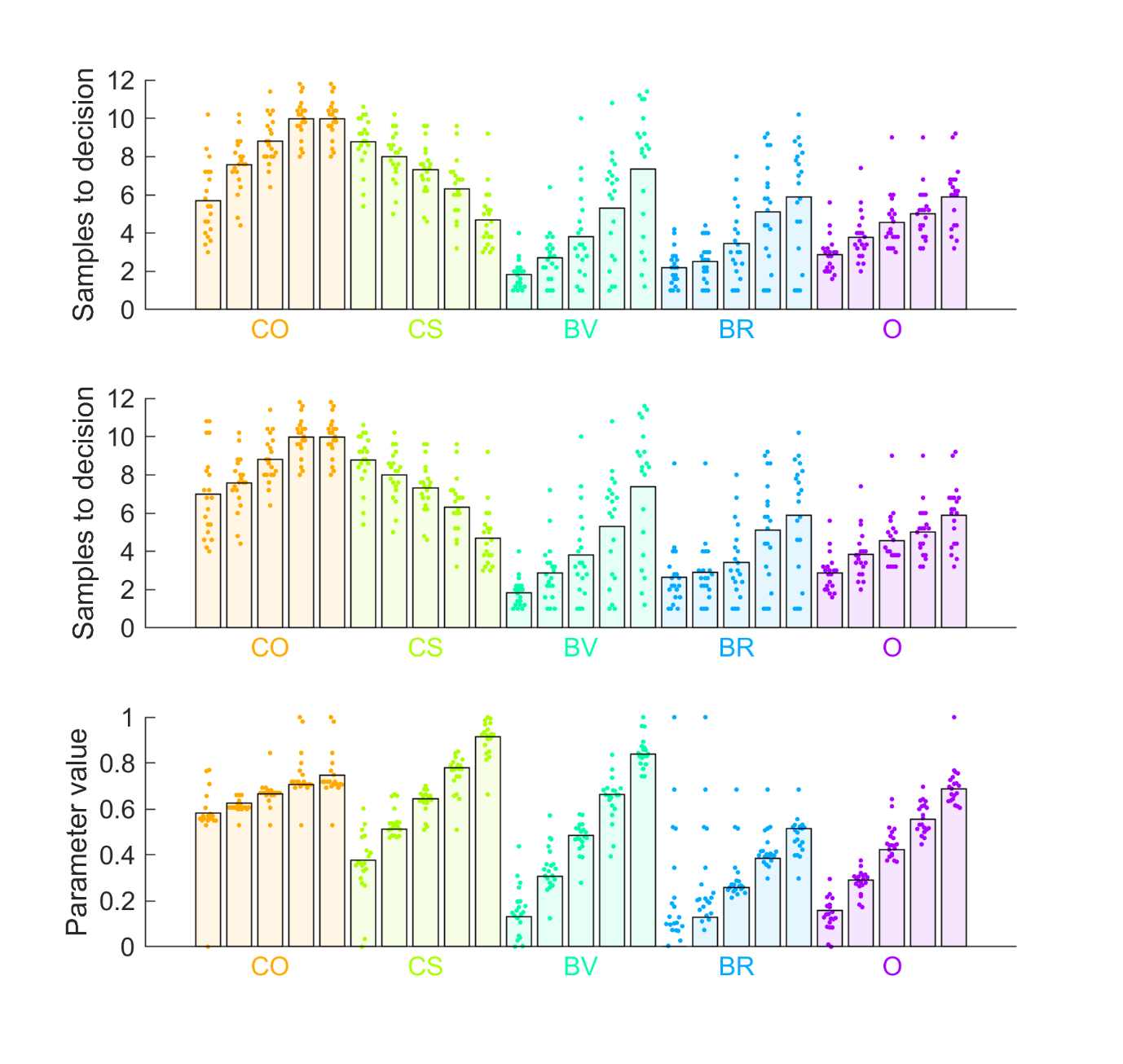
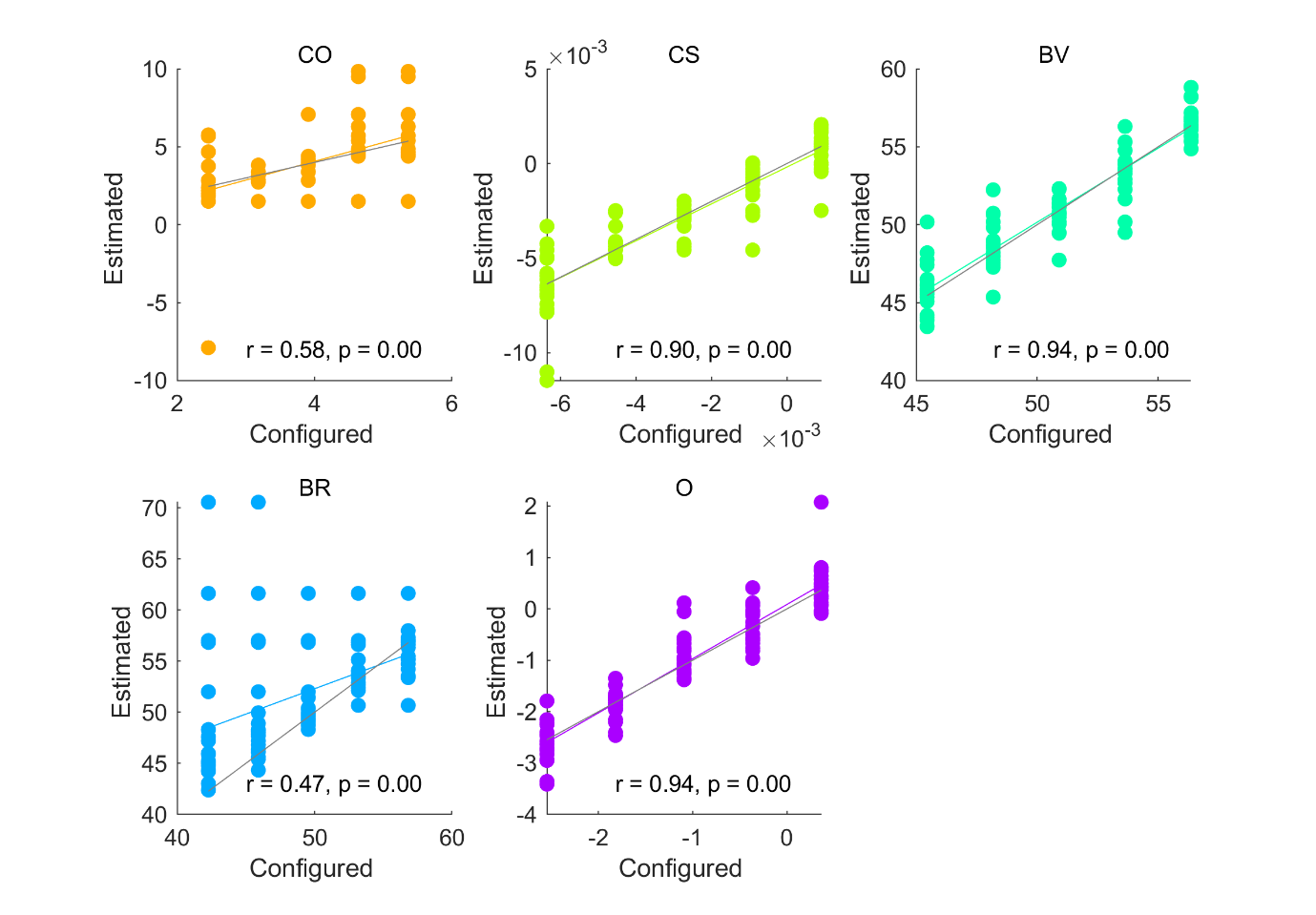


Figure S2. (Top panel) Mean sampling rates (bars) for simulated participants (points), for each configured parameter value for each model. Note that varying configured parameter values leads to systematic increase or decrease in simulated sampling rates. (Middle panel) Models were fitted to the data in the top panel, estimated parameters recovered, and then here we plot mean sampling rates (bars) produced from those recovered parameters in individual participants (points). The sampling rates of the fitted models closely approximate the sampling rates of the original configured models. (Lower panel) The points show how recovered / estimated parameters for individual participants cluster around their corresponding configured parameter values (bars). One extreme outlier was removed from CO model plotting to improve visibility (See caption, Figure S1). Parameter values were normalised to be between 0 and 1 for each model to facilitate plotting of all model parameters on the same scale. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism.

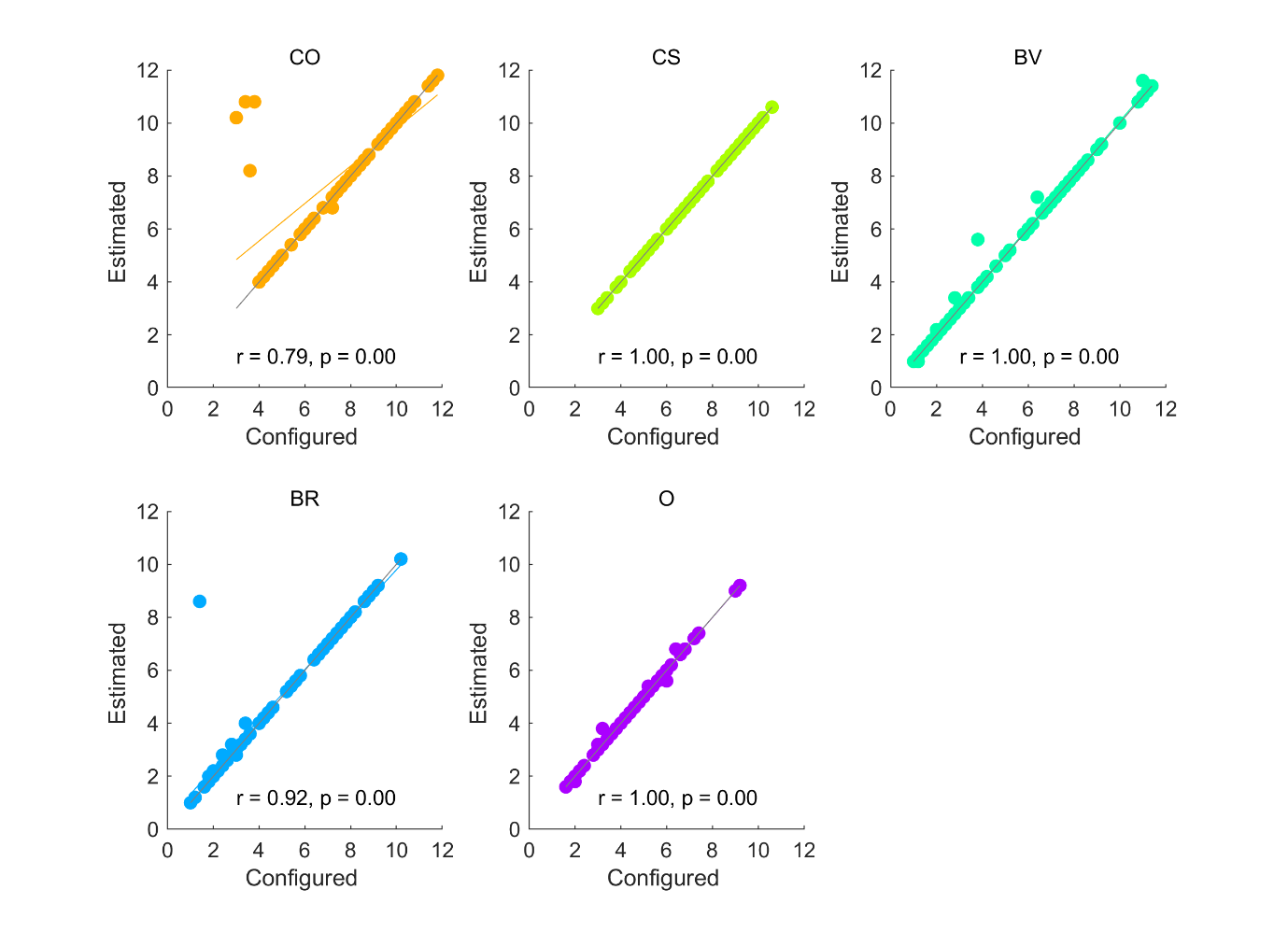


Figure S3. Sampling rates simulated using configured parameters (horizontal axis) are plotted against sampling rates computed from recovered (estimated) parameters. Recovered parameters are highly suitable for reproducing the sampling choices that they are intended to model. The grey diagonal indicates when sampling rates based on configured and estimated parameters would be exactly equal. The coloured line indicates the regression line relating sampling rates based on configured and estimated parameter values. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism.

Figure S4. Model comparison for Pilot baseline (left column) and Pilot full (right column) Top and middle rows show individual participants as points and bars show their mean values. The top row shows the ranks of chosen items. The second row plots the “first” or key theoretical parameter values, estimated for each fitted model. The third row shows the “second”, or inverse temperature parameter beta, estimated for each fitted model. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

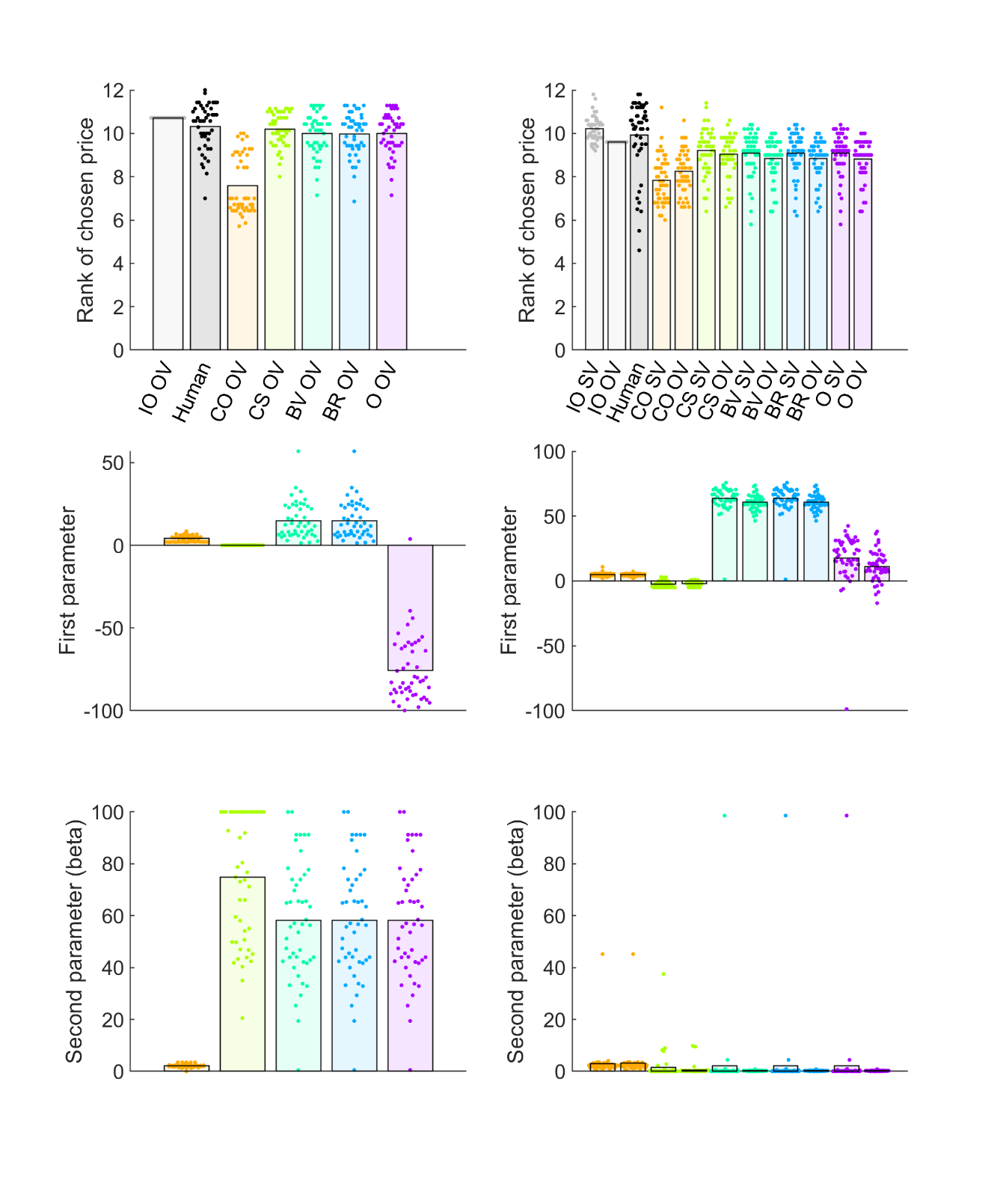


Figure S5. Linear relationships between human participant sampling in Pilot baseline versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured lines represent regression lines, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values.

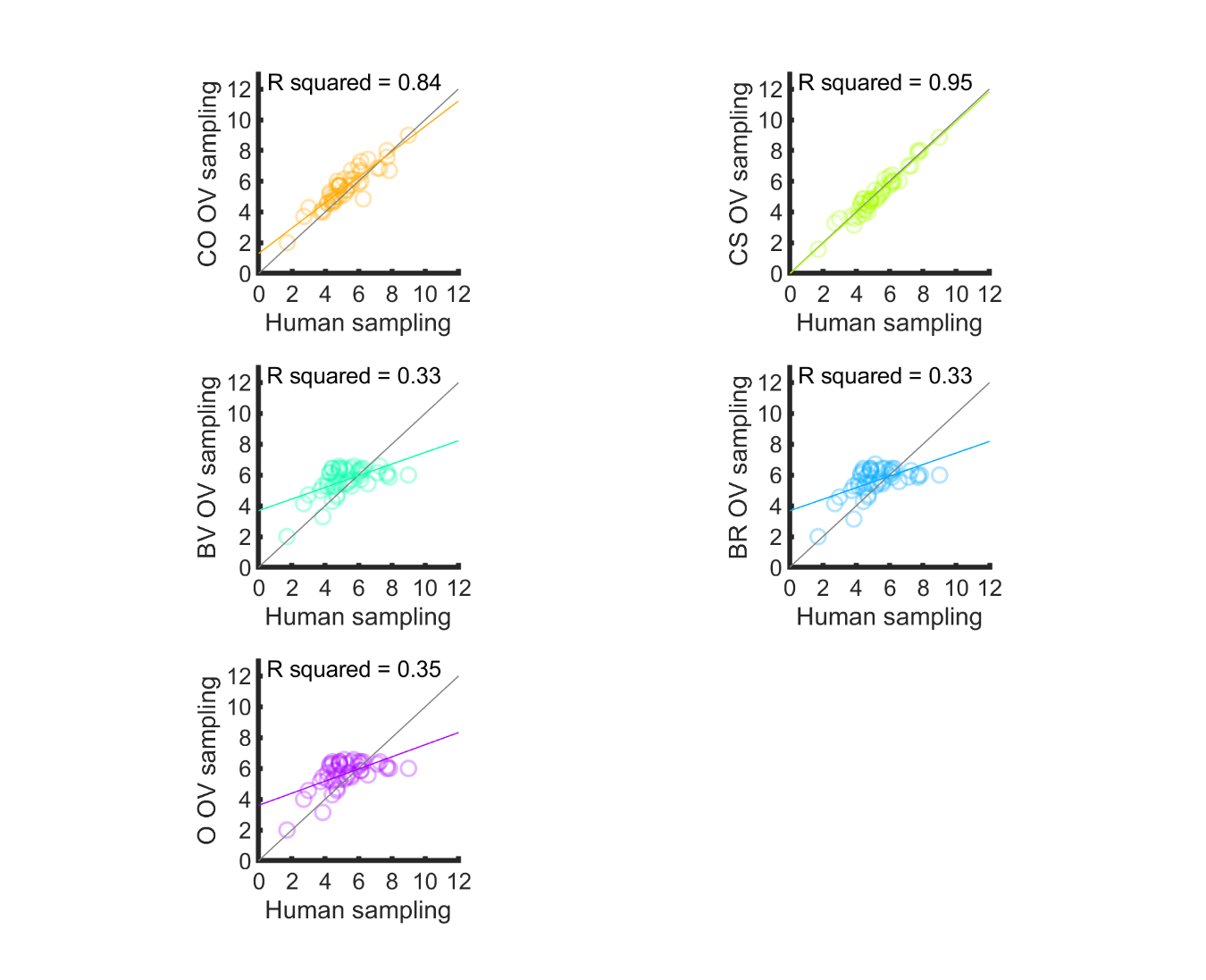


Figure S6. Linear relationships between human participant sampling in Pilot full versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured lines represent regression lines, with corresponding *R2* printed on plot. The CS models predict the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

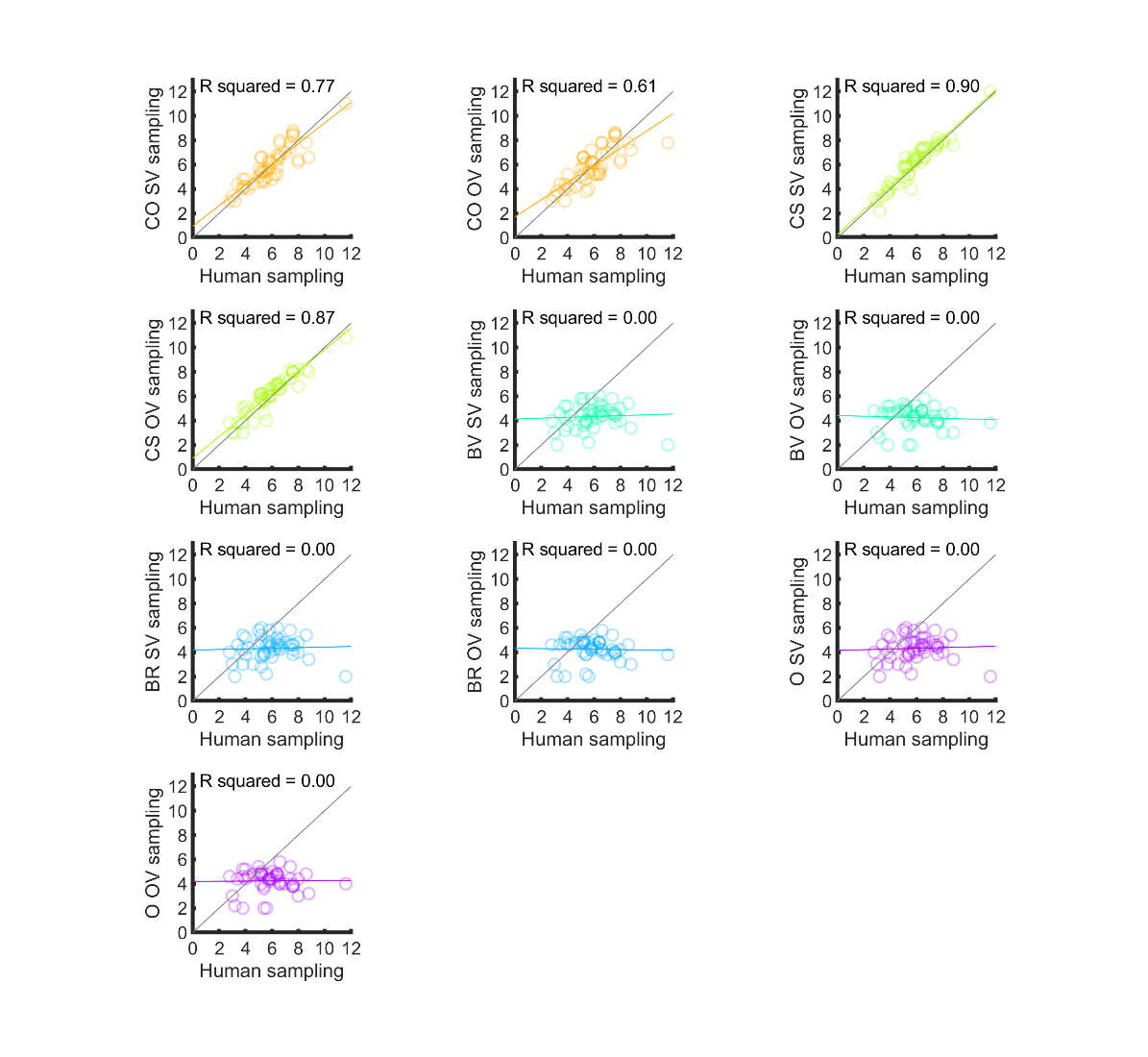


Figure S7. Cohen’s *d* effect sizes for pairwise comparisons of participants’ sampling rates in Study 1.

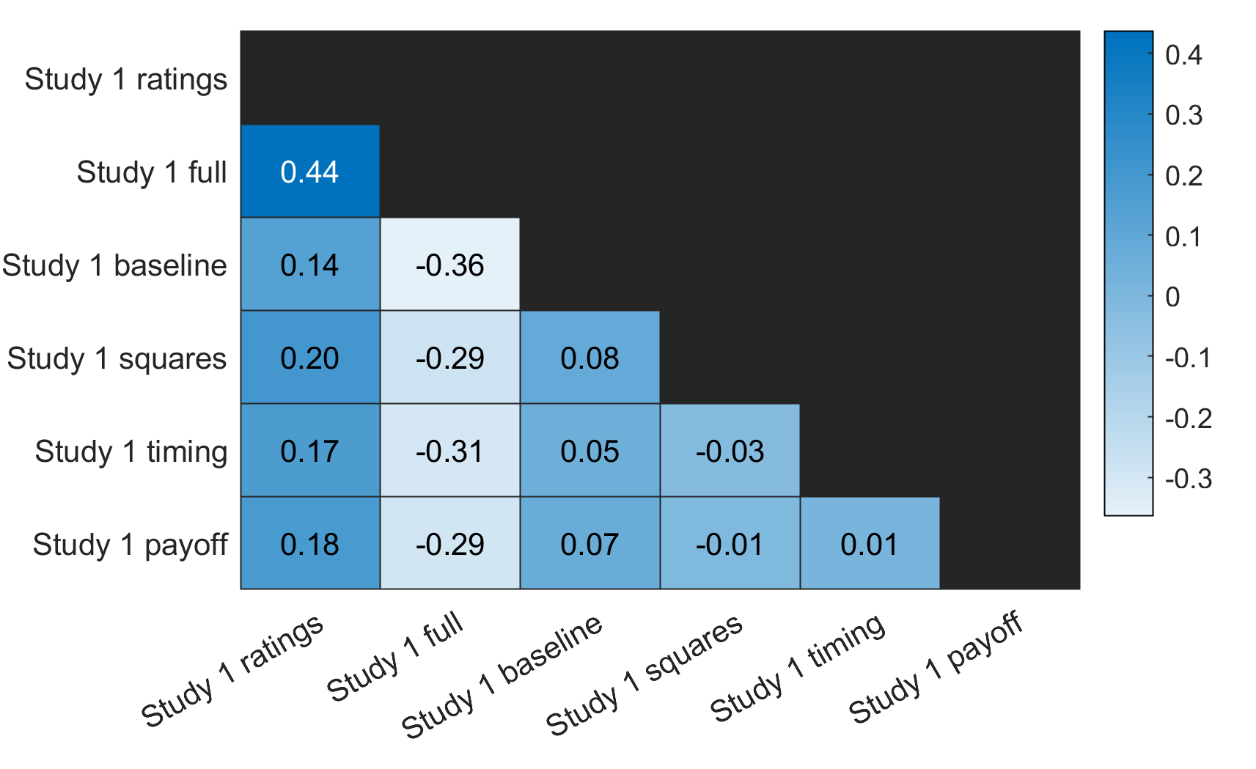


Figure S8. Model comparison for (columns from left to right): Study 1 baseline, squares, timing and payoff conditions. Top and middle rows show individual participants as points and bars show their mean values. The top row shows ranks of chosen prices. The second row plots the “first” or theoretical parameter values, estimated for each fitted model. The third row shows the “second”, or inverse temperature parameter beta, estimated for each fitted model. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

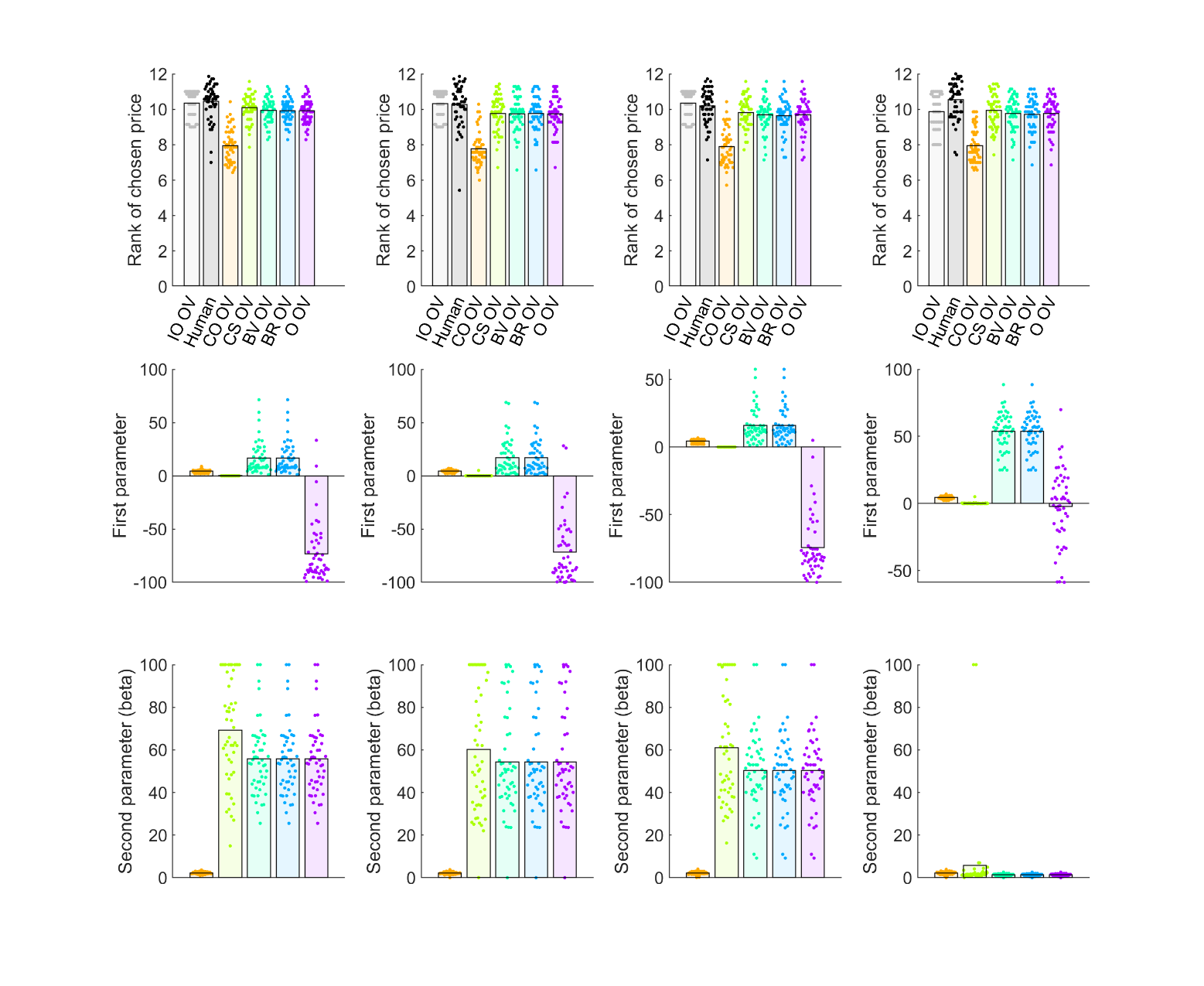


Figure S9. Linear relationships between human participants’ sampling in Study 1 baseline versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

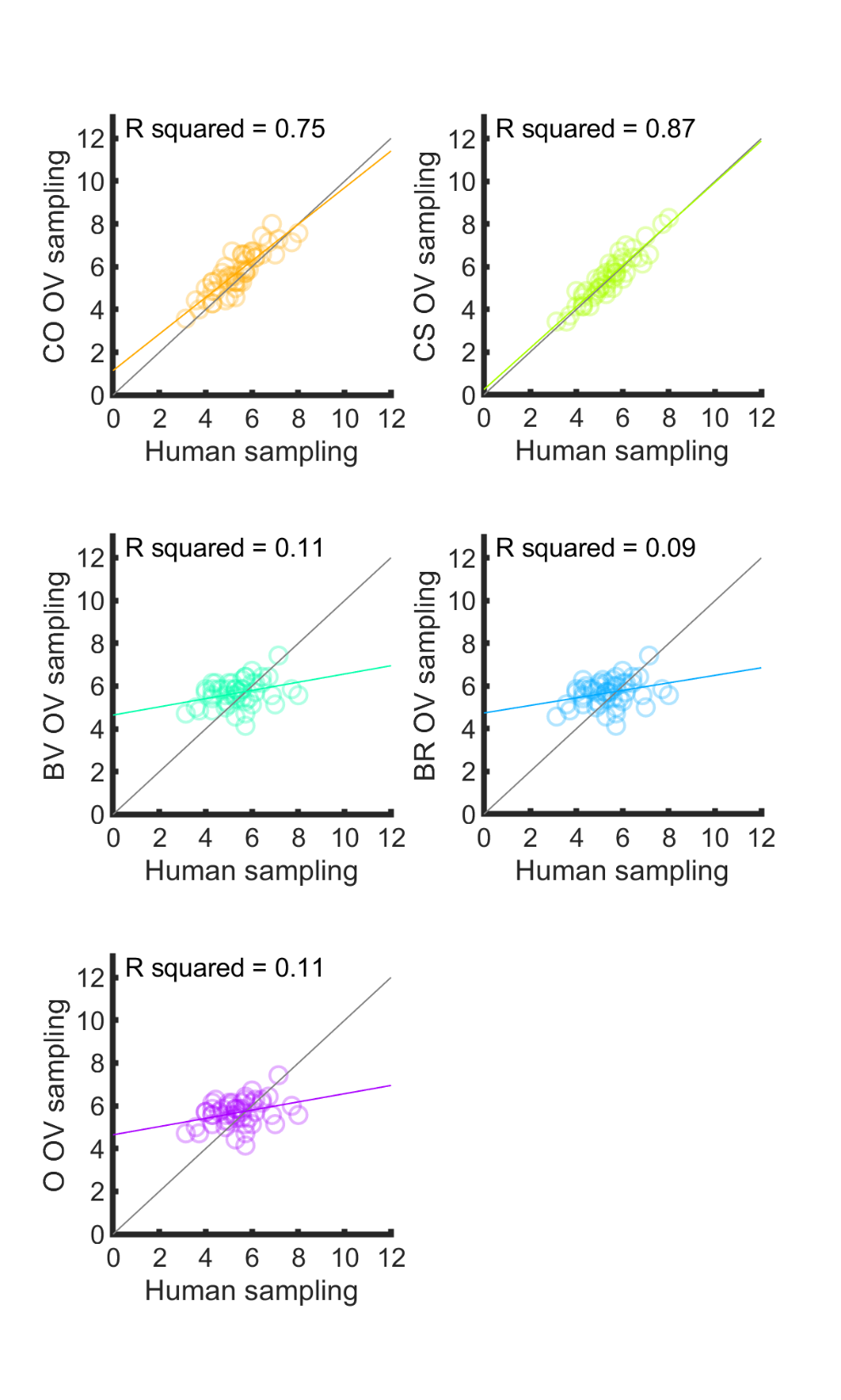


Figure S10. Linear relationships between human participants’ sampling in Study 1 squares condition versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2*printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values.

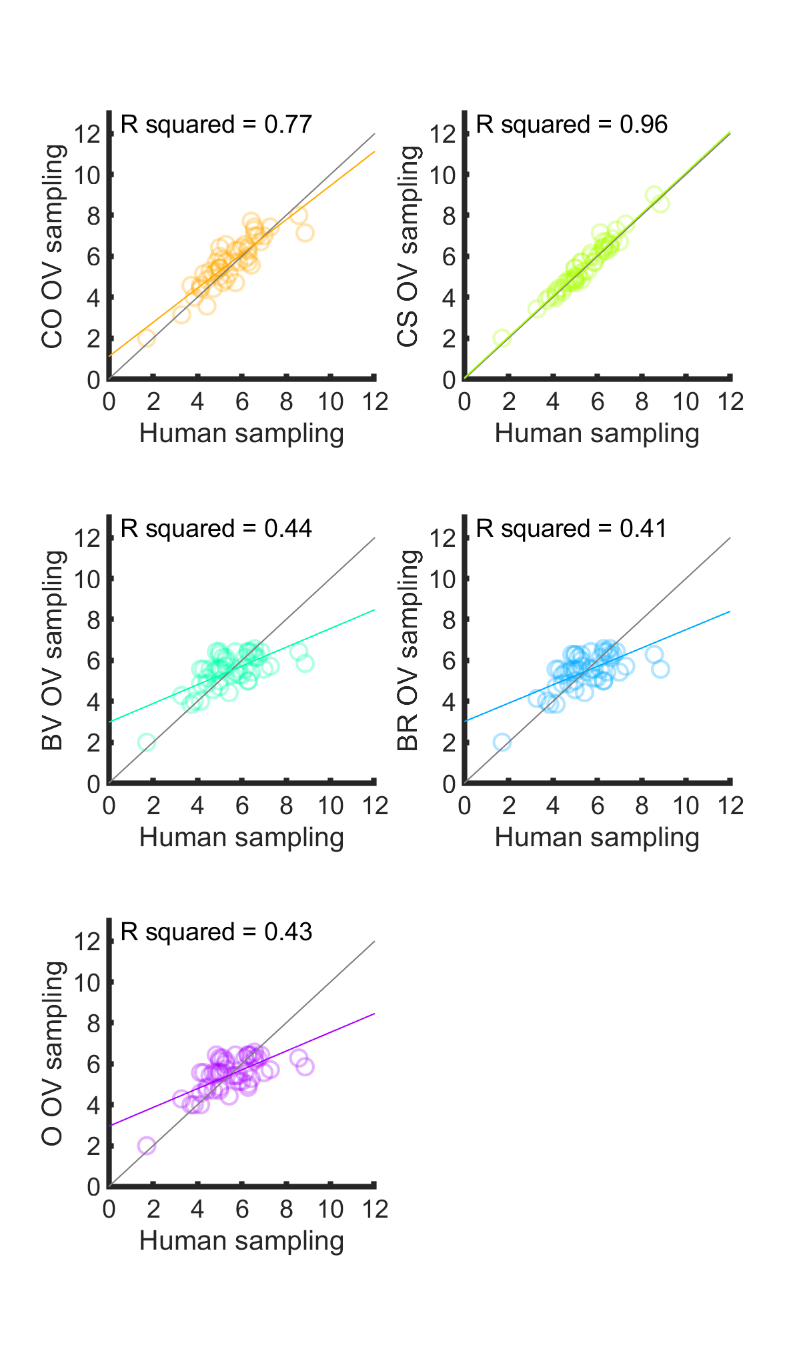


Figure S11. Linear relationships between human participants’ sampling in the Study 1 timing condition versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values.

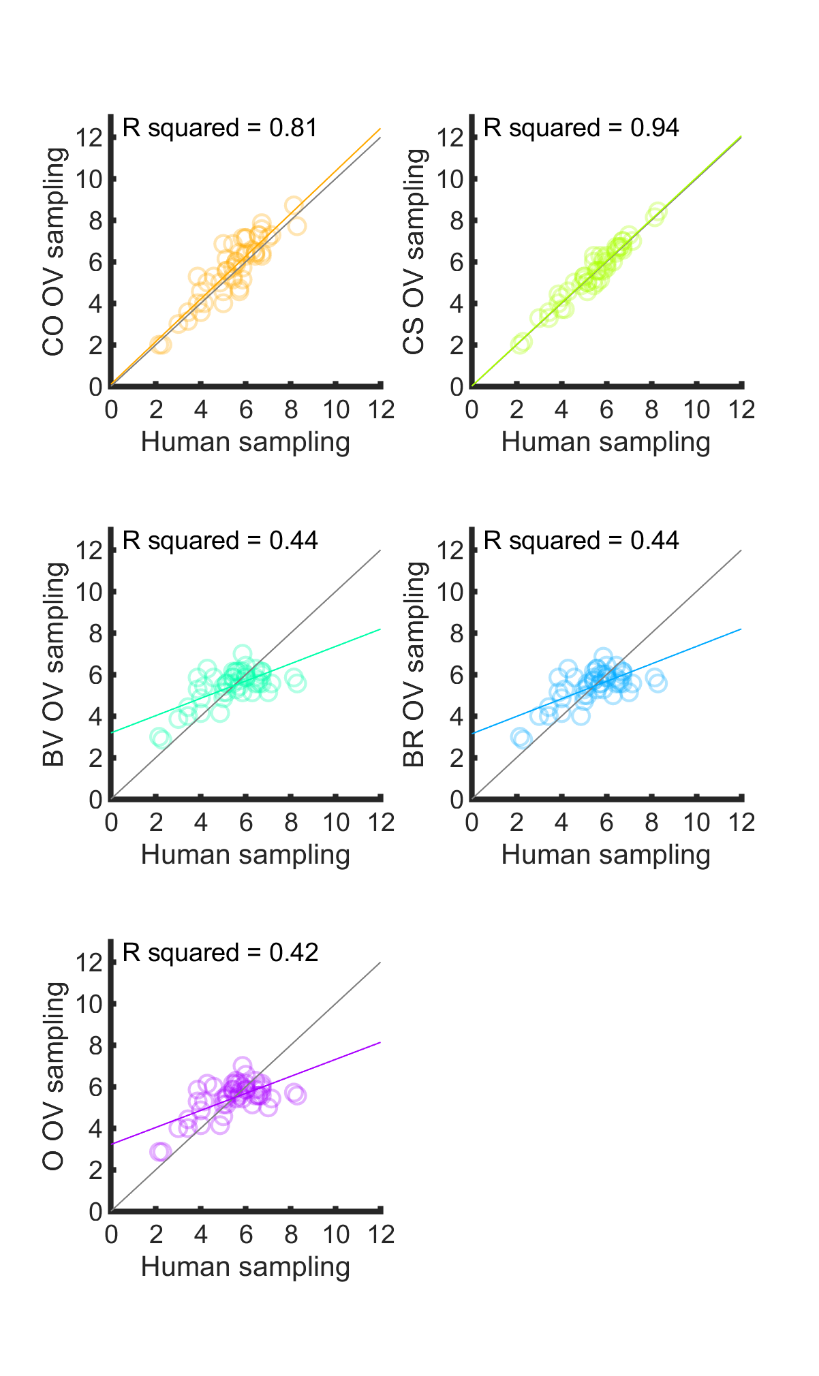


Figure S12. Linear relationships between human participants’ sampling in the Study 1 payoff condition versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2*printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values.

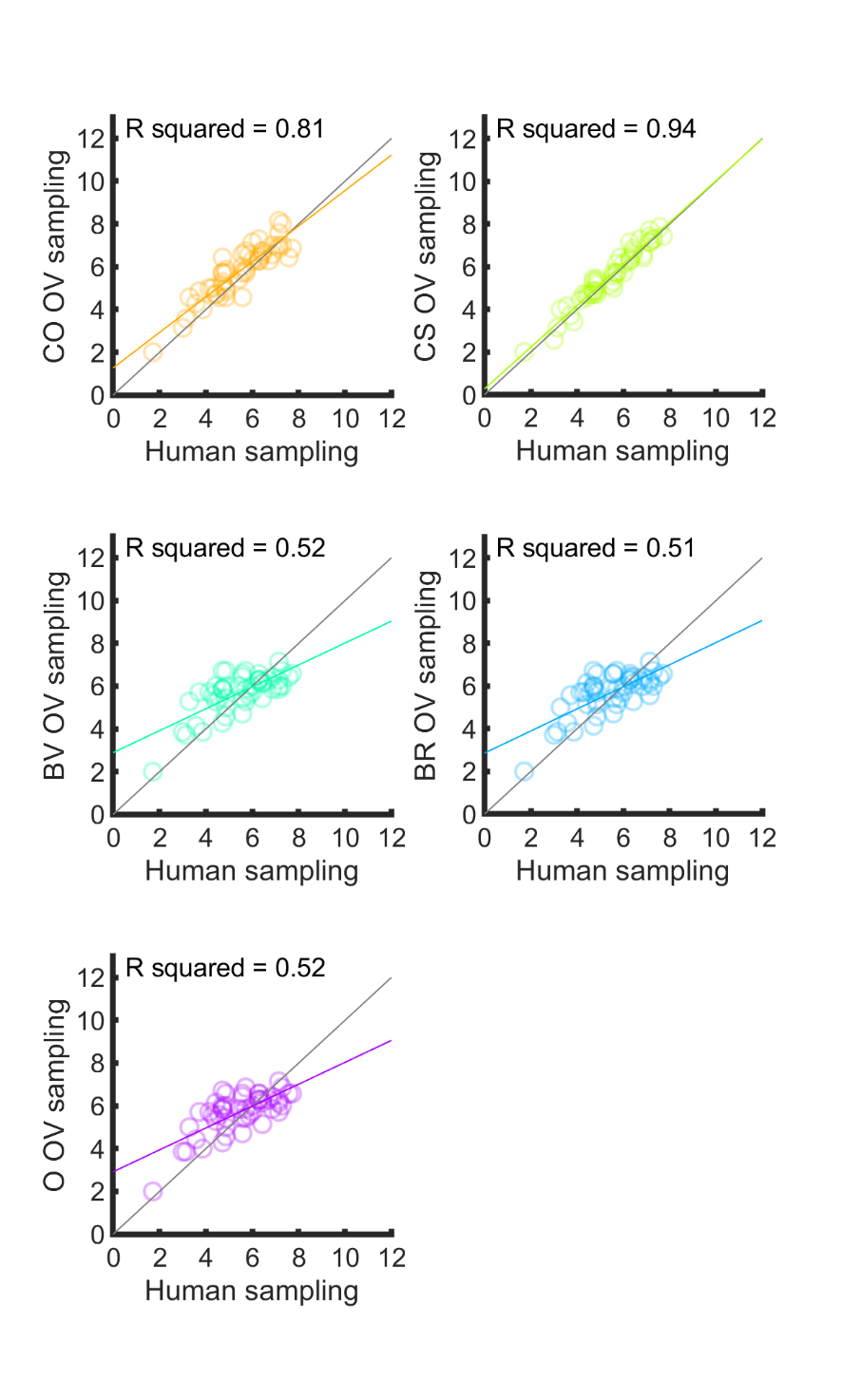


Figure S13. Model comparison for Study 1 ratings (left column) and full (right column) conditions. Top and middle rows show individual participants as points and bars show their mean values. The top row shows ranks of chosen prices. The second row plots the “first” or theoretical parameter values, estimated for each fitted model. The third row shows the “second”, or inverse temperature parameter beta, estimated for each fitted model. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

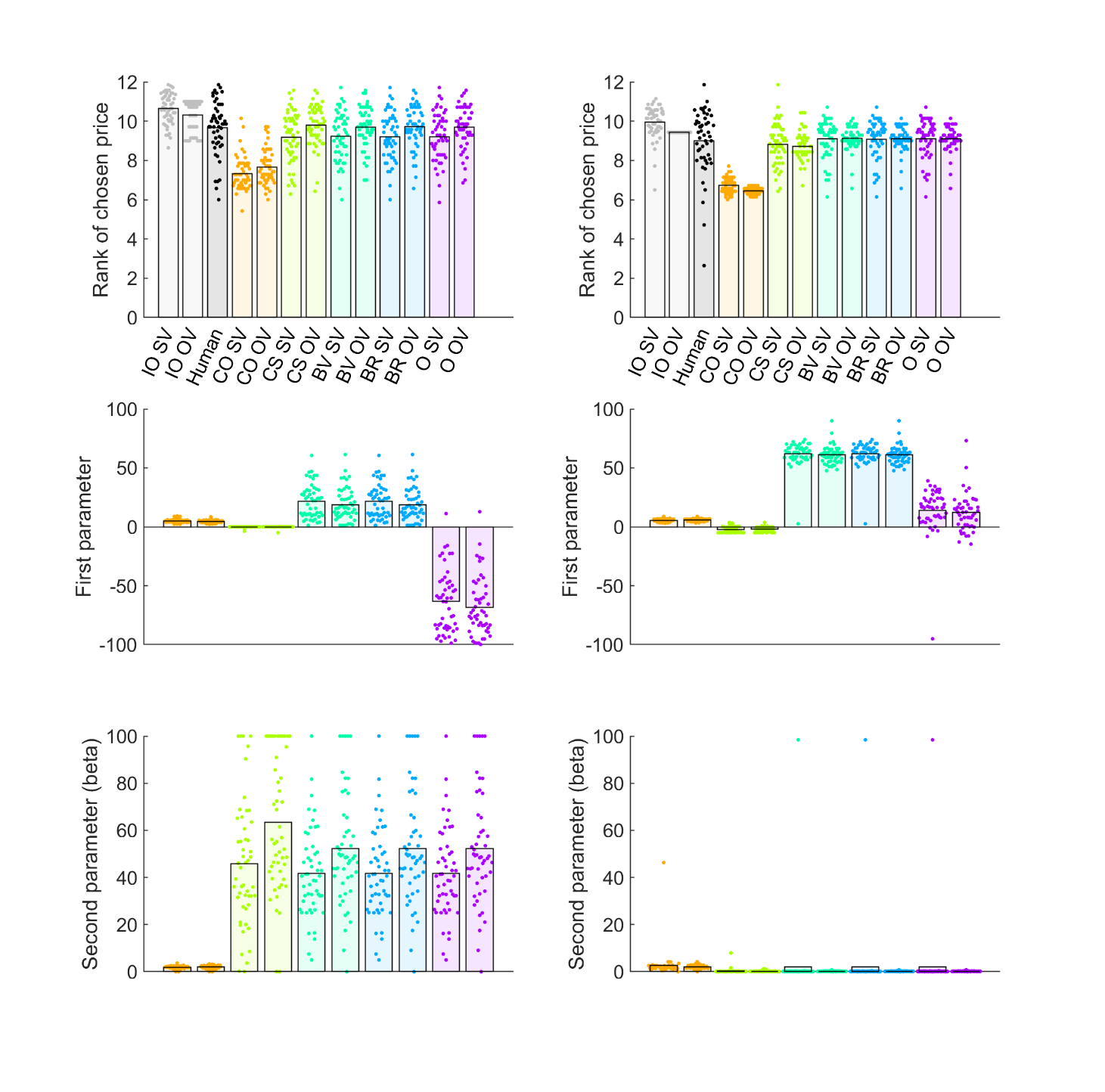


Figure S14. Linear relationships between human participants’ sampling in the Study 1 rating condition versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

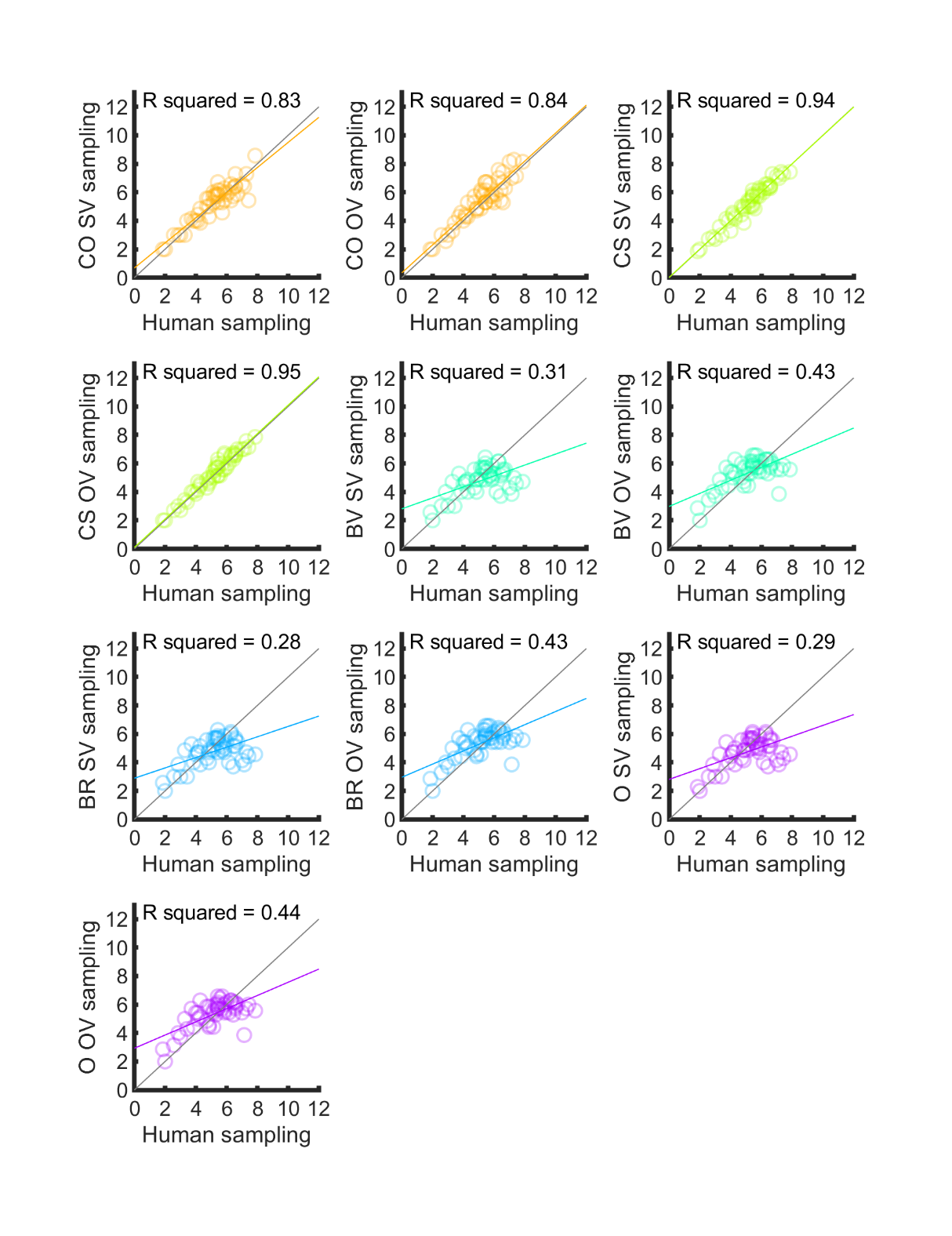


Figure S15. Linear relationships between human participants’ sampling in the Study 1 full condition versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS and CO models both predicts human data with high accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

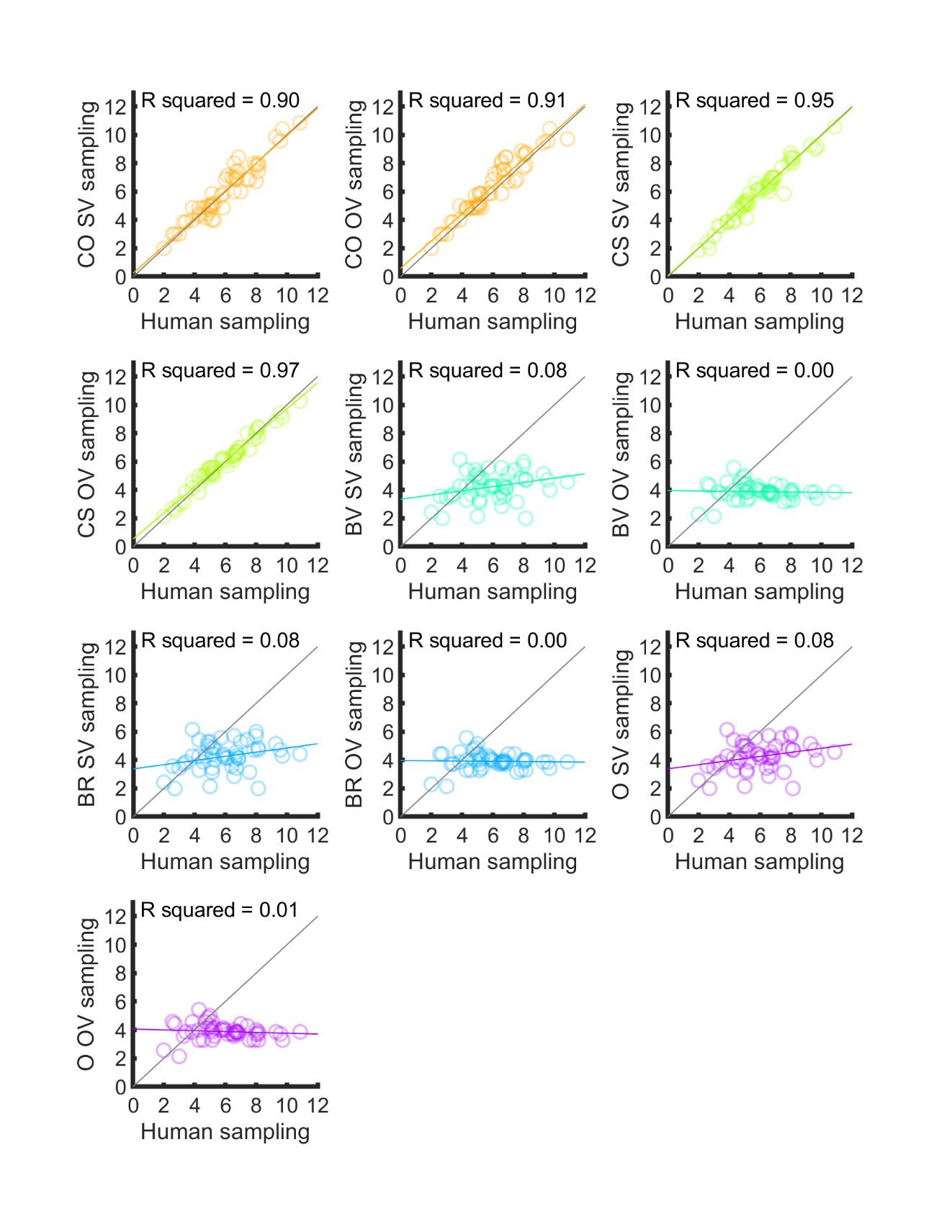


Figure S16. Model comparison for Study 2. Top and middle rows show individual participants as points and bars show their mean values. The top row shows ranks of chosen prices. The second row plots the “first” or theoretical parameter values, estimated for each fitted model. The third row shows the “second”, or inverse temperature parameter beta, estimated for each fitted model. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

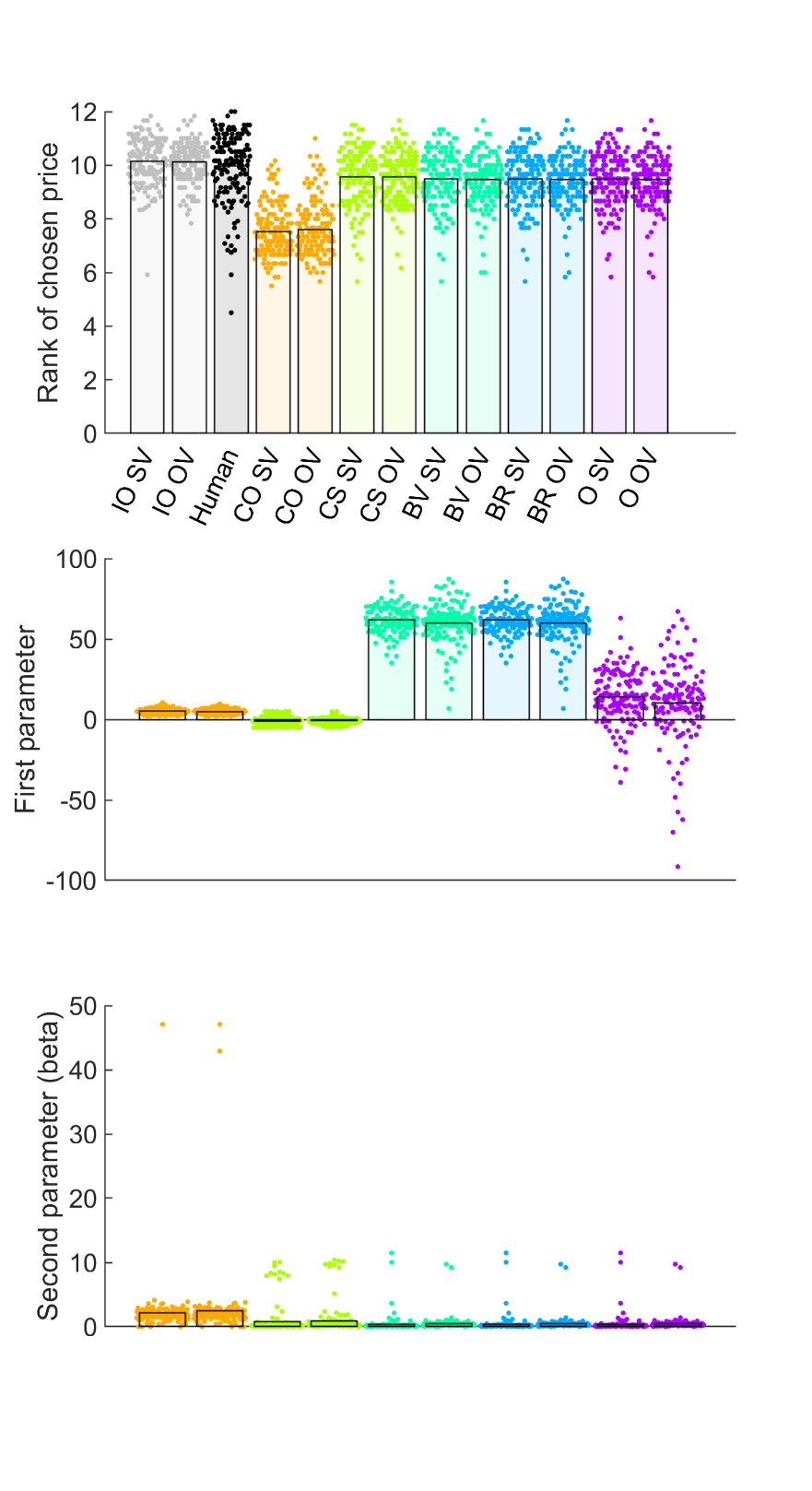


Figure S17. Linear relationships between human participants’ sampling in Study 2, versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

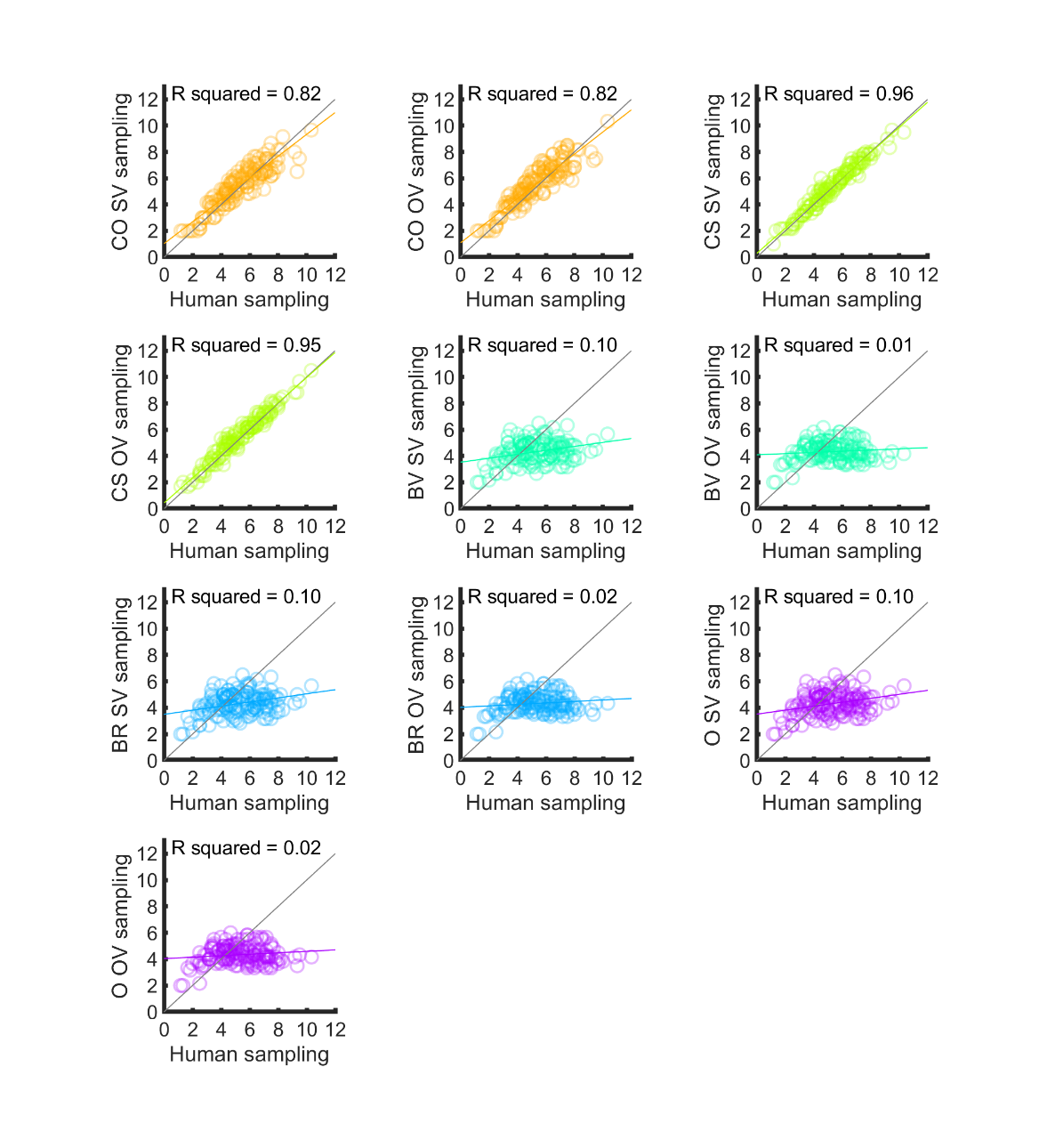


Figure S18. Model comparison for Study 3 10 options (left column) and 14 options (right column) conditions. Top and middle rows show individual participants as points and bars show their mean values. The top row shows ranks of chosen prices. The second row plots the “first” or theoretical parameter values, estimated for each fitted model. The third row shows the “second”, or inverse temperature parameter beta, estimated for each fitted model. Abbreviations: IO = ideal observer, CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

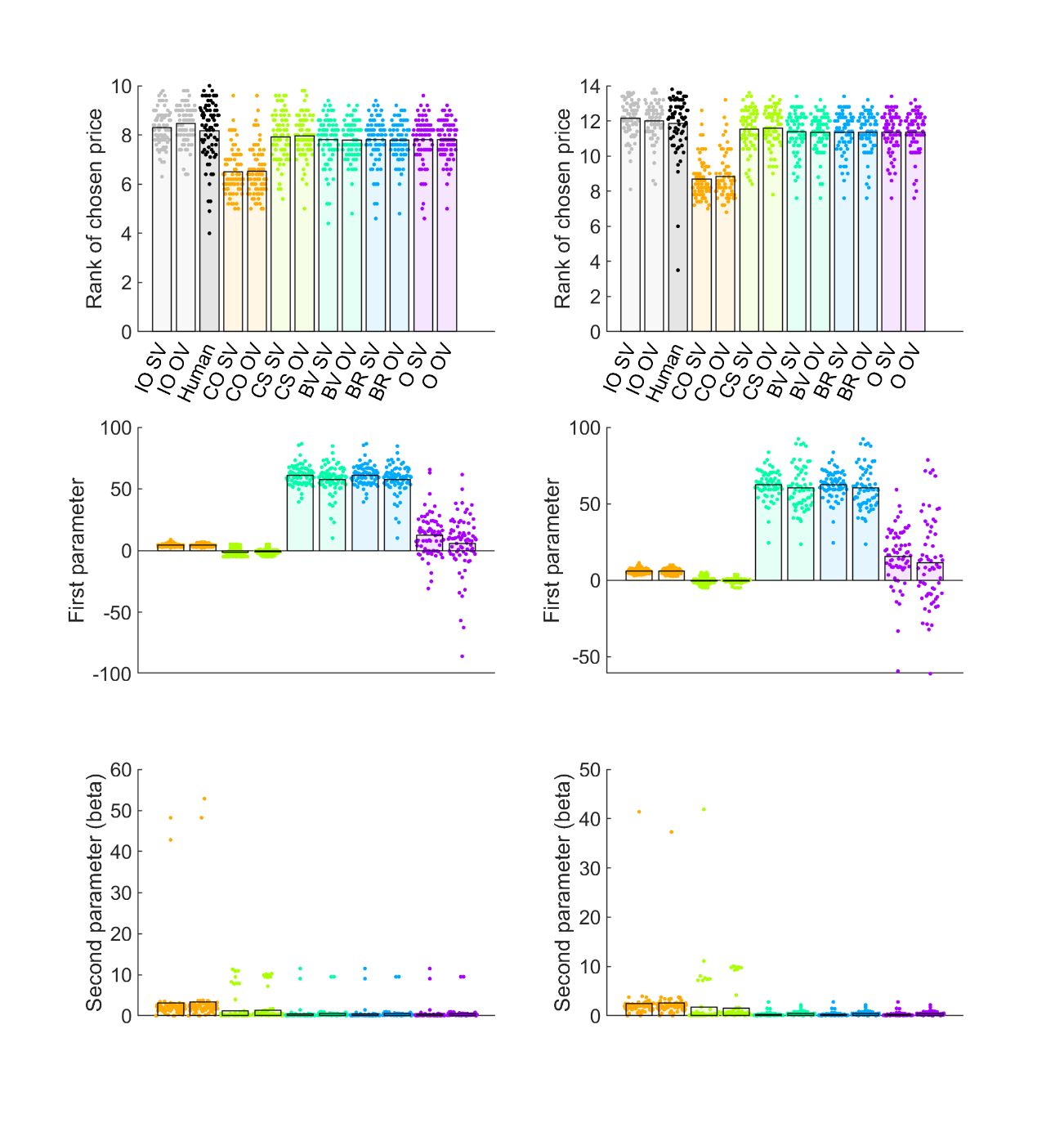


Figure S19. Linear relationships between human participants’ sampling in Study 3 10 options condition, versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

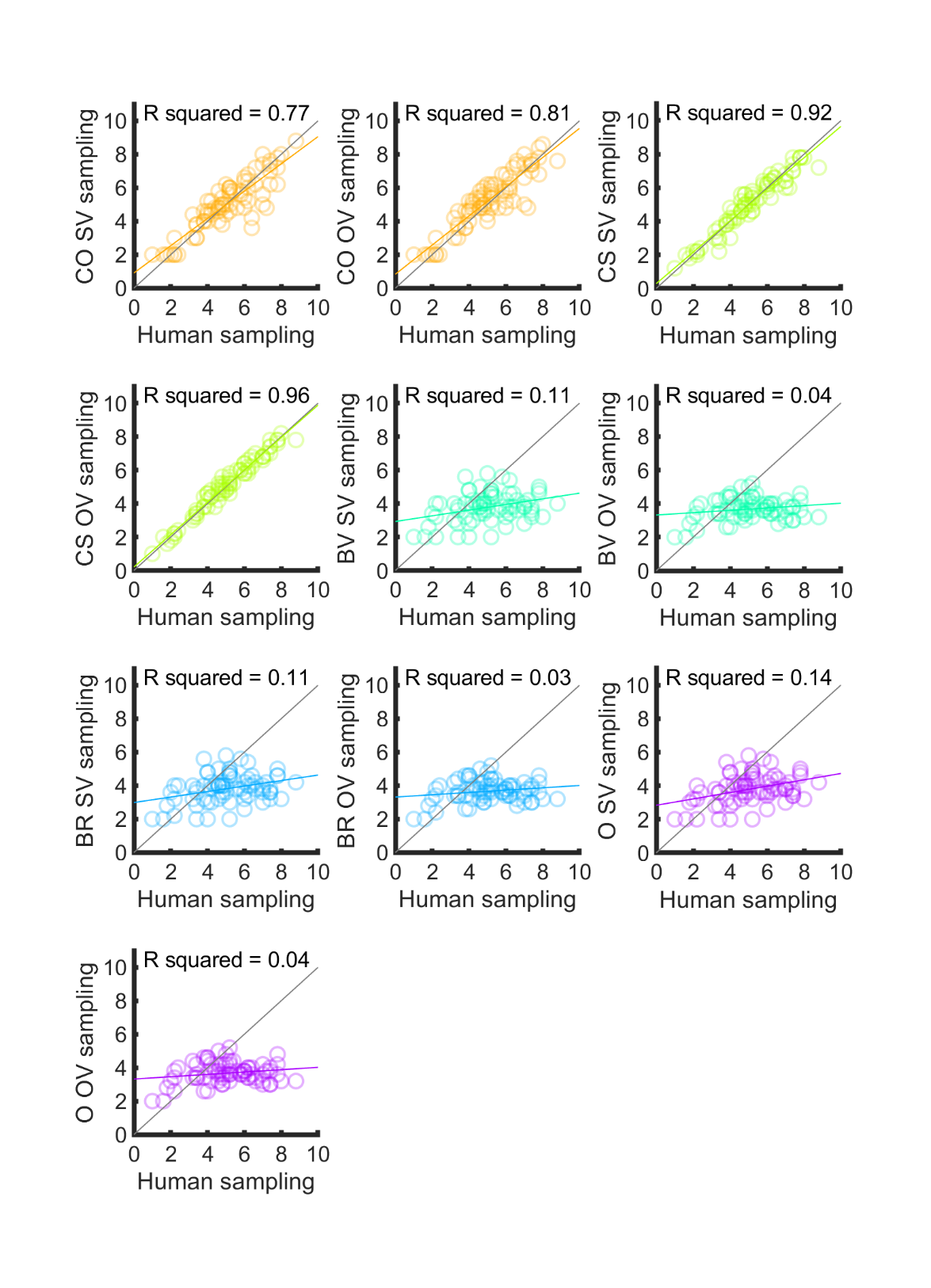


Figure S20. Linear relationships between human participants’ sampling in Study 3 14 options condition, versus sampling of corresponding models. The grey diagonal represents where participant and model numbers of samples are equal. The coloured line represents the regression line, with corresponding *R2* printed on plot. The CS model predicts the human data with the highest accuracy. Abbreviations: CO = cut-off, CS = Cost to Sample, BV = Biased Values, BR = Biased Reward, O = optimism, OV = objective values, SV = subjective values.

