Minding the Gap:
On the Origins of the Description-Experience Gap*

Ryan Oprea† Ferdinand M. Vieider‡

February 8, 2026

Abstract

We provide evidence that "noisy coding" may be responsible for both (i) classic probability-dependence of risk-taking and (ii) its reversal when the properties of lotteries are learned by sampling rather than by explicit description. Guided by a stylized model, we show that simply forcing experimental subjects to sample redundant information about the primitives of lotteries causes both types of probability-dependence to disappear, closing the description-experience gap and resulting in broadly neoclassical, probability-independent risk-taking behavior. This suggests that these anomalies are a joint outgrowth of decision makers' noisy representations of the primitives of lotteries rather than of their taste for risk.

Keywords: risk taking; noisy coding; probability weighting; decision from experience
JEL codes: C91, D91, G0

1 Introduction

One of the signature findings in behavioral economics is *probability-dependence*: as hundreds of experiments have shown, experimental subjects, when given explicit descriptions

*We are grateful to Larbi Alaoui, Rava Azeredo da Silveira, Ido Erev, Cary Frydman, Richard Gonzalez, Ralph Hertwig, Alex Imas, Sebastian Olschewski, to participants at the Economics Psychology Seminar in Basel, the EVRE Workshop in Grenoble, the 2023 RISL$\alpha\beta$ Workshop, the Workshop in Honor of Peter Wakker, and the Summer School on the "Cognitive Foundations of Decision-Making" for helpful comments and suggestions. All errors remain our own. This research was supported by the National Science Foundation under Grant SES-1949366 and by the Research Foundation Flanders under the project "Causal Determinants of Preferences" (G008021N). It was approved by UC Santa Barbara IRB.

†Oprea: Haas School of Business, University of California, Berkeley, roprea@gmail.com.
‡Vieider: RISL$\alpha\beta$, Economics Department, Ghent University, Belgium, fvieider@gmail.com.

of lotteries (decision from description or DfD), tend to be more risk-taking for small probabilities and relatively less risk-taking for large probabilities of winning a prize. This *negative probability-dependence* has been enshrined as a centerpiece of alternatives to standard expected utility theory (EUT; von Neumann & Morgenstern 1944) such as prospect theory (Kahneman & Tversky 1979, Tversky & Kahneman 1992). However, more recently researchers have discovered (Barron & Erev 2003, Hertwig et al. 2004) that when subjects are required to discover the properties of lotteries by sampling from them instead of by reading explicit descriptions of their properties (decision from *experience* or DfE), the direction of this anomaly reverses: subjects instead tend to be highly risk averse for low probabilities, and risk-taking tends to *increase* in the probability of winning a prize (*positive* probability-dependence). As a result, there is a gap (the "description-experience gap") between risk-taking behavior under DfD and DfE – a regularity that is difficult to rationalize under standard utility theory and which, to date, lacks a unified theoretical explanation.

Understanding why probability dependence occurs and reverses when lottery properties are learned by experience is central for understanding the drivers of risk-taking behavior. Decision from experience has been largely explained through sampling error (Hertwig et al. 2004, Fox & Hadar 2006, Hertwig & Pleskac 2010), but removing sampling error has failed to eliminate the gap (Hau et al. 2008, Ungemach et al. 2009) between DfE and standard decision from description, DfD. We propose a unified explanation for probability-dependence and its reversal in DfE vs. DfD based on "noisy cognition" — the idea that people form noisy representations of choice primitives and respond to this noise in a Bayesian manner. Noisy cognition models have recently been proposed as an explanation of standard probability-dependence in DfD (Zhang et al. 2020, Enke & Graeber 2023, Oprea 2024, Vieider 2024*b*, Frydman & Jin 2025, Khaw et al. 2025). We extend this theoretical framework to the sampling-based learning that occurs in DfE, and show that 1) noisy cognition can explain standard patterns observed in description- and experience-based choices; 2) by leveraging the insights from the model on the causes of probability-dependence in DfD, we can experimentally remove standard probability-dependence; and 3) by combining this treatment with a similar intervention on DfE, we can empirically close the the gap between description-based choice and experience-based choice.

The description-experience gap and its significance. Suppose a decision maker (DM) has to make a choice between a sure amount c and a lottery that pays $x > c$ with probability p (and $y < c$ otherwise).[1] In what has come to be the standard protocol, DMs are explicitly

[1] We will use this simple choice as a running example, and our experiment will exclusively employ such

told how many outcomes each lottery can produce, the payoffs each outcome results in and the probabilities of each outcome. The DM uses this information to choose the lottery she prefers. Call this standard paradigm "decision from description", or *DfD*. More recently, researchers have studied an alternative paradigm to DfD for studying lottery choice. In "decisions from experience" (DfE) experiments (Barron & Erev 2003, Hertwig et al. 2004), subjects are told nothing about the two lotteries but must learn all of their properties entirely by *sampling* each of them. In standard DfE experiments (under the so-called "sampling paradigm"), subjects choose how many times to sample each lottery and use the information gleaned from these samples to make their decision.

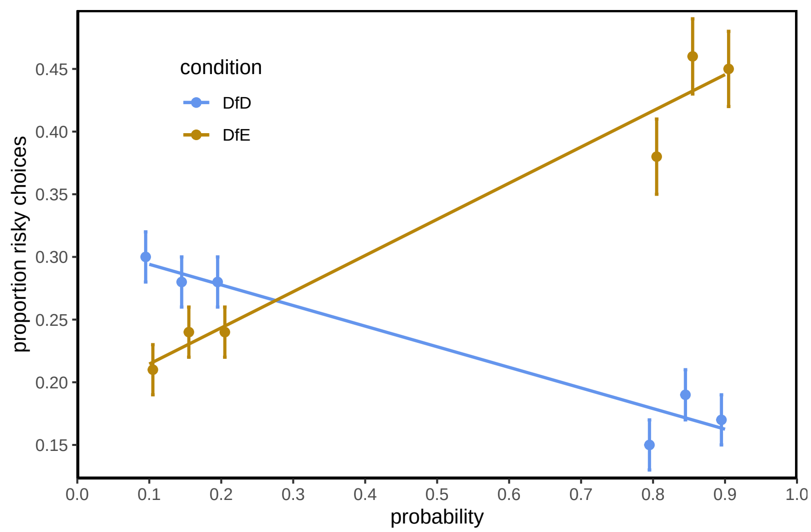


Figure 1: The GAP: Decisions from Description vs Decisions from Experience

The figure shows choice proportions for the risky lottery in DfD and DfE, ordered by probability of winning. The figure shows between subject comparison based on identical tasks that are either described (DfD) or sampled (DfE), fitted with a linear regression line. The tasks are constructed in such a way that the sure amount varies symmetrically around the expected value of the lottery (see below for details). The error bars indicate ±1 standard error.

Figure 1 illustrates the very different patterns of risk-taking over the probability interval one observes in DfD vs DfE, using data from a replication experiment we conducted (see below for details). When facing lotteries with a small probability of winning, DMs take more risk in DfD than in DfE. This tendency, however, flips for large probabilities of winning: subjects are now much more risk-taking in experience-based choice. The inverted responses in DfE and DfD produce what the literature has called the "decision-experience gap" (hereafter, simply the *GAP*) in lottery choice. The significance of this GAP stems

simple choices. Our framework extends to losses in a straightforward manner. It can also be extended to multi-outcome lotteries via an N-dimensional generalization; see the Online Appendix for details.

from the observation that it remains an open mystery in the literature and therefore raises open questions about the nature of risk-taking. In particular, it is difficult to square with the predictions of standard behavioral models of risk-taking such as prospect theory — and has resisted repeated attempts at empirically closing it (reviewed below). Understanding the source of this gap is crucial to understanding not just probability-dependence, but the determinants of risk-taking more generally.

Our contribution. In this paper we offer a theoretical explanation for the GAP that also explains the nature of probability-dependence in risk-taking. We argue that probability-dependence is an outgrowth of noisy beliefs about probabilities (à la "noisy coding" models) in *both* DfE and DfD, but that differences in the way information is gathered in the two settings causes that dependence to express in opposite directions, generating the GAP. We provide evidence from a series of experiments that strongly supports this explanation.

As a number of recent papers have argued (Zhang et al. 2020, Enke & Graeber 2023, Oprea 2024, Vieider 2024*b*, Frydman & Jin 2025, Khaw et al. 2025), limitations in the brain's ability to represent numerical information like described probabilities will tend to produce noisy beliefs about those probabilities which can in turn generate the standard pattern of negative probability-dependence widely observed in DfD. In particular, if people respond in a Bayesian way to their noisy understanding of described probabilities when forming judgments, their beliefs about likelihoods will be compressed towards their prior expectations, generating the characteristic negative probability-dependence typical of DfD.

We show that the same kind of noise in representations of probabilities will naturally arise also in DfE but produce very different effects. In particular, when learning probabilities by sampling, samples are unavoidably finite and therefore noisy, producing sampling variance and limited confidence in sampled information. This has two natural consequences. First, noisy beliefs endogenously determine how much people sample and therefore the degree of sampling *error* afflicting their probability judgements — a key element for understanding experience-based choices (Hertwig et al. 2004, Fox & Hadar 2006, Hertwig & Pleskac 2010). Second, just as in DfD, the noise generated by the resulting sampling variance produces Bayesian compression towards prior expectations. For pessimistic, risk averse decision makers (which comprise the majority of subjects), both of these effects will be shaped by a pronounced asymmetry. In low-probability lotteries sampling error will *confirm* prior expectations, causing subjects to draw small and therefore noisy samples, leading them to make decisions that comply with their risk averse priors. On the other hand, in high-probability lotteries sampling error will *contradict* prior expectations, causing subjects to gather larger samples, reducing sampling error but also sampling variance, and generating behavior further from their risk averse priors than at low probability lotteries. This results in the characteristic *positive* probability-dependence observed in DfE — the exact opposite kind of probability-dependence in DfD — generating the GAP. We show that a number of distinctive secondary predictions of this explanation are strongly borne out in the data.

Crucially, this explanation for the two types of probability-dependence allows us to explain why the literature has had difficulty eliminating the GAP. Efforts to close the GAP by forcing subjects in DfE to sample more intensively than they naturally would — as done e.g. by Hau et al. (2008, 2010), Ungemach et al. (2009), Aydogan & Gao (2020), Cubitt et al. (2022) — has the additional effect of simultaneously removing the sampling variance needed for standard probability-dependence to occur, thereby eliminating standard positive probability-dependence.[2] This means the GAP can never be eliminated by forcing subjects in DfE to observe larger samples alone: according to our model, in order to close the GAP, we must also increase the precision of neurally coded probabilities in DfD, reducing classical probability-dependence in described choices just as forced sampling does in experience-based choices.

We test this prediction by introducing a treatment in which subjects are required to take large, balanced samples from *fully described* choice options (i.e., in DfD). Even though such samples are completely redundant from the perspective of preference-based models such as prospect theory, they should nonetheless impact choice under models of noisy cognition. Our results indicate that, indeed, such samples have a large effect: after observing these forced samples probability dependence largely disappears and subjects instead make mildly risk averse lottery choices that broadly comply with standard EUT. Doing this completely closes the description-experience GAP for the first time: forced sampling causes DfE and DfD behaviors to converge. We interpret this as evidence that noisy cognition is likely the source of the description-experience GAP.

Finally, we supplement this evidence with an additional treatment that allows — but does not force — subjects to freely sample from described choices. This allows us to show, first of all, that subjects do indeed feel a need to sample, even for fully described choices (and

[2]Note that our results here are fully consistent with previous attempts at closing the GAP in similar ways, such as the one of Ungemach et al. (2009). Just like Ungemach et al. (2009) find, we find that forced sampling in DfE alone does not close the GAP. By using richer choice tasks, however, we can directly test for probability-dependence in risk-taking, something that, to our knowledge, previous studies could not do directly.

even in the presence of considerable opportunity costs). Just as importantly, our model predicts that this treatment will introduce sampling error into description-based choice. Our data show that this is indeed the case. By letting subjects freely sample from fully described choice options, we introduce positive probability-dependence of the sort observed in DfE into DfD. Although some differences in the degree of probability-dependence remain, this simple intervention largely closes the GAP in a second way, again implicating noisy cognition in the GAP.

Fit with the literature. Our paper contributes to several literatures. First is a long running literature on probability-dependence in risk-taking and related anomalies, going back to Preston & Baratta (1948). Such probability-dependence became a key component of prospect theory, where it is captured by an inverse S-shaped probability weighting function (Kahneman & Tversky 1979, Tversky & Kahneman 1992, Tversky & Wakker 1995, Wakker 2010), and is the mechanism by which that theory accounts for phenomena like the coexistence of lottery play and insurance uptake and the Allais paradoxes. Numerous empirical studies have documented systematic increases of relative risk aversion in the probability of winning a prize — Imai et al. (2025) provide a meta-analytic overview of the evidence in DfD.

The second is a literature documenting the gap between DfD and DfE (Barron & Erev 2003, Hertwig et al. 2004). Sampling error was proposed as an early explanation for the GAP (Fox & Hadar 2006). However, subsequent investigations showed that, although sampling error is an important contributor to the GAP, interventions including (i) eliminating sampling error by matching probabilities in DfD to DfE, (ii) increasing the samples by offering higher stakes, and (iii) forcing people to sample the complete urn in DfE fail to eliminate the GAP (Hau et al. 2008, Ungemach et al. 2009, Hau et al. 2010, Hertwig & Pleskac 2010, Wulff et al. 2018). Because of this, the underlying causes of the GAP have largely remained a mystery — see Hertwig & Erev (2009) and de Palma et al. (2014) for narrative reviews, and Wulff et al. (2018) for a systematic meta-analysis of the decision-experience gap and possible factors contributing to it. Cubitt et al. (2022) present a careful experimental decomposition, which concludes that sampling error is the prime driver of the GAP (but once more fails to close the GAP by eliminating sampling error, pointing towards missing pieces in the explanation).

The third is a growing literature documenting the role noisy cognition plays in behavioral anomalies (Natenzon 2019, Khaw et al. 2021, Frydman & Jin 2022). Most closely related is a line of research examining how cognitive noise (and efficient ways the brain deals with such noise) contributes to distorted perceptions of probabilities (Zhang & Maloney 2012, Steiner & Stewart 2016, Zhang et al. 2020, Enke & Graeber 2023, Herold & Netzer 2023, Netzer et al. 2025, Frydman & Jin 2025, Oprea 2024, Vieider 2024*b*, Khaw et al. 2025). More broadly, our work is related to a literature documenting the role cognitive frictions play in decision-making under risk (Enke & Graeber 2023, Bohren et al. 2024, Oprea 2024) and, broader still, the way cognitive constraints and the brain's response to these constraints explain a wide class of anomalies in decision-making (Simon 1959, Robson 2001*a*,*b*, Netzer 2009, Robson & Samuelson 2011).[3]

2 Drivers of risk-taking in DfE

What is responsible for the GAP between DfD and DfE and the reversals of probability-dependence that produce it? We begin with DfE and highlight two basic features of sampling that shape choice in that environment. The first (as the prior literature has emphasized) is *sampling error*: unless the DM collects a very large sample, she runs the risk of drawing misleading samples that systematically distort beliefs about the nature of sampled lotteries, particularly at extreme probabilities. The second (which has not been emphasized in the literature on the GAP so far) we will call *sampling variance*: because the DM's sample is finite, she cannot be entirely confident in the sample she draws. The resulting imprecision of sampled information makes it optimal for decision makers to combine sampled information with their prior expectations about lottery characteristics in a Bayesian fashion, leading to a distortion in their posterior beliefs. As we will show, these two features interact, and together produce the positive probability-dependence of risk-taking typically observed in DfE.

To preview our argument, while sampling bias alone *can* produce the positive probability-dependence typically observed in DfE, we argue that in practice sampling variance (and the Bayesian compression towards prior expectations it induces) actually plays an equally important role. Because most subjects have pessimistic prior beliefs about lottery payoffs,

[3] A recent, contemporaneous paper, Bohren et al. (2024), documents and decomposes a complementary description-experience gap that operates in richer environments than the one we (and the previous description-experience literature) study. In evaluating realistic lotteries with many potential outcomes (e.g., eleven states), they show that subjects' behavior tends to be constrained by memory limitations in DfE, while it tends to be constrained by attentional limitations in DfD. This leads to systematic differences in lottery choices in DfE and DfD environments — a gap that can be eliminated with aids to attention and memory. Memory seems to play much less of a role when studying simpler choice sitautions such as the ones we use here — see the GAP decomposition by Cubitt et al. (2022) for details.

they are pre-disposed to make risk averse choices in the absence of strong evidence. The effect this prior has on behavior in DfE, however, will be systematically different for high vs. low probability lotteries (i.e., lotteries with a high vs. low probability of paying x):

- When sampling **low probability lotteries**, initial draws tend to be negative, reinforcing the DM's pessimistic prior and causing them to stop sampling early. Because the samples are small (and therefore noisy), Bayesian compression to the prior mean is strong: the DM's pessimistic prior dominates beliefs (reinforced by a highly biased, small sample) leading to risk-averse choices.

- On the other hand, when sampling **high probability lotteries**, initial draws tend to be positive. This conflicts with the DM's pessimistic prior, inducing them to sample more, thereby reducing sampling variance and causing Bayesian compression to be much weaker than at low probabilities. As a result, even though the DM's larger sample has relatively less sampling error, the fact that it is less noisy causes the DM to put less weight on their pessimistic prior beliefs and more on the sampled evidence. This causes them to make, on net, more risk-tolerant choices than at low probabilities.

Thus, due largely to noise-driven differences in the confidence the DM has in the samples drawn (and the degree of Bayesian compression this induces), DfE produces (on average) positive probability-dependence: greater risk aversion at low probabilities than at high probabilities. This is a joint consequence of the way noise generates differential sampling and regression to the mean of prior beliefs at low versus high probabilities. As we will show in Section 3, a number of secondary patterns in the data are highly consistent with this explanation.

To fully specify a model of DfE, we must describe not only how people form beliefs about probabilities and payoffs, but also how these beliefs co-evolve with higher order beliefs about the structure of the lotteries (e.g., the number of outcomes in each lottery's support). To close the model, it is therefore necessary to make a number of detailed modeling choices about the evolution of these structural beliefs that do not directly impact the way we interpret and design our experiments. In the Online Appendix A.1 we propose such a fully specified model.[4] But in this section, for expositional ease, we abstract from these issues

[4] In the full version of the model in Online Appendix A, we close the model by assuming that (i) subjects mainly use samples to build beliefs about the comparative properties of the two choice options (which seems likely given the choice subjects face), (ii) that subjects know that they are making a risky choice and that the choice is therefore not between two degenerate lotteries (which seems likely given the lotteries subjects

of higher order belief formation altogether by (i) assuming that subjects already know the structure of the lotteries[5], (ii) assuming that subjects quickly identify which lottery is risky during sampling and (iii) focusing attention on the way subjects evaluate the risky arm. In the fully specified, general model in Online Appendix A we discuss the implications of these assumptions, but argue that they are qualitatively irrelevant to the key matters at hand.

2.1 Sampling Error and Sampling Variance

To model the way beliefs change as a DM samples the simple binary lotteries in our experiment, let α be the number of draws in which the DM observed payment x and β the number of draws in which she observed payment y (where, as in the introduction, $x > c > y$). We model this sampling process using a Beta distribution with parameters α and β, producing a representation of the probability p of earning x equal to $\mathbb{E}[\,\widehat{p}\,|\,p\,] = \frac{\alpha}{\alpha+\beta}$ (i.e. the sampled mean probability $\widehat{p}$, given the true probability p).[6] We will assume that the DM's beliefs are represented in a log-odds form. This is not necessary for any of our qualitative conclusions in what follows, but it is increasingly supported in neuroscience both empirically and theoretically.[7] As we emphasized above, these beliefs will be shaped in two ways by the finitude of the samples drawn: sampling error and sampling variance.

exclusively see in Part 1 of the experiment) and (iii) make a few other technical assumptions required to fully specify the joint inference problem. The main implication of (ii) is that inferences in which the outcomes observed in both choice options are attributed probability close to 1 will carry very high noise, in a sense to be made precise below. Within the formalism of the model, this assumption mainly serves to explain why subjects take more than 1 sample from each option.

[5] In our experiment, this is in fact a fairly realistic assumption, given that subjects entering DfE have all just made a number of lottery choices, all with the same structure.

[6] It is important to emphasize that our model *does not require us to assume* that the DM knows the structure of the decision problem. We use a Beta distribution here purely for expositional simplicity, and because binary lotteries is all a DM will ever experience in our experiments. Our model generalizes to any number of outcomes by using a Dirichlet distribution—the multi-dimensional generalization of the Beta—to represent the different states. Indeed, we can use Dirichlet distributions defined over all possible outcomes to explicitly model the inference process of the DM about the underlying state space in DfE—an important element that distinguishes our approach from some of the DfE literature in economics, which has assumed that the DM (often counterfactually) knows the objective state space or which has (in some papers) provided this information ex ante in experiments (Abdellaoui et al. 2011, Aydogan 2021, Cubitt et al. 2022). Online Appendix A provides details of the inference process, and of how the model we use here can be generalized to N states of nature.

[7] It is common in neuroscience to assume that the brain represents the sort of evidence encoded by α and β in terms of log-odds. This is in part because of its computational efficiency for the brain, a straightforward consequence of the fact that new evidence can be simply added to pre-existing evidence, which is a much less computationally expensive operation than, e.g., multiplication. It is also in part because of the empirical success of such representations. For instance, Zhang & Maloney (2012) describe log-odds representations as "ubiquitous", discussing a long list of findings which can be fit by log-odds representations. Glanzer et al. (2019) identify a unique empirical signature of log-odds representations, and argue that such representations underlie neural representations in general.

Sampling error. First, finite sampling will produce *sampling error.* Even though successes α and failures β are, on average, sampled in an unbiased way (i.e., $ln\left(\frac{\alpha}{\beta}\right) = ln\left(\frac{p}{1-p}\right)$ on average), the binomial distribution will produce samples that underestimate small probabilities and overestimate large probabilities (unless the samples are unrealistically large). This issue — typically termed *sampling error* in the DfE literature — has been discussed as one of the key potential drivers of the GAP from the very beginning (Hertwig et al. 2004). Fox & Hadar (2006) argued that sampling error may indeed be the *sole* driver of the GAP, and that eliminating it ought to result in choice patterns that converge towards those observed in DfD. The subsequent literature has thus devoted much energy to trying to eliminate sampling error, either by incentivizing or forcing subjects to take larger samples (Hau et al. 2008), by forcing subjects to take large, balanced samples from both choice options (Ungemach et al. 2009), or by only selecting samples that happen to reflect the true underlying probability (Wulff et al. 2018). Two key insights resulting from this literature are that 1) sampling error explains at least part of the GAP; but 2) while reducing or eliminating sampling error narrows the GAP, it fails to close it completely.

Unbiased samples in any given task will only obtain if the DM takes very large (technically: infinite) samples. As a result, we should expect the ratio of α and β observed by subjects in finite samples to produce systematically distorted impressions of the log odds. This is particularly true of samples taken from lotteries with extreme probabilities, where sampling error is most likely and where the gap between description and experience is most severe.

Panel A in figure 2 illustrates the sampling error occurring in small samples (see also Hertwig & Pleskac 2010, for an extensive discussion). The figure illustrates the error asymmetry — the excess likelihood of sampling a probability that is smaller than the true probability compared to a probability that is larger — for $p = 0.1$. At 10 samples (the first number that can theoretically result in a correct estimate), a DM is still 8.7 percentage points (pp) more likely to underestimate the true probability than to overestimate it. This error asymmetry subsequently decreases at a decreasing rate. At 100 samples (the number of non-representative samples imposed by Hau et al. 2008 in their experiment 3), the asymmetry in the direction of underestimation is still about 3pp. This highlights that 1) some degree of sampling error is almost inevitable for extreme probabilities in realistic samples; and 2) accuracy returns to increased sampling decreases rapidly once one exceeds a certain threshold.

Sampling variance. Second, finite sampling will produce *sampling variance.* Because α

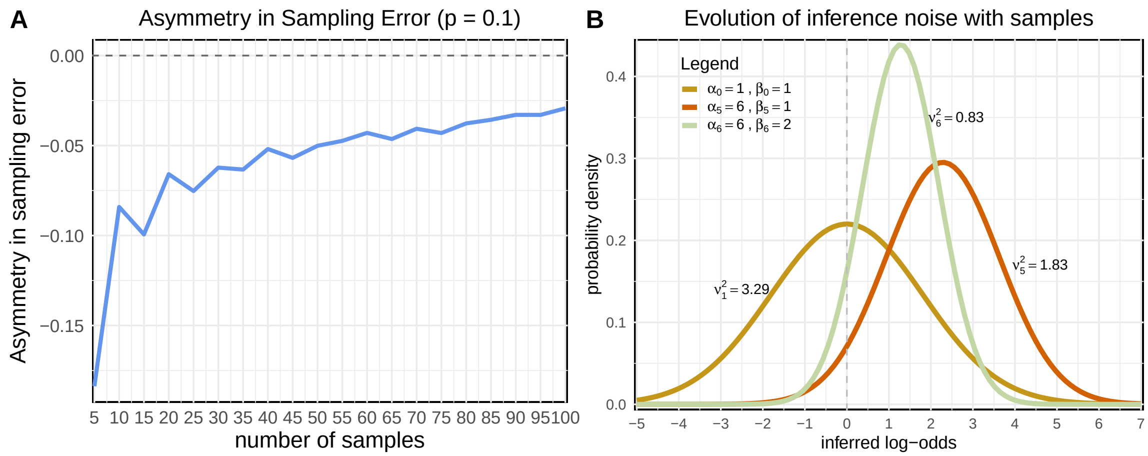


Figure 2: Sampling error and inference error in DfE

The figures show how sampling error and sampling variance evolve as samples accumulate. Panel A shows the likelihood to underestimate a probability of $p = 0.1$ as the number of samples increases (by steps of 5 samples). The smallest number of samples for which such a probability can be accurately estimated is 10. At 10 samples, however, the likelihood of underestimating the probability still exceeds the likelihood of estimating it correctly or larger by 7 percentage points. This asymmetry is reduced at a decreasing rate as samples increase. Panel B illustrates the evolution of sampling variance as a function of samples taken. Subsequent samples do not only update the "best guess" of the probability, but also reduce sampling variance.

and β are finitely sampled, they produce noisy beliefs about the true probabilities. Note that such beliefs will be noisy in finite samples even if the samples are accurate on average, and the DM can never be 100% sure of whether a *given* sample correctly reflects the underlying outcome-generating probability. Unlike sampling error, the prior literature (to our knowledge) has not considered the role sampling variance plays in generating the GAP. As we will argue below, accounting for it will help us to develop a unified understanding of the very different types of probability-dependence observed in DfE and DfD.[8]

Given samples of successes α and failures β, the "best guess" of the log-odds will be given by $ln\left(\frac{\alpha}{\beta}\right)$, which is the log-odds equivalent of the mean of the Beta distribution (the sampled proportion of successes x). The beliefs, however, must be augmented by an error term ε to capture variability in small samples. The log-odds formulation gives rise to approximately normally distributed errors even with relatively few observations (see e.g. Gelman et al. 2014, section 5.6), so that we will assume a logit-normal distribution for the errors. Following the characterization of the logit-normal distribution by Atchison & Shen (n.d.), it is straightforward to obtain an explicit solution for the sampling variance from the

[8] Olschewski & Scheibehenne (2024) present a discussion of different types of noise arising when DMs need to infer (and bet on) means of a series of sampled numbers, and present a concept of "Thurstonian uncertainty" that resembles what we here call sampling variance.

draws representing the odds:

$$\varepsilon \sim \mathcal{N}(\,0\,,\,\nu_n^2\,)\ \ ,\ \ \nu_n^2 = F'(\alpha_n) + F'(\beta_n), \tag{1}$$

where F' represents the trigamma function, and where we now subscript the samples and sampling variance by the number of samples n to emphasize the dependence of these quantities on the number of samples taken. The sampling *precision* ν_n^{-2} (i.e. the inverse of the sampling variance) will increase in the number of draws n. We can thus interpret the precision ν_n^{-2} as a measure of confidence in the sampled proportion $ln\left(\frac{\alpha_n}{\beta_n}\right)$.

Panel B in figure 2 illustrates how sampling variance evolves with subsequent samples. Let us assume a DM starts sampling from initial parameters $\alpha_0 = \beta_0 = 1$. This corresponds to an ignorance prior with a uniform distribution attributing equal ex ante likelihood to all probabilities (i.e. Laplace's rule of succession).[9] This distribution is centered on $p = 0.5$, but shows low confidence in that estimate (all probabilities are seen as equally likely). The distribution parameterized by $\alpha_5 = 6$ and $\beta_5 = 1$ shows the situation after 5 samples, all of which have yielded draws of the prize x. As one would expect, the mean estimate of the probability is now larger. Just as importantly, the distribution has narrowed — the sampling variance has decreased, thus increasing the 'confidence' the DM has in the sampled proportion. Assume now the DM draws a sample of y. This reduces the sampled proportion α/β, but also further increases the precision of the sample. This illustrates an important property of the model: sampling variance is a decreasing function of the number of samples, but constitutes a conceptually separate dimension from the specific samples drawn.[10]

2.2 Sampling and Beliefs

Optimal Bayesian Inference. Both the amount of sampling the DM engages in and her ultimate choice will be shaped in an important way by sampling variance. In particular because finite samples are noisy, a Bayesian DM will rationally combine the results of her sampling with her prior beliefs to draw inferences about the true underlying probability.

[9] Note that this specific value only serves illustrative purposes, and is in no way essential to our conclusions. This will become apparent shortly, when we will describe the Bayesian integration of the evidence from the sample with prior expectations. Online appendix A.1 further discusses the likelihood in a more general setting based on Dirichlet distributions used to infer the structure of the decision problem jointly with the probability attached to each state.

[10] Technically this orthogonality is not perfect, since both will depend on the number of samples drawn to some extent. The dependence will be particularly strong for very small and extreme samples, e.g. when only one single outcome has been observed.

This will produce biases in the DM's posterior beliefs.

Continuing with our log-odds characterization of beliefs, assume that the prior, too, takes logit-normal form:

$$ln\left(\frac{p}{1-p}\right) \sim \mathcal{N}\left(ln\left(\frac{p_0}{1-p_0}\right), \sigma^2\right). \tag{2}$$

As we show in more detail in Appendix A.2, the posterior expectation of the log-odds being inferred, $ln\left(\frac{\widehat{p}}{1-\widehat{p}}\right)$, conditional on the true log-odds, will take the following form:

$$\begin{aligned}\mathbb{E}\left[ln\left(\frac{\widehat{p}}{1-\widehat{p}}\right) \mid ln\left(\frac{p}{1-p}\right)\right] &= \gamma_n \, ln\left(\frac{\alpha_n}{\beta_n}\right) + (1-\gamma_n)\, ln\left(\frac{p_0}{1-p_0}\right) \\ &= ln\left(\frac{\alpha_n}{\beta_n}\right) + (1-\gamma_n)\left[ln\left(\frac{p_0}{1-p_0}\right) - ln\left(\frac{\alpha_n}{\beta_n}\right)\right],\end{aligned} \tag{3}$$

where $\gamma_n = \frac{\sigma^2}{\sigma^2+\nu_n^2}$ is the Bayesian evidence weight, i.e. the weight put on the sampled proportion relative to the prior expectation $ln\left(\frac{p_0}{1-p_0}\right)$. Importantly, γ_n is itself an increasing function of the number of samples n taken, since it is inversely proportional to the sampling variance ν_n^2 (technical details in Online Appendix A). The second line in (3) illustrates why this results in *biased inferences*: even for choice proportions α_n/β_n that are on average correct, the regression to the mean of the prior will systematically distort the inferences drawn.[11]

The discriminability equation. Sampling variance — and the regression to the mean of the prior it produces — will, in turn, determine how intensively the DM samples, i.e., at what point a DM concludes that she has sufficient information to stop sampling. This is a novelty of our approach as the DfE literature has paid relatively little attention to the determinants of the decision of when to stop sampling.[12] Here we will show that the sampling-stopping decision is endogenous to 1) the prior expectation of the DM; and 2) the

[11]The expression indeed shows the definition of bias, inasmuch as it illustrates regression to the mean of the prior as a source of systematic deviations from $ln\left(\frac{\alpha_n}{\beta_n}\right)$, which is an unbiased estimator of $ln\left(\frac{p}{1-p}\right)$ on average. It is important to note that — notwithstanding this systematic bias — the inference process is *optimal* given some constraints on sampling (e.g. in the presence of opportunity costs or time pressure, both of which apply in the context of our experiments). This happens because the bias introduced in each single inference must be traded off against the resulting reduction in the variance across trials. The estimator used here is optimal in the precise sense that it minimized the mean squared error. Bishop (2006), ch. 3, provides a proof of this optimality in a machine learning context.

[12]Some papers have described recency bias and the importance of the last samples, but as far as we are aware none has truly endogenized the sampling process. E.g., Hau et al. (2008) discuss opportunity costs of sampling and their dependence on the stakes of the experiment. Hertwig & Pleskac (2010) point at the strongly decreasing marginal informational content of additional samples as a possible reason for small samples, without however formalizing this intuition.

precision of the sample drawn. The decision on when to stop sampling will in turn determine the degree of sampling error and (because the sample size also determines sampling variance) the weight the DM places on the final sample relative to her prior expectations. Together, sampling error and variance will thus produce the positive probability-dependence of risk-taking characteristic of DfE.

To understand why DMs should be expected to take limited samples in DfE, we leverage the result on probabilistic inferences in equation (3). Assume a DM wants to maximize expected value, conditional on her inference on the probability of winning. In the simple choice problems we use this entails a choice rule in which the DM trades off the inference on the log-odds in (3) against the log cost-benefit ratio, $ln\left(\frac{c-y}{x-c}\right)$. For expositional simplicity, we will assume in this section that the log cost-benefit ratio (unlike the log-odds) is objectively perceived, though clearly it will in fact be learned by sampling just as the log-odds are. This assumption will have no impact on our qualitative predictions here but greatly simplifies the exposition.[13] In Appendix A.2, we show that this yields the following *discriminability* equation:

$$\psi_n = \frac{\gamma_n \times ln\left(\frac{\alpha_n}{\beta_n}\right) - ln\left(\frac{c-y}{x-c}\right) - ln(\theta_n)}{\nu_n \times \gamma_n}, \tag{4}$$

where $\theta_n \triangleq \left(\frac{1-p_0}{p_0}\right)^{1-\gamma_n}$ is the inverse (weighted) prior expectation. This inverse prior expectation can be interpreted as a measure of "risk aversion" within the model generated by the distorting influence of the prior.[14] In other words, risk aversion in the model can result from a pessimistic prior expectation by the DM about the types of lotteries she will face.[15] The subscript n indicates the current sample count, which plays an important role in the characterization of the dynamics underlying the equation.

Intuitively, equation (4) measures the accumulation of information as samples are taken. If ψ_n becomes sufficiently positive, indicating information favorable to the lottery, the DM will stop sampling and choose the lottery; if ψ_n becomes sufficiently negative, the DM

[13] It is straightforward to extend the model to include noisy representations of cost-benefit perceptions — see Vieider (2024*b*). Such noisy representations can, in fact, quantitatively enhance the patterns we describe here in sequence. In our structural model estimates in Online Appendix E, we take explicit account of the effects of sampling on the DM's beliefs about the cost-benefit ratio.

[14] We conceive of the quantities governing the prior p_0, and σ^2, as constant for the duration of the experiment. This is plausible in our setting where 1) subjects face the same choices in DfE that they have phased in part 1 on DfD; and 2) the experiment is very short.

[15] Note that we do not *assume* the prior to entail risk aversion. We rather treat it as a free parameter through which any underlying risk aversion of the DM may manifest in the model.

will stop sampling and choose the sure amount instead. Unless a negative or positive threshold is reached, she will keep sampling.[16] In particular Equation (4) trades off two dimensions: the weighted log-odds, $\gamma_n \times ln\left(\frac{\alpha_n}{\beta_n}\right)$, which present evidence in favor of taking the lottery; and the evidence against the lottery, given by the level of pessimism in θ_n, and the log-cost benefits. These two dimensions are weighed by their common standard deviation $\nu_n\gamma_n$, which measures the degree of confidence in the quantities being traded off. Given the normality assumption on likelihood and prior, ψ_n will follow a standard normal distribution. The cumulative distribution function of ψ_n can thus directly be used to predict choice probabilities. In a sampling framework such as DfE, however, α_n, β_n, and the derived quantities $\nu_n = \sqrt{F'(\alpha_n) + F'(\beta_n)}$ and $\gamma_n = \frac{\sigma^2}{\sigma^2+\nu_n^2}$, as well as ψ_n itself, will all evolve as a function of samples n. In such a model, the DM will stop sampling (and make a choice) only once she feels that she has sufficient information to reach a decision.

Importantly, reaching a positive versus negative decision threshold (and thus a decision) will depend on prior expectations incorporated in θ_n (as well as on the log-cost benefit ratio), making the problem asymmetric. Let us assume for simplicity that costs and benefits are equal. Intuitively, a risk averse DM — i.e. a DM with pessimistic prior expectations $p_0 < 0.5$ — will have less trouble accepting negative evidence (draws of the lower outcome y) and reaching the negative discriminability threshold than reaching a positive discriminability threshold after observing the same proportion of draws of the prize x. Since subjects tend to be risk averse, this means that we should expect asymmetric behavior in sampling and interpreting low relative to high probability lotteries. In particular we should expect risk averse DMs to sample less at low than high probabilities and thus (because of resulting differences in sampling variance) to rely less on sampled information (and more on prior expectations) when making decisions about low than high probability lotteries.

Illustrative Example. Figure 3 shows an illustration of how discriminability ψ_n evolves. The illustration is based on a slightly pessimistic DM with $p_0 = 0.45$ and for simplicity, we assume costs to be equal to benefits, so that their logged ratio drops out of the equation. To focus ideas, we further assume that in a series of 10 samples, the DM observes exactly one instance of x if $p = 0.1$ (and 9 of y), and just one instance of y for $p = 0.9$. The discriminability thresholds are set at ± 1.28, corresponding to a one-sided test with a 90%

[16]What amount of information exactly is deemed 'sufficient' by a DM can thereby be subjective and vary from DM to DM. In other words, the precise thresholds used do not affect the qualitative insights derived here. What is important is that the DM will stop sampling once ψ_n reaches a sufficiently extreme value, passing a subjective discriminability threshold. Note that thresholds may themselves change over time, as is the case in drift-diffusion modeling. Again, this does not affect the qualitative insights we derive here.

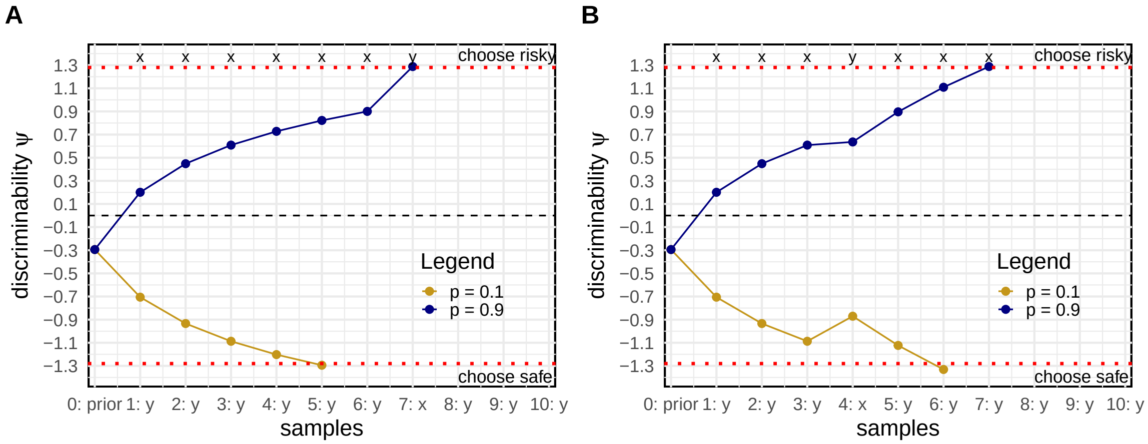


Figure 3: Evolution of discrimnability with samples

The figure shows how discriminabiity in equation (4) evolves with a series of 10 balanced samples. The DM is assumed to be mildly risk averse, with $p_0 = 0.45$ and to face equal costs and benefits. Panel A illustrates what happens when the outlying observation is drawn in sample 7, whereas panel B illustrates what happens if it is drawn in sample 4 instead.

confidence level. Panel A depicts the situation in which the less likely outcome is observed in the 7th sample. When sampling from $p = 0.1$, ψ_n hits the discriminability threshold after a uniform series of 5 'failures' y, leading the DM to stop sampling and choose the safe option. However, although the *samples* for $p = 0.9$ are the mirror image of those for $p = 0.1$, the evolution of ψ_n is asymmetrically different due to the pessimism in the initial prior. As a result, the DM takes 2 more samples, at which point ψ_n reaches the threshold and the DM chooses the lottery. Importantly (and perhaps counterintuitively), this happens even though the 7th and last sample is a draw of a 'failure' y.[17] This illustrates the effect of the increase in precision: even though the DM now slightly *under*-estimates the true probability, she nevertheless chooses the lottery because she now has high confidence in her estimate.

Panel B illustrates a further implication: even the position of the outlying observation in the 10 samples will influence the decision of when to stop sampling. Here, the less likely event is observed in the 4th draw instead of the 7th. While the positive threshold is still reached after 7 samples, reaching the negative one now requires one more draw than before.[18] There

[17] The discriminability equation seems to make a small 'jump' upon sampling of the failure y. This arises from the definition of sampling variance $\nu_n^2 = F'(\alpha_n) + F'(\beta_n)$, since the marginal increase of the trigamma function is largest at small values.

[18] An interesting consequence of this sort of stopping decisions may be apparent recency effects in DfE, as discussed e.g. by Erev & Barron (2005). In the context of our model, however, such recency effects would be mostly driven by the decision on when to stop sampling (see also Wulff et al. 2018, for a discussion of this point).

is also a general implication that emerges from this illustration: all sequences of 10 samples we used in the example are accurate in the sense that taking 10 samples results in the true underlying choice proportion being sampled. Nevertheless, the endogenous decision on when to stop sampling results in *sampling error* in all four cases. This illustrates how sampling variance and sampling error interact: sampling variance, in combination with the prior expectation, will determine when a DM stops sampling. Of course, in reality, 10 samples will typically *not* correctly reflect the true probability, as illustrated in figure 2 above. This will further increase the distortions introduced by the endogenous sampling-stopping decision.

The interaction between precision and sampling error results in a testable insight: ex ante risk averse DMs should sample more from large probability lotteries than from small probability lotteries on average, whereas risk-loving or optimistic DMs should do exactly the opposite. This prediction is novel, and has not previously been examined in the literature, making it a test that is particularly diagnostic of the value of our model for explaining and predicting behavior in DfE.

2.3 Implications for risk-taking

The decision on when to stop sampling, described above, (in conjunction with the log-cost benefits) will produce risk-taking patterns including the positive probability-dependence that famously arises in DfE. Risk averse (pessimistic) DMs facing *small probability* lotteries will stop sampling early because the accumulation of failures will tend to reinforce their prior expectation. Because these samples are small and therefore noisy, this will tend to cause the DM to lean heavily on their pessimistic priors in forming beliefs, pushing them towards the sure option and thus producing widespread risk aversion for small probability lotteries. This is reinforced by biased small samples that, due to sampling error, also make the risky lottery look unappealing. For large probability lotteries, by contrast, the initial series of successes drawn by a majority of DMs will clash with their pessimistic expectations. This prevents the positive discriminability threshold from being reached, leading DMs to take larger samples. Because sampling error decreases very slowly (cfr. figure 2, panel A), larger samples will still on average make the risky lottery look favorable, predisposing subjects to the risky lottery.[19] Reinforcing this, the increased precision of the larger samples will also reduce the

[19] It is important to note that these are *average* patterns. Some DMs may draw very favorable samples from small probability lotteries or very unfavorable samples from large probability lotteries, and thus take the opposite decisions.

weight the DM puts on the pessimistic prior expectation (i.e., $\theta = \left(\frac{1-p_0}{p_0}\right)^{1-\gamma}$ converges to 1 in the limit as γ increases to 1, and $ln(\theta)$ goes to 0), again making the DM more attracted to the risky lottery. This generates relatively high risk-taking for large probabilities and therefore the positive probability-dependence of risk-taking typically observed in DfE. This explanation for DfE behavior also generates a distinctive, diagnostic prediction: increased risk-taking at large (relative to small) probabilities — and positive probability-dependence more generally — ought to be driven primarily by DMs who are, ex ante, especially risk averse.

3 Sampling in Decisions from Experience

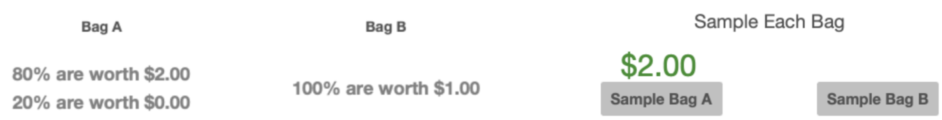
In this section we use data from Experiment 1 (discussed in the introduction) and a second experiment (Experiment 2) to examine the distinctive predictions of our model for DfE behavior developed in Section 2 above.

3.1 Experiment description

In Experiment 1, we replicate the GAP, as shown in Figure 1 in the introduction. Subjects face 18 distinct binary choices between a sure amount c and a lottery paying $x > c$ with probability p, or else $y = 0$. We further randomly pick 4 choice problems to be repeated. The lotteries vary p across 0.1, 0.15, 0.2, 0.8, 0.85 and 0.9 and vary payoffs x and c. The sure amounts c for a given probability include the expected value (EV) of the lottery, and two amounts that are symmetric around the EV of the lottery (i.e. $c = EV(x, p) \pm h$, where h is \$0.3 or \$0.4). This will allow us to get a rich picture of behavior, and is crucial to identifying probability-dependence in risk-taking. We did not include intermediate probabilities in the design, a choice that follows the DfE literature where the use of intermediate probability is rare since they are typically not very informative for the GAP.[20] Lotteries are described to subjects as "bags," containing 20 "coins," each of which is worth a different amount of money. At the end of the experiment, a lottery is randomly selected and a single coin is drawn from the bag to determine the subject's payment.

Treatments. Experiment 1 consists of two treatments. In the DfD treatment, the subject is explicitly told the properties of each lottery (i.e., the contents of each bag); Figure 4a shows a screenshot. A pair of radio buttons below the lottery description allows the subject

[20]This was also meant to limit the amount of clicking necessary in the experiment, given that the forced sampling treatments described below require 41 mouse clicks per task.



(a) DfD Treatment (b) DfE Treatment

Figure 4: Screenshots from Experiment 1.

to make and submit a choice between the two lotteries. In the DfE treatment, the subject is instead shown two buttons, one for each of the two lotteries/bags. Figure 4b shows a screenshot. When the subject clicks on the button, she is shown a single realization of the lottery (i.e., a single draw from the bag, *with* replacement). The subject is told nothing about either lottery and must learn all of their properties by sampling. The subject in the Figure 4b example has just clicked the "Sample Bag A" button and drawn $2. Each sample is shown for 0.5 seconds. Subjects are allowed to sample as many (or as few) times as they like from the two bags, with no time constraints. Below the sampling buttons are the same two radio buttons shown beneath the lottery descriptions in DfD, and the subject can choose one of the lotteries to determine her payment whenever she is ready.

Stages. Each session in the experiment proceeds in two stages. In Stage 2, subjects experience their main treatment: 22 randomly ordered lottery choices under DfD or DfE, depending on treatment. In Stage 1, subjects face the same 22 binary choice tasks under DfD (in a different random order). We included Stage 1 for several reasons. First, doing this allows us to examine the GAP both within-subject (by comparing Stage 1 and Stage 2 in the DfE treatment) and between-subjects (by comparing Stage 2 in the DfE vs. DfD treatment). Second, including Stage 1 is useful for fixing prior beliefs about lotteries and linking DfD and DfE behavior.

Implementation. We ran 99 subjects through the DfD treatment and 99 subjects through the DfE treatment on Prolific. We paid all subjects $6 and selected 10% of them to be paid based on a lottery outcome from a randomly selected task. The median subject spent 18 minutes in the experiment and the average subject earned $18.67 per hour. Instructions, including 4 comprehension questions, are included in Online Appendix F.

3.2 Results: Sampling, Sampling Error, and Risk-Taking

Sampling patterns. We start by testing the key endogenous sampling predictions from our model, outlined in the previous section: (i) sampling behavior should vary with the probability of the prize, p; and (ii) this dependence should vary according to the subject's pre-existing level of risk aversion as captured by the prior mean. To test this, we categorize DfE subjects according to their measured risk aversion using their propensity to choose risky lotteries in Stage 1 by quantifying the proportion of Stage 1 risk averse choices subjects make.[21]

In Figure 5, panel A, we plot the mean number of samples taken from the risky option in Stage 2 as a function of probability p for subjects classified as High and Low risk aversion based on a median split of risk averse choices in the first, DfD stage of the experiment. We find clear evidence of the predicted pattern: highly risk averse subjects sample substantially more at high than at low probabilities; relatively risk tolerant (Low risk aversion) subjects show (somewhat weaker) evidence of the *reverse* sampling pattern. Given that most subjects in our sample are risk averse this results in an overall average tendency for subjects to sample more for larger than smaller probabilities. These results strongly support the idea that sampling precision interacts with prior beliefs to determine sampling behavior in DfE.

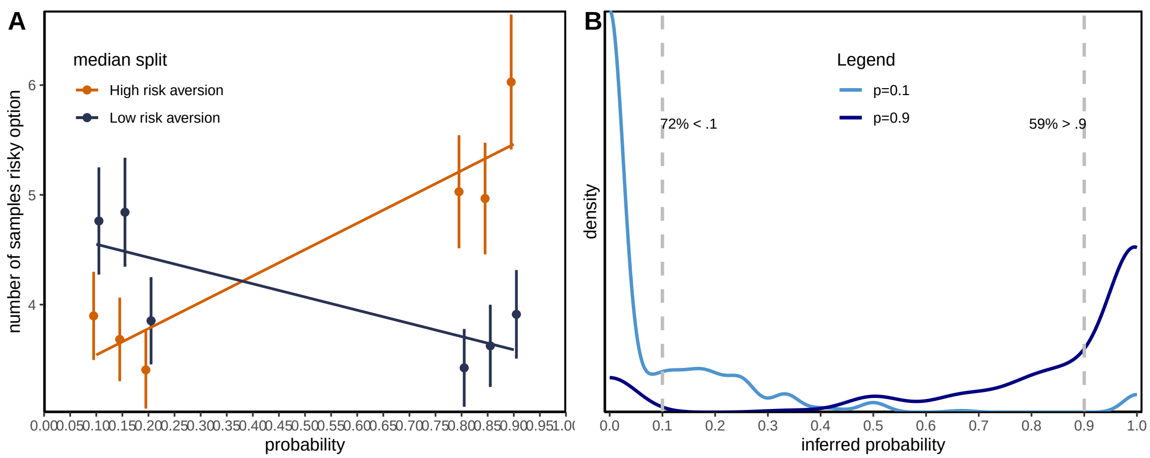


Figure 5: Samples by probability and risk aversion

Panel A shows the number of samples taken from the risky option by probability and risk aversion. Risk aversion is assessed as the proportion of safe choices in the first, DfD part of the experiment, after removing repeated tasks. The categorization is obtained using a median split. Error bars show ± 1 standard error. Panel B shows the distribution of actual sampling error in the samples taken for a small probability of $p = 0.1$ versus a large probability of $p = 0.9$. Dashed vertical lines show the true underlying probabilities generating the samples.

Endogenous sampling choices drive sampling error. Panel B of Figure 5 shows the probabilities implied by samples drawn for a small probability of $p = 0.1$ and for a large probability of $p = 0.9$ (findings for other probabilities are similar, and are shown in Online

[21] As we will explain shortly, we expect behavior in DfD to also be affected by sampling variance, so that this measure is only a *proxy* for risk aversion as captured by the prior. The results we report are, however, robust to using the structurally estimated prior mean instead.

Appendix E). The figure shows the direct consequence of smaller samples taken for $p = 0.1$ on average by a risk averse subject population: the error of underestimating a probability of $p = 0.1$ is clearly more frequent than the error of over-estimating $p = 0.9$. Across all small probability lotteries, our subjects gather samples that indicate a smaller probability than the true one in 66% of cases overall, and an accurate sample in 3.4% of cases. For large probability lotteries this is reversed, with 55% of samples indicating over-estimates of the true probability, and only 2.2% resulting in a correct estimate. Sampling error is clearly more severe for small probabilities than for large probabilities — a direct consequence of the smaller number of samples taken for small probabilities on average.

Individual-level analysis. So far we have only shown *aggregate* patterns. An important conclusion from our simulations was, however, that behavior will depend on individual-level prior expectations (and the risk aversion they produce), as well as on the idiosyncracies of the samples the DM draws — including both the proportion of winning outcomes, and the sequence in which these proportions are drawn. Here, we use regression analysis to show that the results we document above are robust (i) to using continuous measures of risk aversion; (ii) to conducting this analysis at the individual level; and (iii) to using structurally estimated measures of prior expectations instead of the proxy constituted by proportion of choices of the sure amount in DfD. All of these effects are fully in line with the model predictions delineated above, and thus support our account of endogenous sampling, and how it leads to sampling error.

Table 1 shows regressions detailing individual-level patterns over which our model makes predictions. Regression (1) shows that the number of samples taken increase in the probability of winning ("probability") across all subjects. Regression (2) uses proportions of risk averse choices in the initial DfD phase ("risk aversion") to show that the larger overall samples are mainly driven by the number of samples taken strongly increasing in pre-existing risk aversion ("prob $\times$ risk av."). Regression (3) further probes the robustness of these results by instead using the theoretically correct measure of pessimism in the prior expectation, $ln\left(\frac{1-p_0}{p_0}\right)$, which we obtain from structural estimations of equation (4) from the first phase DfD data.[22] All of the results remain stable.[23]

[22]Note that to be applicable to the DfD data, the true underlying log-odds $ln\left(\frac{p}{1-p}\right)$ have to be substituted for $ln\left(\frac{\alpha}{\beta}\right)$ in that equation. Section A in the Online Appendix discusses the theoretical rationale for this substitution, and section E provides the details about the econometric estimation.

[23]The pure effect of probability switches from being positive and significant to being negative and significant. The reason for this is that we use the inverse prior log-odds on their natural scale, so that 0 coincides with risk neutrality. The simple effect of probability in reg. (3) thus reflects the sampling behavior of a risk

dep. var:	**sqrt. number of samples**			**sampling error**		
	reg. (1)	reg. (2)	reg. (3)	reg. (4)	reg. (5)	reg. (6)
probability	**0.044**	**0.049**	**-0.061**	**-0.034**	**-0.032**	0.017
	(0.009)	(0.009)	(0.015)	(0.010)	(0.010)	(0.018)
risk aversion		0.182	0.077		0.021	0.010
		(0.113)	(0.050)		(0.021)	(0.010)
prob × risk av.		**0.073**	**0.036**		**-0.029**	**-0.016**
		(0.009)	(0.004)		(0.010)	(0.005)
constant	**2.353**	**2.361**	**2.129**	**0.384**	**0.386**	**0.356**
	(0.111)	(0.112)	(0.186)	(0.022)	(0.021)	(0.036)
observations	2178	2178	2178	2178	2178	2178
subjects (clusters)	99	99	99	99	99	99

Table 1: Regression analysis of samples taken and sampling error

Regressions in the table are based on a Bayesian outlier-robust regression model. Robust regression is implemented by means of a student-t distribution with 2 degrees of freedom, with random intercepts to cluster errors at the subject level. Regressions (1), (2), and (3) use the square root of the total number of samples as dependent variable. Regressions (4), (5), and (6) use the z-score of the sampling error, defined as the true probability minus the inferred probability for small probability lotteries, and as the inferred probability minus the true probability for large probability lotteries, as dependent variable. Numbers in parentheses indicate standard errors, and significant effects at the conventional 5% level are highlighted using boldface. Risk aversion is captured by the proportion of risk averse choice in phase 1 DfD in columns (1), (2), and (4), and by the inverse log-odds prior $ln\left(\frac{1-p_0}{p_0}\right)$ in regressions (3), (5) and (6). Probability and risk aversion are normalized by taking z-scores.

Regressions (4) through (6) present regressions of the sampling error (coded in the direction of underestimating the small likelihood event) on the same independent variables as used in regressions (1) through (3). Regression (4) shows that sampling error decreases in the probability of winning on average. Regression (5) shows that sampling error for large probabilities decreases (and sampling error for small probabilities *increases*) in the level of pre-existing risk aversion. Finally, regression (6) shows that the key interaction effect is stable to using the correct prior expectation from regression (3) instead.[24] Taken together, these results show that 1) the number of samples taken increases in the level of pre-existing risk aversion; 2) risk aversion particularly increases samples for large probabilities, while it lowers them for small probabilities; and 3) the same individual characteristics also determine the extent of sampling error. These patterns strongly match those predicted by our noisy cognition model, outlined in Section 2.

Number of samples, sampling error, and risk-taking. To complete the picture of

neutral individual, rather than of the median individual as in reg. (2).

[24] Just like in reg. (3), the large change in the simple effect of probability is due to the different normalization of risk aversion when passing from reg. (4) to reg. (5).

dep. var:	**choice of lottery over sure amount**			
	reg. (1)	reg. (2)	reg. (3)	reg. (4)
probability	**0.349**	0.064	**0.265**	0.108
	(0.032)	(0.095)	(0.055)	(0.058)
risk aversion		**-0.256**	**-0.361**	**-0.377**
		(0.110)	(0.052)	(0.049)
prob × risk av.		**0.119**	**0.175**	0.070
		(0.016)	(0.064)	(0.065)
sqrt nr. of samples			**0.858**	**1.544**
			(0.052)	(0.083)
samp. error for lottery			**0.116**	**0.151**
			(0.018)	(0.019)
samp. error × sqrt nr. of samples				**0.735**
				(0.057)
constant	**-0.596**	0.110	0.204	-0.089
	(0.087)	(0.119)	(0.181)	(0.166)
observations	2178	2178	2178	2178
subjects (clusters)	99	99	99	99

Table 2: Regression analysis of risk-taking

The table shows Bayesian Probit regressions of risk-taking on a number of independent variables. Errors are clustered at the subject level using random intercepts. Probability, risk aversion, sampling error, and the square root of samples are normalized by taking z-scores. The sampling error is defined in the direction of favoring the lottery. Standard errors are shown in parentheses. Effects significant at conventional 5% levels are highlighted in boldface.

what drives behavior in DfE, we next look at risk-taking choices. Table 2 reports a series of Probit regressions to investigate drivers of risk-taking (choosing the risky lottery) at the individual level. Regression (1) shows a regression on probability only, which has the expected positive effect on risk-taking (i.e., the positive probability dependence characteristic of DfE). Regression (2) shows the reduced form regressions using the same characteristics used above to predict sampling behavior and sampling error (the estimated inverse prior log-odds, and their interaction with the probability). The positive effect of the probability of winning on risk taking is now taken up by the interaction between probability and prior log odds, that is, risk-taking increases in the probability of winning for DMs *with the highest pre-existing degree of risk aversion* ("prob × risk av."). This is precisely what is predicted by the mechanism described by our model.

Regression (3) further adds the square root of the number of samples taken ("sqrt. nr. of samples") and the sampling error (now coded as the error in samples in favor of the lottery) ("samp. error for lottery"). Both are highly significant predictors of the level of risk-taking. Importantly, the number of samples — which proxies for the confidence the DM has in the sampled evidence — has a strong positive effect on risk taking on top of the effect of sampling error. Regression (4) further adds the interaction between sampling error and the number of samples taken. Risk-taking strongly increases in this interaction. At the same time, the pure effect of probability, as well as the interaction between a pessimistic prior expectation and the probability, lose their significance. This confirms the interactive role of sampling error and confidence in those samples (proxied by the square root of the number of samples) predicted by our model: sampling error in favor of the lottery will be most influential in determining decisions when the DM has high confidence in the sampled proportion.

Summary. Taken together, the regressions above strongly support the mechanism that our model predicts drives positive probability-dependence of risk-taking in DfE. Regressions (1) through (3) in table 1 illustrate (i) that the number of samples taken increases in ex ante risk aversion (i.e., pessimistic priors) and (ii) that this happens with particular strength in high probability lotteries (where early samples tend to contradict these pessimistic priors). Regressions (4) through (6) in that same table illustrate the way these sampling patterns lead to reduction in sampling error, particularly for risk averse subjects in high probability lotteries. Finally, regressions (1) through (4) in table 2 show that pre-existing risk aversion interacting with the probability of winning positively predicts risky choice (regression 2) — an effect that is mostly explained by the number of samples this induces the DM to take, the sampling error this produces, and their interaction (regression 4). Together, these results strongly match the predicted drivers of positive probability-dependence suggested by our model as discussed in Section 2 above.

3.3 Experiment 2: DfE+forced Treatment

In Experiment 2, we attempt to eliminate the GAP by forcing DfE subjects (again, in Stage 2) to sample from each lottery (i) using a representative sample and (ii) via a relatively large number of draws. The balanced nature of the samples is meant to eliminate sampling error. The large number of draws is meant to increase sampling precision, thereby inducing subjects to rely on the sampled information more than they rely on their prior expectation.[25]

[25] Importantly, since we force sampling for every task individually, we expect forced sampling to reduce sampling variance for every single task. Because of this, we believe our data cannot be cleanly used to assess the way the brain allocates cognitive resources (and therefore precision) *across tasks and probabilities* as discussed and documented, e.g., by Frydman & Jin (2025). In an important sense, we believe our task-by-task noise intervention censors information on this reallocation in our design.

Figure 6: Screenshot from the DfE+forced treatment (Experiment 2).

We do this using the DfE+forced treatment, pictured in Figure 6. This treatment is identical to DfE except that subjects are required to sample all twenty "coins" from each bag (lottery) *without replacement* before making a choice between lotteries. Below each button, the subject is shown how many times she has sampled from each bag and the total number of draws she must make in total (set to 20 in this treatment). The radio buttons for submitting the final lottery choice do not appear on the subject's screen until she has sampled all 20 coins from each bag. In terms of the model, requiring the subject to exhaustively sample a frequentist representation of each lottery means that subjects observe samples α, β for each lottery such that $\frac{\alpha}{\alpha+\beta} = p$, removing scope for sampling error. By setting the number of elements in the frequentist representation to 20, we force subjects to sample far more times than they are observed to do in the DfE treatment, thereby increasing the precision ν^{-2}. In all other respects the experiment is identical to the DfE treatment.

Forced sampling eliminates probability-dependence in DfE. Figure 7 plots choice behavior from DfE+forced, and reproduces behavior from DfE for comparison. As predicted, forced sampling produces a dramatic effect on behavior, particularly in reducing the high levels of risk taking observed for large probabilities. Importantly, as predicted, DfE+forced does this largely by eliminating probability-dependence.

To analyze this more systematically, we calculate choice proportions and their standard errors for each of the 18 tasks. We then aggregate choice proportions across tasks weighing them by the inverse of their squared standard errors, as done in meta-analysis or measurement error models. Regressing the choice proportions of the lottery on the probability of winning provides a direct test of probability-dependence in the choice proportions (see Online Appendix C for details). In DfE, this produces a coefficient on p of 0.284, with a credible interval of [0.226 , 0.347], showing how risk-taking systematically increases in the probability of winning. Regressing choice proportions observed after forced sampling on the probability of winning the prize, we find a slope of 0.080, with a CrI of [−0.020 , 0.182]. This slope is significantly smaller than in DfE. The slope is also not significantly different from 0 at conventional levels — positive probability-dependence of risk-taking in DfE is no

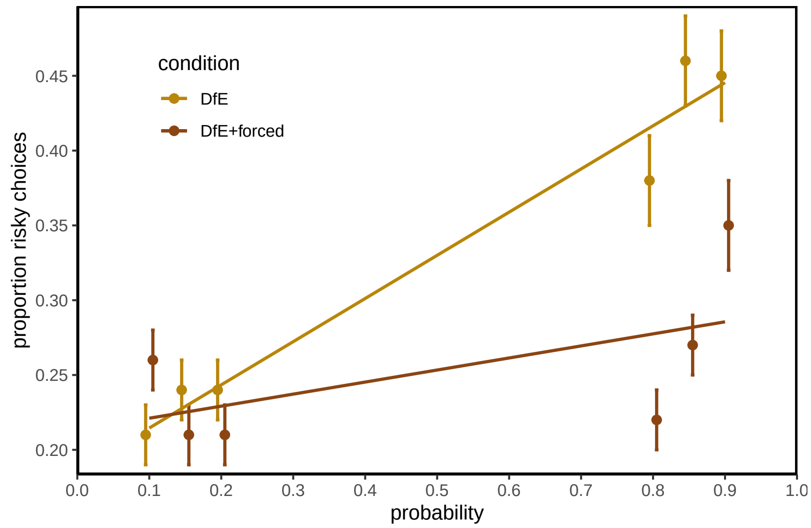


Figure 7: Effects of forced sampling in DfE on choice proportions

The figure shows the effects of forced complete sampling in DfE from both options. The error bars indicate ± 1 standard error.

longer present following forced sampling.[26] Notwithstanding these significant changes on the DfE side, however, the GAP does not close: although it has narrowed to 8.4 pp, the GAP remains substantial as well as statistically significant, with a CrI of [0.053 , 0.115].

Our results are consistent with previous findings on forced sampling in DfE. Pioneering the use of forced sampling in DfE, Ungemach et al. (2009) found the GAP to narrow, but not close (see also Cubitt et al. 2022). Our conclusions are fully consistent with this finding, but strengthen it further. In particular, our richer test stimuli allowed us to test probability-dependence after forced sampling directly, and to show that it disappears — something that previous experiments (e.g., Ungemach et al. (2009)) could not do due to the smaller number of task and absence of variation in the EVs of the choice options conditional on a given probability.

[26] One reason why there is still a slight positive tendency in risk taking could be memory effects. While the previous literature using similar settings to our own has not found much of a role for memory (Ungemach et al. 2009, Cubitt et al. 2022), we do find that the more recent half of samples has a slightly stronger effect on risk-taking than the first half. This is consistent with effects of memory documented by Bohren et al. (2024), albeit in a somewhat different setting. It is also consistent with the structural estimations we show in Online Appendix E.

4 Closing the Gap: Forced sampling in DfD

Forced sampling leads to dramatic changes to DfE behavior, but it does not close the GAP. In particular, although forced sampling removes positive probability dependence in DfE, standard DfD continues to exhibit *negative* probability dependence, causing a GAP to persist. Our hypothesis is that this probability dependence occurs in DfD for reasons similar to those driving probability dependence in DfE. Specifically, we hypothesize that just as limited samples produce imprecision in probabilities under DfE, limited cognitive resources devoted to representing probabilities in the brain in DfD produces a similar imprecision in the perception of probabilities. While forced sampling reduces this imprecision (and with it probability dependence) in DfE+forced, imprecision remains high in DfD causing probability dependence to persist. Because of this, in order to fully close the GAP, we have to increase the precision in probability perceptions in DfD in a manner symmetric to the way we did in DfE.

We start from the observation that probabilities will have to be neurally represented by spikes or action potentials in the brain before entering the decision processes. Neural resources allocated to such encoding are necessarily finite, which will result in inevitable noise (Heng et al. 2020). This will result in a noisy signal r for the described log-odds, as modeled by Khaw et al. (2025) and Vieider (2024*b*). Here, we hypothesize that this noisy signal can be conceived of as a ratio of "neurally sampled" evidence in favor of the lottery and against it, and summarized by a quantity $ln(\widehat{\alpha}/\widehat{\beta})$ similar to the ratio of literal samples used to characterize DfE above.

Just as in DfE, DMs will respond to this sampling imprecision by regressing their posterior beliefs towards their prior beliefs in a Bayesian fashion. However, unlike DfE, where samples were external, we assume noisy internal representations of $ln(\widehat{\alpha}/\widehat{\beta})$ are unbiased: on average (because they are based on accurately-described probabilities) they reflect the true underlying log-odds, $ln\left(\frac{p}{1-p}\right)$. Because representations of $\widehat{\alpha}$ and $\widehat{\beta}$ are based on limited "neural samples" the Bayesian evidence weight $\widehat{\gamma}_n$ will be smaller than one. As a result, applying the Bayesian inference framework from equation (3) to DfD will trigger regression to the mean of the prior (see A.2 for details). On average, this will result in an overestimation of log-odds that are small relative to the mean of the prior (odds smaller than one will be uplifted by the power $\widehat{\gamma}_n < 1$), and an underestimation of log-odds that are large relative to that mean (odds larger than one will be compressed by the power $\widehat{\gamma}_n < 1$).[27] This results in

[27] In principle, it is possible that the number of draws underlying $\widehat{\gamma}_n$ varies across different probabilities. We abstract from this mechanism here, since it is not central for the point we are making.

the standard pattern of risk-taking observed in the DfD literature and the *opposite* of what the model predicts for DfE: negative probability-dependence, with risk-taking decreasing in p.[28] Here, however, this pattern is a direct outgrowth of noise in internal log-odds representations: as $\widehat{\alpha}$ and $\widehat{\beta}$ increase, $\widehat{\gamma}_n$ tends to 1, thus eliminating probability-dependence in DfD. In other words, probability-dependence of risk-taking is predicted by the model to disappear as noise in internal representations of log-odds is reduced to zero.

Closing the Gap. This explanation suggests a distinctive test for the hypothesis that imprecision in neural representations is responsible for classic probability-dependence in DfD. If it is true that probability-dependence in DfD is driven by the noisy perception of described probabilities as hypothesized above, then providing additional information in the form of forced, balanced samples ought to remove this probability-dependence, just as it did in DfE.[29] This makes for a powerful test because 1) from the point of view of standard models such as prospect theory, the information gleaned from samples is *fully redundant* in the DfD treatment; and 2) combining sampling information with described information ought to allow us to increase the precision of the neural log-odds representations, thus reducing or even eliminating probability-dependence in DfD. This provides a particularly crisp test of noisy coding accounts of probability-dependence, since we can directly act on the supposed causes of the phenomenon to try and remove it. It also gives us a predicted recipe for closing the GAP.

4.1 Experiment 3: the DfD+Forced Sampling Treatment

In Experiment 3, we attempt to eliminate the GAP by forcing DfD subjects to redundantly sample large, representative samples from each lottery. In DfD+forced we show subjects the same information about lotteries as we do in the DfD treatment (pictured in Figure 4), but we also provide subjects the sampling tools pictured in Figure 6 below the explicit description, and force subjects to draw 20 times from each just as in DfE+forced (as in

[28] As discussed in the introduction, there is now a well-developed literature suggesting that cognitive noise may be responsible for negative probability-dependence in DfD environments (Zhang & Maloney 2012, Steiner & Stewart 2016, Zhang et al. 2020, Enke & Graeber 2023, Herold & Netzer 2023, Netzer et al. 2025, Frydman & Jin 2025, Khaw et al. 2025, Vieider 2024*b*).

[29] Of course, there are additional forces that could limit the potential for additional information to reduce imprecision of representations. For instance, it is possible that the costs of absorbing information from external samples are no lower than the costs of generating internal samples, in which case this kind of intervention might have limited effect. Similarly, there may be hard constraints on the precision of internal representations that limit the absorption of information from external samples. Because of this, the test implied by this intervention is asymmetric: while evidence that additional information reduces probability-dependence would constitute strong evidence in favor of noisy coding explanations, evidence that it has no effect is ultimately inconclusive.

DfE+forced, this occurs in Stage 2). Indeed, the DfD+forced treatment is identical to the DfE+forced treatment, except that lotteries are fully described to the subject prior to, during and after sampling. To our knowledge, this treatment has not been conducted before in the literature.

Forced sampling eliminates probability-dependence from DfD. Panel A of Figure 8 shows the effect of forced sampling in DfD, by plotting average choice proportions for DfD+forced and (for comparison) DfD. As predicted, we find that forced sampling has *exactly the reverse effect* on DfD as on DfE. At small probabilities, we find a sizeable *decrease* in risk taking at most probabilities.[30] For large probabilities, on the other hand, risk taking *increases* with forced sampling in DfD. This is precisely what we would expect based on the model predictions: given the representativeness of the samples, internal log-odds representations continue to be accurate. At the same time, the forced samples provide additional information, thus lowering the representational noise of the log-odds and increasing the Bayesian evidence weight $\widehat{\gamma}_n$. This strongly reduces regression to the mean of the prior, thereby eliminating probability-dependence in risk-taking. While probability-dependence in DfD is -0.172, CrI $[-0.239\,,\,-0.102]$, it slightly reverses upon forced sampling, without however reaching statistical significance (slope 0.058, CrI $[-0.039\,,\,0.157]$).

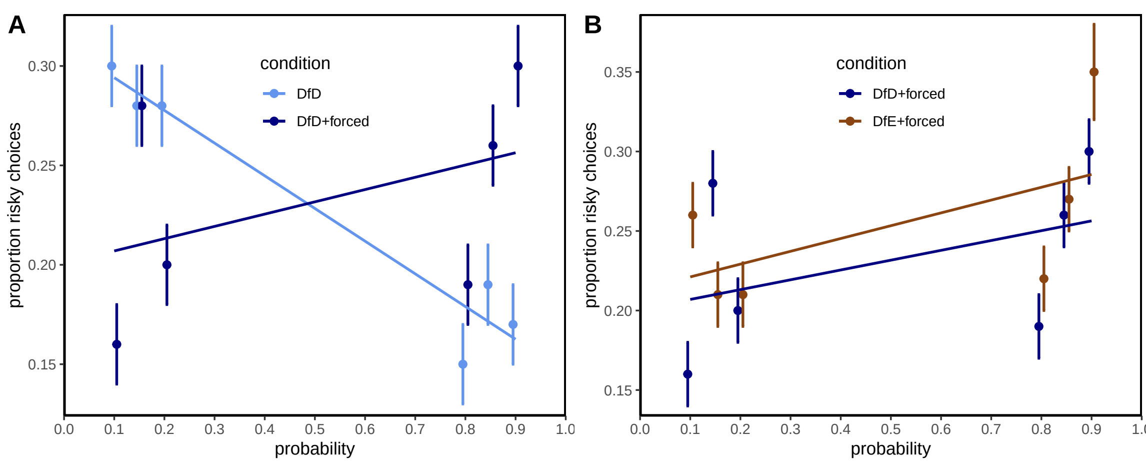


Figure 8: Effects of forced sampling on choice proportions

The figure shows the effects of forced complete sampling in description-based choice. Panel A shows the effect of forced sampling in DfD+forced on choice proportions for different probabilities, and compares it to DfD. Panel B directly juxtaposes choice proportions in DfE+forced and DfD+froced. The lines are fitted by linear regression to the average choice proportions by probability. The error bars indicate ± 1 standard error.

[30] The exception is $p = 0.15$. This is, however, in part caused by the aggregation across different values of sure payments, c. For this particular probability, the changes across different sure amounts go in opposite directions canceling each other out — see Online Appendix E for the plot broken down by values of c.

Closing the GAP. What does the elimination of sampling error and the increase in precision via forced sampling (in both DfE and DfD) do to the GAP? Panel B of figure 8 shows that the choice proportions are now very similar. Probability-dependence in DfD+forced, at 0.058 (CrI $[-0.039\,,\,0.157]$), and in DfE+forced, at 0.080 (CrI $[-0.020\ ,\ 0.182]$), are not significantly different from each other. Nor does risk-taking in either treatment show any sign of significant probability-dependence: the opposite patterns in DfD and in DfE disappear upon forced sampling. Instead, we find mild risk aversion as in standard EUT in both treatments: choice proportions of the lottery hover around 25% for both small and large probabilities, and are not significantly associated with the probability level.

Additional insights emerge from our structural estimations, detailed in Online Appendix E. Compared to decisions in DfD, subjects become much more similar to one another with forced sampling, suggesting that at least part of the heterogeneity in behavior observed in DfD was due to differences in the noise in internal representations of odds. Indeed, it is precisely the subjects who had the noisiest internal representation of log-odds in the first, DfD phase, who are most impacted by the forced sampling intervention. As a consequence, the absence of probability-dependence becomes the rule in the data. In addition, decision noise is reduced strongly upon forced sampling — a secondary model prediction emerging due to the interlaced nature of probability-dependence and stochastic choice predictions.

To provide a more nuanced picture, and to examine the GAP directly, figure 9 shows differences in choice proportions between DfD and DfE for all 18 tasks. Panel A shows the original GAP between DfD and DfE. We use a measure g capturing the difference in choice proportions, defined so that positive values correspond to behavior typically documented in the literature for the standard GAP — more risk-taking in DfD than DfE for small probabilities, more risk taking in DfE than DfD for large probabilities. We then meta-analytically aggregate the GAP across tasks, which yields an estimate of the overall GAP while correcting for random sampling variation in single tasks (details in Online Appendix C). In the *absence* of forced sampling, the GAP is significant in 12 out of 18 tasks when looking at the raw choice proportions, and in 13 out of 18 tasks in the meta-analytic posterior.[31] At 15.7 percentage points (*pp*), with a 95% credible interval of [9.7 , 21.8] pp, the GAP is significant and large measured against the meta-analytic average reported by Wulff et al. (2018), which comes to book at 9.7 pp.

Panel B compares description-based and experience-based choice proportions after forced

[31] The exceptions in which the GAP is not statistically significant at conventional levels are small probability tasks with $c \geq px$.

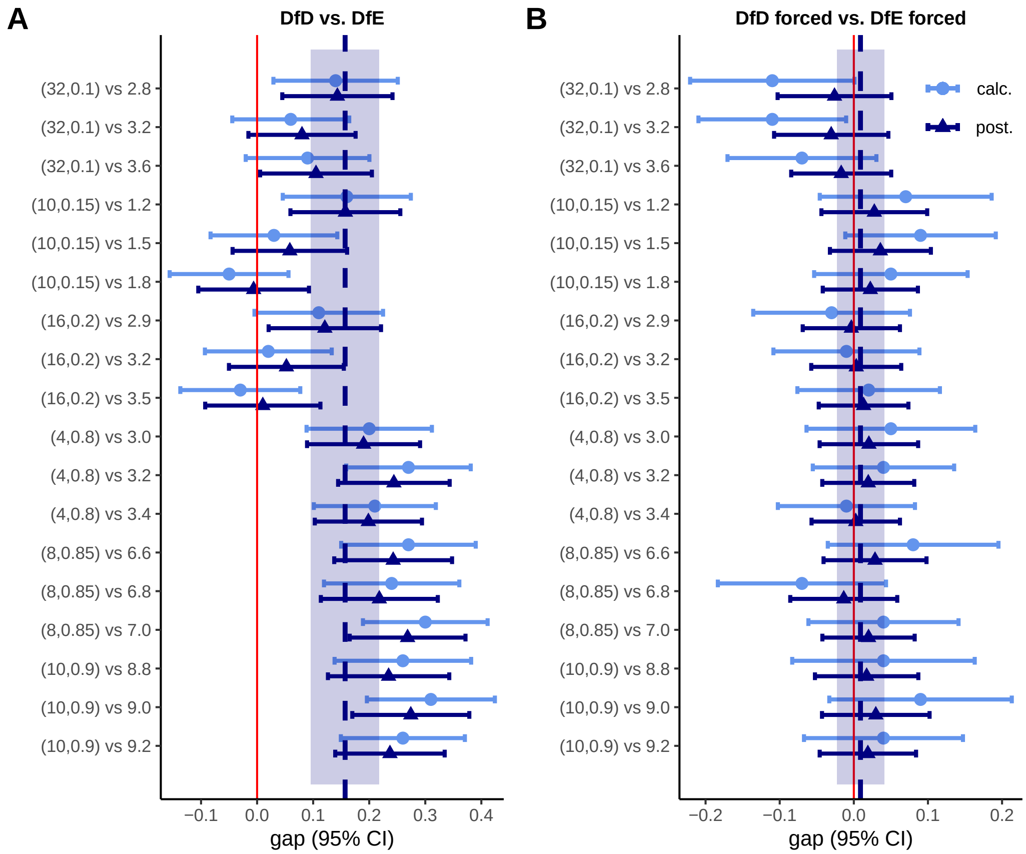


Figure 9: Meta-analysis of the GAP

Panel A shows a forest plot of the gap for our standard implementation of DfD versus DfE. Panel B shows a forest plot of the GAP after forced sampling both from description and from experience. The light blue circles, labeled 'calc.', indicate the raw differences in choice proportions in the data, g. The dark blue triangles, labeled 'post.', indicate the inferred posterior parameters, $\widehat{g}$. The thick, dashed vertical line indicates the meta-analytic posterior mean, ω, and the shaded rectangle indicates the 95% credible interval around that estimate.

sampling (DfE+forced vs. DfD+forced), and shows that the GAP disappears in these treatments. We find no significant gap for *any* of the 18 choice proportions in the meta-analytic posterior. In the one case in which we see a significant gap in the raw choice proportions, this gap goes in the *opposite* direction of the standard GAP. At 0.9 pp (95% credible interval of $[-2.3\,,\,4.1]$ pp), the meta-analytic posterior mean is arbitrarily close to 0. The GAP has therefore closed. Our results thus provide strong evidence that the decision-experience GAP is a consequence of the two elements we expect forced sampling to remove — the elimination of sampling error in DfE, and the reduction in regression to the prior mean in both DfE and DfD.

5 Free Sampling from Described Choice Options

Evidence from the DfD+forced treatment in the previous section suggests that representations of lotteries in DfD are indeed noisy, indicating some natural limit to the precision with which probabilities can be mentally represented. This raises two intriguing questions: First, will this noisiness cause subjects to *choose* to sample even when given fully described options, and even when they are not forced to do so? Second, if subjects do indeed sample, will the sampling error flip the probability-dependence in risk-taking to qualitatively resemble the pattern observed in DfE? Assuming that subjects combine the sampled information — which will inevitably be affected by error — with the unbiased information in the objective DfD description, we may indeed expect probability-dependence of risk-taking to change from negative to positive. This in turn means that voluntary sampling in DfD may close the GAP in a second, distinct way.

In the DfD+free treatment, we show subjects the same information about lotteries as we do in the DfD treatment, but we also provide subjects the sampling tools just like in the DfD+forced treatment. Unlike in DfD+forced, however, the radio buttons to indicate a choice appear from the very start and subjects are free to choose without drawing any samples. Subjects are told explicitly that they can sample if they want to but that they do not have to, and that they can also indicate their decision directly without sampling. Also unlike DfD+forced, in DfD+free (like in the DfE treatment) samples are iid. We ran this treatment on Prolific with 101 subjects using otherwise identical tasks and procedures as in DfD.

We do indeed observe that subjects voluntarily sample when given the chance to do so, suggesting at least partial awareness of coding noise among subjects under DfD. Across subjects and tasks, the average number of samples is 1.74, of which 1.4 are taken from the risky option. Only 8 out of 100 subjects never sample at all, but most subjects take relatively few samples. Samples are highest at the beginning, with 4.7 samples being taken on average across all subjects in the first round. This declines rapidly to some 2.9 samples on average in the second round, and to 2.4 in the third. After round 8, the average settles to a steady level of 1.3 samples per task and subject. The fact that DMs do sample fully redundant information seems remarkable in our context, given the high opportunity costs of subjects on Prolific.

We next examine what happens to choice behavior once free sampling is introduced. Our model raises an intriguing possibility: if, as we hypothesize, subjects' perceptions of DfD

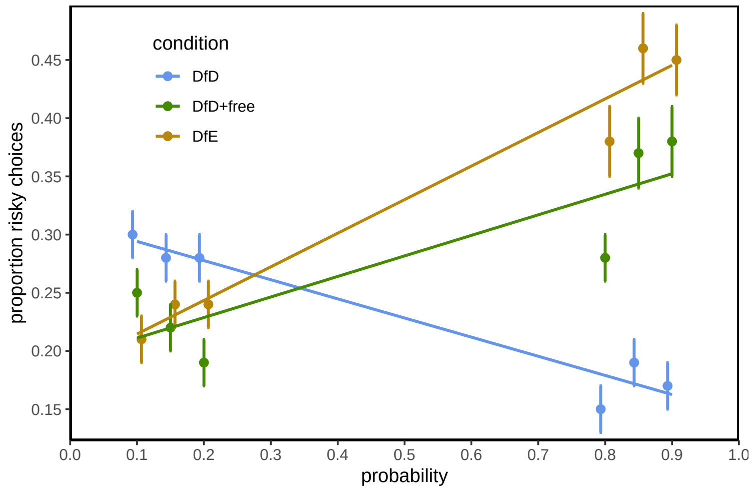


Figure 10: Structural estimates, DfD versus DfE

probabilities are noisy, free sampling in DfD may introduce sampling error like that observed in DfE. That is, although subjects are given an objective description of the probabilities, our model suggests that actual samples drawn are combined with the unbiased neural samples representing the evidence in favor and against the lottery. If subjects draw small samples, however, they will suffer from the same issues we have seen in DfE: they will tend to underestimate the likelihood of observing the rare event, so that our model predicts that they will suffer biased updates of the true log-odds.

Figure 10 shows the raw choice proportions in DfD+free, and directly juxtaposes them with the choice proportions in DfD and in DfE. The difference from DfD is very large, with somewhat less risk-taking for small probabilities, and much more risk-taking for large probabilities. This results in a positive dependence of choice proportions on the probability of winning, with a slope of 0.174, and a 95% credible interval of [0.141 , 0.241]. This suggests that sampling error indeed affects DfD+free choices, just as our model predicts. The effect is in fact strong enough to considerably narrow the GAP in the opposite direction when examining it meta-analytically: at 3.5 pp, the average GAP is now small, and (just) not statistically significant, with a 95% CrI of [-0.001 , 0.072].

The picture is more nuanced when looking at probability-dependence directly. Although we now see positive probability-dependence of risk-taking in DfD+free, the influence of the description is strong enough to keep the probability-dependence significantly smaller than observed under DfE (where we have observed a slope 0.284, with a credible interval of

[0.226 , 0.347]). This shows that samples from description — while affecting probabilities in the way predicted by our model — are still balanced against the description provided on the screen, with choices suggesting an aggregation of the two types of information.

Our findings are consistent with studies that have investigated the effect of providing iid feedback after payoff-relevant choices under the form of single draws from the chosen option. Van de Kuilen (2009) studied the effect of feedback provision after risky choices in a prospect theory framework. He concluded that providing feedback shifted behavior towards linearity in probability weighting, but could not fully test this proposition due to exclusive focus on probabilities ≥ 0.5 (see also van de Kuilen & Wakker 2006). Jessup et al. (2008) and Tymula et al. (2023), who use both monkeys and humans as subjects, provided feedback after choices for a large number of trials. While none of these studies focuses on the GAP, they show that classic probability-dependence in DfD reverses upon the provision of feedback, resulting in the type of positive probability-dependence observed in DfE. Our model sheds new light on these findings: unless independently and identically distributed samples are extremely large, they will introduce sampling error into DfD. The reason this happens, our work suggests, lies in the imprecision in the mental representations of probability that are explicitly described: the residual uncertainty in their mental representations provides opportunities for additional information to affect those representations.

To conclude, we have found that the GAP can be closed (or severely reduced) in two distinct ways, both relying on the fact that people only noisily understand probabilities in standard DfD. In the previous section we showed that forcing subjects to draw large, balanced sample in both DfE and DfD causes behavior in the two settings to converge. In this section we show that we can also close the GAP via an intervention on DfD alone. Guided by our model, we introduced sampling error into DfD, while keeping precision relatively low due to the few samples added. Simply allowing subjects in DfD to sample dramatically narrows the GAP, with DfD+free approaching the type of positive probability-dependence characteristic of standard DfE.

6 Discussion

In this paper we provide evidence suggesting that probability-dependent risk-taking and the description-experience gap — two key phenomena in the lottery choice literature — are a joint consequence of the incomplete and imprecise ways decision makers perceive and represent information. Reducing the imprecision of subjects' beliefs by forcing them to observe redundant information causes probability-dependence in risk-taking to disappear and closes the description-experience gap. In addition to shedding significant light on a key mystery in the literature, we believe there are several broader implications of our findings.

First, we believe our results demonstrate the potential scope of the noisy cognition approach by extending it from description-based choice to experience-based choice. Noisy cognition thereby organizes a key paradox under existing descriptive models of choice — the description experience gap — providing additional evidence of the value of the approach. Our sampling-based characterization allows us to present a distinctive (and arguably particularly direct) test of the role noisy cognition plays in standard probability-dependence when choice options are fully described: by forcing subjects to take large, balanced samples from both choice options, we manage to completely eliminate probability-dependence in risky choice. We believe this treatment effect is difficult to account for via alternative explanations that are not similarly rooted in cognitive imprecision. In particular it is difficult to reconcile with models that attribute probability-dependence to rationally-expressed, non-standard preferences.

Second, and because of this, our results may provide some clues about the nature of "true" (i.e., welfare relevant) risk preferences and the methods we use to measure them. Under the lens of noisy coding models, many lottery anomalies are a consequence, not of non-standard preferences, but rather of what we have called imprecision in mental representations, driven by limitations in the way the brain encodes information. We provide some evidence that these imprecisions can be removed or reduced by forcing subjects to intensively sample lotteries, leading (at least according to noisy cognition models) to behavior that better represents true preferences. Because of this, the use of such techniques may be helpful in accessing the sorts of preferences that are of most value to policy makers interested in developing welfare-maximizing policies. In our own data, we find that when we strongly reduce imprecisions in mental representations via forced sampling, behavior reveals mild, probability-independent risk aversion that is broadly consistent with standard, neoclassical predictions. It may be valuable to more intensively and systematically use methods like these to assess the nature of risk and other preferences in future work.

Finally, our investigation is aimed at better understanding important behavioral patterns that have been primarily documented in laboratory experiments. Nonetheless, we think there are two important real-world implications of these findings that might turn out to be valuable. First, the DfE literature suggests we should expect negative probability dependence (à la classical probability weighting) to be a special case that arises primarily in decisions in which people learn about risks purely by description – indeed the reverse can be expected to occur when risks are learned instead by endogenous, exploratory learning. Our results suggest that *whether or not* decision makers have descriptive information on risks, sufficient exposure to exogenous realizations of those risks (as occurs in many important field settings) will tend to dampen and even eliminate probability dependence altogether.[32] Second, our finding that probability dependence is an outgrowth of cognitive frictions rather than rationally-expressed preferences suggests we take caution when attempting to infer welfare-relevant preferences from risky decision-making in the field. In many settings, we have reasons to expect these decisions to be distorted in such a way as to obscure decision makers' true tastes for risk. Indeed, these distortions may even be *more* severe in the relatively more complicated decision contexts typical of the field. However, our results also give us some guidance on real-world settings that are most likely to be most revealing of risk preferences. In particular, our results suggest that settings in which decision makers are exogenously exposed to substantial experiential information on the properties of risks (as opposed to described probabilities or endogenously sampled information) are likely to be the ones to yield the sharpest and least biased information on decision makers' welfare-relevant tastes for risk.

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[32] This evidence raises important questions as to what may determine real-world behaviors such as insurance uptake, which have been explained in the past by the overweighting of small probabilities. There is significant recent evidence that probability weighting as measured in experiments poorly accounts for the typical small-stake insurance choices (see e.g. Sydnor 2010). An older literature has also shown that framing identical choices in terms of insurance rather than as abstract decisions increases risk aversion for small probability losses significantly (see e.g. Hershey & Schoemaker 1980). This is highly coherent with our approach, where priors may well be situation-specific. The stylized fact that insurance is often taken up after experiencing losses could explain the prevalence of small-stake insurance (and the concomitant under-insurance against large losses) Slovic et al. 1977, Kunreuther 2006).

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Online Appendices

A Model derivation

A.1 A general inference model

In Decisions from Experience (DfE), subjects need not only learn the outcomes and underlying probabilities, but also the whole structure of the decision problem (i.e., the number of outcomes in the lottery's support). In the body of the paper we assume away this component of the inference problem for simplicity and to focus our discussion on the influence of sampling error and sampling variance. Here, for completeness, we propose a stylized model of how such higher order learning could take place based on the sort of sampling from the two options that occurs in DfE. We argue that expanding the model in this way has little qualitative impact on our findings.

We start by discussing the structural inference process. Assume a DM believes that outcomes will range from 0 to some upper limit u, outcomes beyond which are not considered plausible.[33] Take two objective probability distributions over all outcomes underlying the two choice options, $\{p_0, p_1, ..., p_u\}$ and $\{q_0, q_1, ..., q_u\}$, where subscripts indicate monetary outcomes. In DfE, DMs will infer the probability distributions from the draws they observe. Let the initial likelihood at time $t = 0$, before any draws are taken, be encoded in two $u+1$-dimensional Dirichlet distributions, $\mathcal{D}_A(\pi_j) \propto \prod_{j=0}^{u} \widetilde{p}_j^{\,\pi_j - 1}$ and $\mathcal{D}_B(\omega_j) \propto \prod_{j=0}^{u} \widetilde{q}_j^{\,\omega_j - 1}$, where $\widetilde{p}_i \triangleq \frac{\pi_i}{\sum_j \pi_j}$ and $\widetilde{q}_i \triangleq \frac{\omega_i}{\sum_j \omega_j}$ represent the subjective expectations of the probabilities attributed to an outcome i in the two choice options A and B. Given the ex ante exchangeability of the two choice options, the two Dirichlets will have the same parameters at time $t = 0$. We assume that DMs consider any given outcome as equally likely in the two choice options, so that $\pi_i = \omega_i \ \forall \ i$ at $t = 0$. This assumption directly follows from the exchangeability of the two options before any draws have been observed, and is implemented in our experiment by randomizing the risky and safe options in positions A and B.

We assume that what matters for decisions is the direct comparison between the two choice options. To capture this in our model, we map the inferences based on the Dirichlets encoding draws from the two choice options into a *comparative Dirichlet* which entails a statewise comparison between to two options. That is, what matters for choices are events in which one option pays a given outcome, while the other option pays a different outcome. In our experiment, these will be the events under which the risky option pays x while the

[33] In principle, u can take any value, as long as it is finite. In our experiment, we tell subjects beforehand that all outcomes will range between \$0 and \$ 35 inclusive, thus setting their expectations about this range.

safe option pays $c < x$, and the event under which the risky option pays y while the safe option pays $c > y$ (see below for a generalization). The probabilities of the comparative events e_1 (obtain $x > c$ rather than c) and e_2 (obtain c rather than $y < c$) can now be obtained from the single-state Dirichlets $\mathcal{D}_A(\pi_j)$ and $\mathcal{D}_B(\omega_j)$ defined for the two options, since $P[e_1] = P[x \cap c] = \widetilde{p}_x \times \widetilde{p}_c$ and $P[e_2] = P[y \cap c] = \widetilde{p}_y \times \widetilde{p}_c$. Given that for finite samples $\widetilde{p}_c < 1$ and $\widetilde{p}_x + \widetilde{p}_y < 1$, the inferred probabilities will generally be subadditive, that is, $P[e_1] + P[e_2] \leq 1$ (with 1 being the limiting case as samples tend to infinity). This implies that we can express the subjective beliefs in the comparative states of the world once again by a Dirichlet, $\mathcal{D}(\delta_i) = \prod_{i=1}^{u} P[e_i]^{\lambda\widehat{\delta}_i - 1}$, where $\lambda \triangleq \sum_{i=1}^{u} \delta_i$ is the concentration of the new Dirichlet, and $\widehat{\delta}_i \triangleq \delta_i/\lambda$ captures the mean belief about a given state i. While some probability mass will thus remain attributed to 'non-observed outcomes', this part will drop out of the main choice equation below.

This justifies the assumption of the Beta distribution in the main text: while the latter imposes additivity in $\widehat{p}_x$ and $\widehat{p}_y$, that assumption serves to simplify our discussion, but has no substantive implications for our conclusions (given that the non-observed states receiving the remaining probability mass drop out of the discriminability equation). If, say, a third outcome from the risky option were to be observed at some point, this would add a new comparative state to the comparison (see below). In the text we further discussed inference bias in terms of the samples taken from the risky option only. More generally, however, the samples from the safe option will also count. While a precise closed-form solution does not exist for that case, we can approximate the samples by the total samples for each state, where the samples from the safe option are simply added to the samples indicating each comparative sample in the sum of the trigamma functions. This means that our discussion in the main text may *quantitatively* underestimate the samples, but that this more general case will not qualitatively affect any of the conclusions drawn.

In the main text, we implicitly assume that subjects know which of the two options is the risky one and which the safe. In reality, subjects need to infer this from the samples they take. We make three assumptions in this regard. The first, and most substantively relevant, is that subjects make inferences on the choice environment (including potentially the intentions of the experimenter). This entails that choices between two non-degenerate options are deemed extremely unlikely. Practically, this entails that sampling variance will remain high until a plausible set of outcomes has been observed.[34] The second assumption

[34]This assumption seems particularly defensible in our DfE experiments, since all subjects assigned to this treatment have all finished making dozens of binary DfD choices for lotteries with one degenerate and one

is that we assume the initial parameters of the two choice option Dirichlets to be sparse, i.e. $\pi_i, \omega_i \ll 1 \; \forall \; i$. This assumption implies that subjects do not expect a very diffuse probability distribution with many different outcomes. Practically, this helps explain why samples are relatively small, since it keeps the probability mass assigned to unobserved outcomes low in the comparative Dirichlet.

An additional assumption in the main text is that subjects can infer which of the two options is the risky one. This obtains trivially once a subject has observed all three outcomes used in our experiment (the two in the risky option, and the one in the safe option, which constitute a 'plausible minimal outcome set' inasmuch as they indicate a non-degenerate choice, or equivalently, they map into two comparative states with a meaningful tradeoff between log-odds and log-cost benefits). This indeed follows directly from the two assumptions above: that subjects expect non-degenerate choices, and that the initial parameters are sparse (meaning that they do not necessarily expect more outcomes once they have observed a plausible outcome set). The inference is somewhat less trivial as long as only one outcome has been observed from each choice option.

We illustrate this based on the choice options we provide in the experiment. For small probabilities, subjects are overwhelmingly likely to observe the lower outcome y. Given that in our experiment y is always equal to 0, and that we tell subjects that they will only ever face non-negative amounts, this immediately identifies this choice option as the risky one. For large probabilities, where subjects may observe two strictly positive amounts x and c from the two options, this is less obvious. We thus furthermore assume that the parameters of the option-specific Dirichlets before any samples are taken will be characterized by sparsity increasing in outcomes. That is, for any $j > i$, where the two indices are non-negative outcomes, $\omega_j = \pi_j \leq \pi_i = \omega_j$ at time $t = 0$, before any samples have been taken. In practice, this entails that subjects consider smaller outcomes more likely than larger outcomes. Notice that this is the equivalent of a pessimistic prior for the inference process, and that it is thus fully coherent with both our model and our empirical results.[35]

non-degenerate lottery.

[35]In principle, this inference process could be modelled as a probabilistic process resulting in stochastic assessments of the riskiness of the two choice options after each sample. Such a model would follow a very similar structure as our discriminability model, and we do thus not formalize it here. Such a model would be most relevant for large probability lotteries in cases where only one outcome has been observed from each option. The notion that subjects infer the structure of such choice problems from sampling draws is indeed supported by the observation that samples from the *safe* option increase in the objective probability of winning for both risk averse and risk seeking subjects in our data.

A.2 Noisy log-odds representation

In our actual experiment, subjects will experience exactly 1 outcome from the sure option, and no more than 2 from the risky option. We can thus use the 2-dimensional special case of the comparative Dirichlet distribution discussed above – the Beta distribution (see above for an explicit discussion of this simplifying assumption). In particular, the parameter α will encode the 'good state', in which the lottery pays a prize $x > c$, whereas β will encode the 'bad state', under which the lottery pays an outcome $y < c$. The perceived or sampled probability of the good state favoring the lottery will thus be $\mathbb{E}[\widehat{p}] = \frac{\alpha}{\alpha+\beta}$.

We start from an optimal choice rule entailing expected value maximization. The DM will thus choose the lottery over the sure amount whenever $\widehat{p}x + (1 - \widehat{p})y > c$, or equivalently whenever

$$ln\left(\frac{\widehat{p}}{1-\widehat{p}}\right) > ln\left(\frac{c-y}{x-c}\right).$$

The transformation into log-odd space is convenient for computational reasons, but otherwise inconsequential (see Vieider 2024*b*, for an alternative derivation). The choice rule entails that the log-odds in favor of the lottery will be traded off against the log of the ratio of costs ($c-y$, potentially get the lower outcome y when c could have been had) and benefits ($x-c$; obtain the prize x instead of the lower sure amount c). Here, we will assume without loss of generality that the log cost-benefits are perceived objectively. This is a simplifying assumption that allows us to focus on the likelihood dimension, where most of the action takes place. It is straightforward to generalize the derivation to include the noisy coding of costs and benefits as well (cfr. Vieider 2024*b*).

The mean of the sampled log-odds can simply be derived from the two parameters containing the counts of successes and failures:

$$\mathbb{E}\left[ln\left(\frac{\widehat{p}}{1-\widehat{p}}\right)\right] = ln\left(\frac{\alpha}{\beta}\right)$$

Given limited samples, however, even samples that are accurate on average will contain some error on single draws, driven by natural sampling variation around the true mean. Averaging across all probabilities, we will thus observe

$$ln\left(\frac{\alpha}{\beta}\right) = ln\left(\frac{p}{1-p}\right) + \varepsilon,$$

which, following Atchison & Shen (n.d.), could equivalently be written as the difference of the digamma functions of the two parameters, $\digamma(\alpha) - \digamma(\beta)$.

Log-odds tend to follow approximately normal distributions, giving rise to a *logit-normal* (Atchison & Shen n.d.). This suggests that $\varepsilon \sim \mathcal{N}(0, \nu^2)$. The sampling variance ν^2, in turn, again derives from the properties of the logit-normal distribution, and is given by the sum of trigamma functions of the two parameters, i.e. $\nu^2 = \digamma'(\alpha) + \digamma'(\beta)$.

Optimal Combination with Bayesian prior

Given the noise in inferences, it will be optimal to combine the observations with a Bayesian prior. The optimality of this operation derives from the fact that — even though it will introduce systematic bias into the estimates under the form of regression to the mean of the prior — it will minimize the mean squared error across many estimates (see Ma et al. 2023, chapter 4, for an illustration). The reason for this is that the reduction in variance of the estimator will more than make up for the introduction of bias.

The objective for the mind now becomes to infer the log-odds from the underlying samples (whether they be true samples or virtual/neural samples — we drop the subscripts here and derive the equation just once). The inference problem for any given choice task will thus be as follows:

$$\mathbb{E}\left[ln\left(\frac{p}{1-p}\right) \mid \alpha, \beta\right] = \frac{\sigma^2}{\sigma^2+\nu^2}\, ln\left(\frac{\alpha}{\beta}\right) + \frac{\nu^2}{\sigma^2+\nu^2}\, \mu,$$

where we redefine $\mu = ln\left(\frac{p_0}{1-p_0}\right)$ in the main text, and where the Baysian evidence weight or "likelihood-discriminability" parameter is given by $\gamma \triangleq \frac{\sigma^2}{\sigma^2+\nu^2} = \frac{1}{1+\nu^2/\sigma^2}$. A step-by-step derivation of this equation can be found in Vieider (2024a), chapter 2.

In DfD, the "virtual draws" encoded in α and β (referred to as $\widehat{\alpha}$ and $\widehat{\beta}$ in the main text) are unobservable. We can, however, estimate the equation by aggregating across multiple similar probabilities. This will yield the expectation over repeated stimuli of the posterior expectation above, which takes the following form:

$$\mathbb{E}\left[\mathbb{E}\left[ln\left(\frac{p}{1-p}\right) \mid \frac{\widehat{\alpha}}{\widehat{\beta}}\right] \mid p\right] = \frac{\sigma^2}{\sigma^2+\nu^2}\, ln\left(\frac{p}{1-p}\right) + \frac{\nu^2}{\sigma^2+\nu^2}\, \mu,$$

which now allows us to substitute the true log-odds for the sampled log-odds. Choice to choice fluctuations in the samples will be reflected in the variance of the distribution, which takes the form $\gamma^2\nu^2 = \frac{\sigma^4\nu^2}{(\sigma^2+\nu^2)^2}$.

Proof. The proof exploits the well-known property of the normal distribution whereby $z \sim \mathcal{N}(\widehat{z}, \tau^2)$ implies $bz + a \sim \mathcal{N}(b\widehat{z} + a, b^2\tau^2)$. To obtain the response distribution above, let $ln\left(\frac{\alpha}{\beta}\right) = z$, $\frac{\sigma^2}{\sigma^2+\nu^2} = b$, $\frac{\nu^2}{\sigma^2+\nu^2}\,\mu = b$, $ln\left(\frac{p}{1-p}\right) = \widehat{z}$, and $\nu = \tau$. □

Note that the problem does not change in any substantive way if we abandon the assumption of draws correctly reflecting the underlying distribution on average when real samples are taken in DfE. We then simply change the objective probability p to the sampled probability $\widehat{p}$ in the equations above. Sampling bias in $\widehat{p}$ will then occur on top of the inference bias, which still results in regression to the mean of the prior, just like represented above.

Stochastic choice rule

We can now trade off the inferred log-odds, as derived above, against the log-cost benefits, as suggested by our optimal choice rule. Letting $\mu \triangleq ln\left(\frac{p_0}{1-p_0}\right)$, we obtain $\delta = ln\left(\frac{p_0}{1-p_0}\right)^{1-\gamma}$, and by extension, $\theta = \delta^{-1} = ln(\frac{1-p_0}{p_0})^{1-\gamma}$. Putting everything on the scale of the standard deviation of the response distribution derived in the previous section yields the z-score describing the choice probability of the lottery:

$$pr[(x,p;y) \succ c] = \Phi\left[\frac{\gamma\, ln\left(\frac{p}{1-p}\right) - ln\left(\frac{c-y}{x-c}\right) - ln(\theta)}{\gamma\,\nu}\right],$$

where Φ is the standard normal cumulative distribution function. In DfD (as well as DfD+forced and DfE+forced), the probability will correspond to the correct one, and the model can thus be simply estimated on choice data by plugging the probit link function above into a Bernuoulli distribution (see below).

In DfE, we need to slightly amend the function above. In particular, we will now substitute sampled probabilities $\widehat{p}$ for the true probabilities above (adding a constant to both numerator and denominator to make sure it is defined—see discussion of the inference process above). An additional assumption concerns the log cost-benefit ratio when either x , y, or c have not yet been observed. The simplest assumption is that of a "naive" decision maker, who assumes the ratio to be 1 in that case (and hence its logarithm to be 0). However, this is just a special case of what a more sophisticated decision maker would do. Multiplying the log cost-benefit ratio by an additional parameter ρ, conditional on one of the outcomes not yet having been observed, allows for a more flexible specification whereby the DMs can (correctly) infer a positive correlation between log-odds and log cost-benefits. The "naive" DM discussed above is then just a special case for whom $\rho = 0$.

N-dimensional generalization

The inference framework discussed at the beginning of this section is fully general. While we have described it for the particular case of comparisons used in our experiment, it can just as easily be applied to comparison between multi-outcome lotteries. The inference framework introduced above remains directly applicable, with the two option-specific Dirichlet simply counting instances of different outcomes. Our setup assumes that outcomes are ordered by size to arrive at the comparative distribution. The comparative Dirichlet is then constructed over k comparative states constructed based on the ranked outcomes.

Take two lotteries offering outcomes $\boldsymbol{x} = \{x_1, ..., x_k\}$ and $\boldsymbol{y} = \{y_1, ..., y_k\}$ under the comparative events $e_1, ..., e_k$, where each comparative event is characterized by a probability $\widehat{p}_i$, which could be different from the true underlying probability p_i. We assume that the outcome are ordered such that $x_1 \geq x_2 \geq ... \geq x_k$ and $y_1 \geq y_2 \geq ... \geq y_k$. We further assume for our representation that $\boldsymbol{x}$ is riskier than $\boldsymbol{y}$ in the sense of having wider spread or variance. Draws from the two choice options ought to be seen as independent, just as is the case in the actual samples taken. The optimal choice rule, which once again entails expected value maximization, takes the following form:

$$\sum_{i=1}^{k} \frac{\widehat{p}_i}{1 - \widehat{p}_i}(x_i - y_i) > 1, \tag{5}$$

which sums the relative benefits of the riskier option, $x_i - y_i$.

Assuming that the different states will be processed in parallel, the stochastic choice equation then takes the following form:

$$P[\boldsymbol{x} \succ \boldsymbol{y}] = \sum_{i=1}^{k} \Phi \left[\frac{\gamma \times ln\left(\frac{\widehat{p}_i}{1-\widehat{p}_i]}\right) + \mathbb{1} \times ln\left(\mathbb{1}(x_i - y_i)\right) - ln(\theta)}{(k-1)\,\nu \times \gamma} \right].$$

where $\mathbb{1} = 1$ if $x_i - y_i > 0$ and else $\mathbb{1} = -1$, thus assuring that the logarithm is defined. The multiplication of the "relative benefit" by $\mathbb{1}$ further makes sure that this quantity enters with the appropriate sign, since it could favor either choice option in any given state i. Given that any single comparison is standard-normally distributed, the sum over the different comparisons will also follow a standard normal distribution. While this formulation *could*, in principle, result in predicted choice probabilities greater than 1 or smaller than 0, this is unlikely in practice, given that benefits and costs are usually designed to compensate each other. A regularizing condition could be imposed to overcome this issue should it ever become relevant in practice. We lave careful study of this extension for future work.

B Experiments

Choice stimuli

We selected our choice stimuli from those in the early DfE literature (Hertwig et al. 2004), but generalized them so as to allow us to structurally estimate our model, and to obtain a more balanced picture of the behavior. We assured identification of the structural estimations using simulations, which allowed us to find the optimal compromise between number and type of task and the length of the experiment. The limiting factor derived in particular from the forced sampling experiments, where subjects had to take 40 samples by tasks, as well as expressing their final choice.

We thus chose 6 different lotteries—3 with a small probability, and 3 with a large probability of winning. We then obtained three choice tasks by lottery by setting the sure amount c equal to the expected value, and by adding or subtracting a fixed amount. This provides some valuable variation for the structural estimations, and results in the following 18 unique tasks (4 randomly selected ones of which were repeated in the experiment):

C Meta-analytic estimation

Quantifying the GAP. To get a better idea of the size of the decision-experience GAP in our data, and to relate it to typical findings in the literature, we can aggregate the evidence across tasks using the tools of meta-analysis.[36] Let $\pi_d = {}^{R_d}/_{N_d}$ be the proportion of risky choices in DfD, where R_d is the number of risky choices, and N_d the number of observations.

[36]The meta-analytic tools we use are identical to a "measurement error model". That is, the assumption is that each single choice proportion is observed with some error. Meta-analysis then allows us to aggregate across the choice proportions while eliminating measurement error and thus correcting our analysis for multiple testing across many moderate (and not statistically independent) samples.

Table 3: Choice tasks

small p	large p
(31,0.10) vs. 2.8	(4,0.80) vs. 3.0
(31,0.10) vs. 3.2	(4,0.80) vs. 3.2
(31,0.10) vs. 3.6	(4,0.80) vs. 3.4
(10,0.15) vs. 1.2	(8,0.85) vs. 6.6
(10,0.15) vs. 1.5	(8,0.85) vs. 6.8
(10,0.15) vs. 1.8	(8,0.85) vs. 7.0
(16,0.20) vs. 2.9	(10,0.90) vs. 8.8
(16,0.20) vs. 3.2	(10,0.90) vs. 9.0
(16,0.20) vs. 3.5	(10,0.90) vs. 9.2

Choice tasks are describes as usual, with (x, p) designating a lottery providing a prize x with probability p or else 0, and c designating the sure amount.

Let $\pi_e = {}^{R_e}/_{N_e}$ be the proportion in DfE. We define the difference in choice proportions as g, where we encode the difference in the direction of the standard gap, so that $g = \pi_d - \pi_e$ for $p < 0.5$ and $g = \pi_e - \pi_d$ for $p > 0.5$. This difference will be approximately normally distributed, with variance $\pi(1-\pi)\left({}^{1}/_{N_d} + {}^{1}/_{N_e}\right)$, where $\pi = \frac{\pi_d + \pi_e}{N_d + N_e}$. We can now use g and its associated standard error, se, for meta-analytic aggregation across tasks, indexed by i:

$$g_i \sim \mathcal{N}(\,\widehat{g}_i\,,\, se_i^2\,)$$
$$\widehat{g}_i \sim \mathcal{N}(\,\omega\,,\, \tau^2\,),$$

where g and se are data, $\widehat{g}$ is the unknown true effect, and ω and τ are parameters capturing the meta-analytic mean and standard deviation across tasks, respectively. We then quantify the GAP by meta-analytically aggregating the differences in choice proportions across tasks in a direction that is consistent with the standard GAP.

Reversals in Likelihood Dependence. We can also use meta-analysis to test whether choice proportions exhibit probability-dependence, and whether the nature of this dependence is different in DfE and DfD. To do this, we analyze the choice proportions π_i directly (instead of examining differences in choice proportions g_i) so that we estimate $\pi_i \sim \mathcal{N}(\,\widehat{\pi}_i\,,\, se_i\,)$. We then use meta-regression to assess the dependence of the choice proportions on the probability of winning, by letting $\widehat{\pi}_i \sim \mathcal{N}(\,\lambda_0 + \lambda \times p_i\,,\, \tau^2\,)$, where $\widehat{\pi}_i$ is the unknown true choice proportion.

We estimate the model in Stan (see Vieider 2024a for a tutorial on the use of Stan for decision models; chapter 4 contains a part specifically dedicated to meta-analysis). Here is the Stan code used to estimate the model:

```
//footnotesize
data{
    int<lower=1> N; \\number of observation
    vector[N] gap; \\difference in choice proportions
    vector<lower=0>[N] se; \\standard error of the difference
}
parameters{
    vector[N] gamma; //true, estimated gap (called g_hat in paper)
    real mu; //meta-analytic mean (omega in paper)
    real<lower=0> sigma; //variance
}
model{
//regularizing priors
    sigma ~ normal( 0 , 1 );
    mu ~ normal( 0 , 1 );

    // measurement error model:
    gap ~ normal( gamma , se );

    // likelihood:
    gamma ~ normal( mu , sigma );
}
```

The meta-regression is introduced into the same code simply by modifying the mean *mu*, making it dependent on the probability of winning:

```
//footnotesize
data{
    int<lower=1> N;
    int<lower=1> K; //dimension of design matrix
    vector[N] gap;
    vector<lower=0>[N] se;
    matrix[N,K] X; //design matrix of explanatory variables
}
```

```
parameters{
    vector[N] gamma;
    real mu;
    real<lower=0> sigma;
     vector[K] beta;
}
model{
    sigma ~ normal( 0 , 1 );
   mu ~ normal( 0 , 1 );

    // measurement error model:
    gap ~ normal( gamma , se );
    // likelihood:
   gamma ~ normal( mu + X * beta , sigma );
}

}
```

D Structural estimation

We implement our structural equations based on the discriminability equation in the main text, using the objective probability of winning, p, in DfD, DfD+forced, and DfE+forced. We use the sampled probability $ln\left(\frac{\alpha}{\beta}\right)$ in DfE, and complement this with an assumption about the log-cost benefits in the case that one of the outcomes has not yet been observed when the decision is taken, as described above.

We keep the model as simple as possible in order to maximize our comparative power and to keep the model parsimonious. This means, first of all, that we normalize the coding noise variance by division with the variance of the prior, so that $\gamma = \frac{1}{1+\frac{\nu^2}{\sigma^2}}$. This helps both identifiability and comparability across treatments but happens without loss of generality, since it is the ratio between coding noise variance and prior variance that determines behavior (see also Natenzon 2019). Another assumption that we maintain throughout the paper is that the mean of the prior, μ, remains unaffected over the course of the experiment. We exploit this in the estimation by letting μ be the same across the 2 parts of the experiment, whereas ν and as a consequence γ and θ are all allowed to vary freely.

We estimate the model using a Bayesian hierarchical setting in Stan (Carpenter et al. 2017). The hierarchical setting allows us to pool information from the aggregate estimation, which provides the priors, and from individual-level parameter estimates, which contribute to the aggregate in proportion to their precision. The aggregation equation follows exactly the equation we describe for our Bayesian inference process. Vieider (2024*a*) provides a step-by-step tutorial on the estimation of decision models in Stan.

Below, we include an commented version of the code we use in DfD, DfD+forced, and DfE+forced (the code used in DfE is very similar, and only has an additional parameter ρ, as well as including the truly observed log-odds as data; it is available upon request). We define the variables at the level of the individual *choices*. This allows us to implement a literal specification of our model, where task-specific quantities are encoded by parameters α and β. These parameters are nested in individual-level parameters, which we use to fit the choice data, and which ensures that the choice-level parameters are identified and well-behaved (since the individual-level parameters act as informative priors). Finally, individual-level parameters are nested within an overall distribution.

We check convergence by making sure that all R-hats are below 1.05. We also carefully check that any divergent iterations do not indicate problems with the posterior (and discard all estimates with more than 1% divergent iterations). The hyperpriors on the aggregate parameter means are given very wide priors, which makes them *mildly regularizing*—they help the convergence of the simulation algorithm by being centered around the region where we expect the parameter values to fall, but they attribute significant probabilitry mass to 1 order of magnitude above the region into which we would expect the parameters to reasonably fall. Our estimates are indeed not sensitive to the choice of the exact parameter values. This follows best practices in Bayesian estimation.

```
data{ \\declare data
    int<lower=1> N; \\number of observations
    int<lower=1> N_id; \\number of subjects
    array[N] int id; \\unique identifier
    array[N] real high; \\outcome x
    array[N] real low; \\outcome y
    array[N] real sure; \\outcome c
    array[N] real p; \\probability
    array[N] int choice_risky;\\choice: 1 if risky
    array[N] int part2; \\dummy to indicate part 2
```

```
}
transformed data{
 array[N] real lcb; \\log cost benefit ratio
 array[N] real llr; \\log-odds
    for (i in 1:N){
      lcb[i] = log( (sure[i] - low[i]) / (high[i] - sure[i]) );
      llr[i] = log( p[i]/(1 - p[i]) );
      }
}
parameters{
    vector[3] means; \\aggregate mean parameters on log scale
    vector<lower=0>[3] tau_id; \\aggregate parameter variances
    cholesky_factor_corr[3] L_omega_id; \\decomposed covar matrix
    array[N_id] vector[3] Zid; \\stan dardized individual-level parameters
}
transformed parameters{
// covar and temp parameters
  matrix[3,3] Rho_id = L_omega_id * L_omega_id'; \\obtain covariance matrix
  array[N] vector[3] pars; \\parameter matrix on log scale
// generative parameters:
  vector[N] mu; \\prior mean
  vector<lower=0>[N] kappa; \\concentration part1
  vector<lower=0>[N] kappaf; \\concentration part2
// derived parameters from here
  vector[N] alpha; \\derived parameters--see definitions in text, and below
  vector[N] beta;
  vector[N] nu;
  vector[N] gamma;
  vector[N] theta;
  vector[N] omega;
  vector[N] alphaf;
  vector[N] betaf;
  vector[N] nuf;
  vector[N] gammaf;
  vector[N] thetaf;
  vector[N] omegaf;
  for (i in 1:N){
    pars[i] = means + diag_pre_multiply(tau_id,L_omega_id) * Zid[id[i]];
    mu[i] =  pars[i,1];
    kappa[i] = exp(pars[i,2]);
    kappaf[i] = exp(pars[i,3]);
// define derived parameters
    alpha[i] = kappa[i] * p[i];
```

```
    beta[i] = kappa[i] * (1 - p[i]);
    nu[i] = sqrt( trigamma( alpha[i] ) + trigamma( beta[i] ) );
    gamma[i] = 1/( 1 + nu[i]^2 );
    theta[i] = exp( ( gamma[i] - 1) * mu[i]) ;
    omega[i] = nu[i] * gamma[i];
    alphaf[i] = kappaf[i] * p[i];
    betaf[i] = kappaf[i] * (1 - p[i]);
    nuf[i] = sqrt( trigamma( alphaf[i] ) + trigamma( betaf[i] ) );
    gammaf[i] = 1/( 1 + nuf[i]^2 );
    thetaf[i] = exp( ( gammaf[i] - 1) * mu[i] ) ;
    omegaf[i] = nuf[i] * gammaf[i];
  }
}
model{
    vector[N] udiff; \\local vector
\\priors for aggregate (hierarchical) parameters
    tau_id ~ exponential(5);
    L_omega_id ~ lkj_corr_cholesky(4);
    means[1] ~ normal(0, 5);
    means[2] ~ normal(0, 5);
    means[3] ~ normal(0, 5);

\\priors for individual level parameters, standardized:
  for (n in 1:N_id)
      Zid[n] ~ std_normal();

\\the mode:
  for ( i in 1:N ) {
         udiff[i] = ( ( gamma[i] * llr[i] - lcb[i] - log(theta[i]) )/ omega[i] ) * (1 - part2[i
                     ( ( gammaf[i] * llr[i] - lcb[i] - log(thetaf[i]) )/ omegaf[i] ) * part2[i];
         choice_risky[i] ~ bernoulli( Phi( udiff[i] ) );
    }
}
\\code below recovers individual-level parameters
generated quantities{
  vector[N] log_lik;
  vector[N] udiff;

  vector[N_id] mun;
  vector[N_id] kappan;
  vector[N_id] alphan;
  vector[N_id] betan;
  vector[N_id] nun;
```

```
  vector[N_id] gamman;
  vector[N_id] thetan;
   vector[N_id] kappafn;
  vector[N_id] alphafn;
  vector[N_id] betafn;
  vector[N_id] nufn;
  vector[N_id] gammafn;
  vector[N_id] thetafn;

  vector[3] temp;
    for(n in 1:N_id){
      temp = means + diag_pre_multiply(tau_id,L_omega_id) * Zid[n];
      mun[n] = temp[1];
      kappan[n] = exp(temp[2]);
      kappafn[n] = exp(temp[3]);
      alphan[n] = kappan[n]/2;
      betan[n] = kappan[n]/2;
      nun[n] = sqrt( trigamma( alphan[n] ) + trigamma( betan[n] ) );
      gamman[n] = 1/(1 + nun[n]^2 );
      thetan[n] = exp( ( gamman[n] - 1 ) * mun[n] );
      alphafn[n] = kappafn[n]/2;
      betafn[n] = kappafn[n]/2;
      nufn[n] = sqrt( trigamma( alphafn[n] ) + trigamma( betafn[n] ) );
      gammafn[n] = 1/(1 + nufn[n]^2 );
      thetafn[n] = exp( ( gammafn[n] - 1 ) * mun[n] );
      }

    for ( i in 1:N ) {
        udiff[i] = ( ( gamma[i] * llr[i] - lcb[i] - log(theta[i]) )/ omega[i] ) * (1 - comp[i])
                     ( ( gammaf[i] * llr[i] - lcb[i] - log(thetaf[i]) )/ omegaf[i] ) * comp[i];
        log_lik[i] = bernoulli_lpmf( choice_risky[i] | Phi_approx( udiff[i] ) );
    }
}
```

D.1 Structural estimation results

We use structural estimation to more deeply assess the hypothesis that both probability-dependence and the description-experience gap are a consequence of cognitive noise — and that our treatments eliminate these patterns by eliminating this noise. We structurally estimate our model from choice data based on our discriminability equation (4). The key parameter driving both probability-dependence and the GAP in our model (and, therefore, our focus in this section) is γ, the weight the DM puts on her perception of the log-odds in the decision process. We will refer to this as "likelihood-discriminability," mirroring the name given the equivalent parameter in the LLO function, "likelihood-sensitivity." In the model, γ is an inverse function of coding noise: the smaller coding noise ν becomes, the closer γ will come to 1, producing perfect discriminability of log-odds. Importantly, this parameter is estimated, in part, using inconsistencies in subjects' choices across repeated instances of the same task (recall, four random tasks were repeated for each subject) which give us direct, subject-level measures of *behavioral noise.* This analysis therefore relies on new data, not reported in the previous analysis.

We estimate the model using Bayesian hierarchical techniques, which optimally combine individual-level information with group-level evidence (Gelman et al. 2014). This allows us to study distributions of individual-level parameters based on relatively few decision tasks (details and code are provided in Online Appendix E). We normalize the variance of the prior to $\sigma = 1$ throughout, so that sampling variance is measured relative to the variance of the prior, ν/σ. This is done without loss of generality and to improve comparability across studies, simply leading to a rescaling of the equation (see Natenzon 2019 for an equivalent simplification).[37] We execute tests on distributional differences and correlations in individual-level parameters based on the means of the individual-level posteriors throughout. All comparisons are within-subject, leveraging our two stage design, unless specified otherwise. We report four main findings:

First, we find that, conditional on the information subjects have about probabilities, estimates of γ indicate strong (and similar) levels of sampling variance in DfD and DfE, with γ estimates well below the unbiased benchmark of 1. To estimate γ in a way that makes DfE and DfD estimates comparable, we estimate the model in DfE on the actually experienced probabilities (i.e., probabilities implied by the sample subjects have drawn), rather than the lottery's true probabilities.[38] Because of this, we must make an assumption on how subjects

[37] We estimate the model on choice data while leveraging our within-subject design. That is, we estimate the model using the data from both treatments, and assuming that the parameters governing the prior remain the same across the two treatments, while leaving the other model parameters free to vary. This allows us to maximize the informative content of our sparse choice stimuli. See Online Appendix E for details.

[38] We assume throughout that the initial Beta parameters, before any samples are observed, are $\alpha = \beta = 0.1$. This assumption derives from our general inference framework, based on a diffuse Dirichlet space – see Online Appendix A.1 for details. While values smaller than 1 are plausible (they imply that subjects expect relatively few outcomes in our general inference framework), our results are not sensitive to variations of this value within that range.

perceive the log cost-benefit ratio in cases in which the subject fails to sample both lottery outcomes before making a choice. Panel A in Figure 11 shows the cumulative distribution function of individual-level γ estimates under the assumption that DMs are "naive" in the sense that they judge costs and benefits to be equal in such cases. In panel B, we instead assume DMs are sophisticated in the sense that they realize that larger log-odds imply larger log cost-benefits; the correlation measuring the degree of sophistication thus must be estimated as an endogenous parameter (see Online Appendix E for details and additional results).

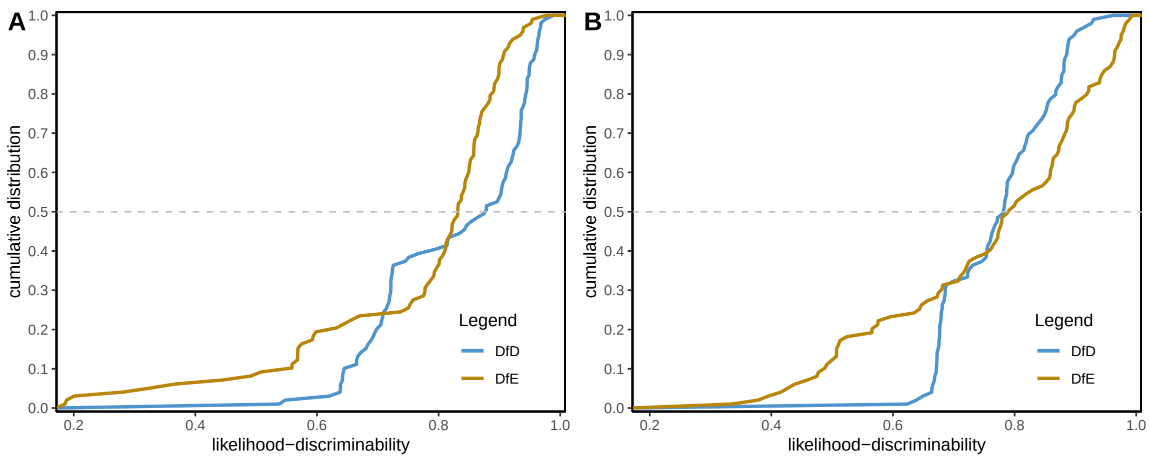

Figure 11: Structural estimates, DfD versus DfE

The figure shows structural estimates of the model parameters. Panel A compares likelihood-discriminability γ in DfD and DfE for a naive decision maker, who assumes costs and benefits to be equal when one of the outcomes has not been observed. Panel B compares likelihood-discriminability, γ, for a sophisticated DM, who (correctly) infers that log-odds and log costs-benefits are correlated in the choice problems. The correlation coefficient is thereby estimated endogenously from the data (see Online Appendix E for details).

Regardless of the approach taken, two findings stand out from Figure 11. First, in both DfD and DfE, γ falls well below the unbiased benchmark of 1, suggesting a strong role for inference bias in both settings as predicted by our model. Second, the distributions of γ estimates are similar in both DfD and DfE.[39] This is important because our model explains the GAP between these settings not via differences in γ but rather via the very different effects the model predicts γ has in DfD vs. DfE environments. The results therefore assure us that the model parsimoniously explains differences in lottery choices across treatments, conditional on the information available to subjects.

Second, we show that forced sampling in DfD and DfE results in a sharp increase in γ

[39] For the naive estimates pictured in panel A, likelihood-discriminability γ is somewhat smaller in DfE than in DfD ($p = 0.006$). For the sophisticated estimates in panel B, the two distributions produce roughly equal deviations above and below 0.5, and are not significantly different ($p = 0.979$).

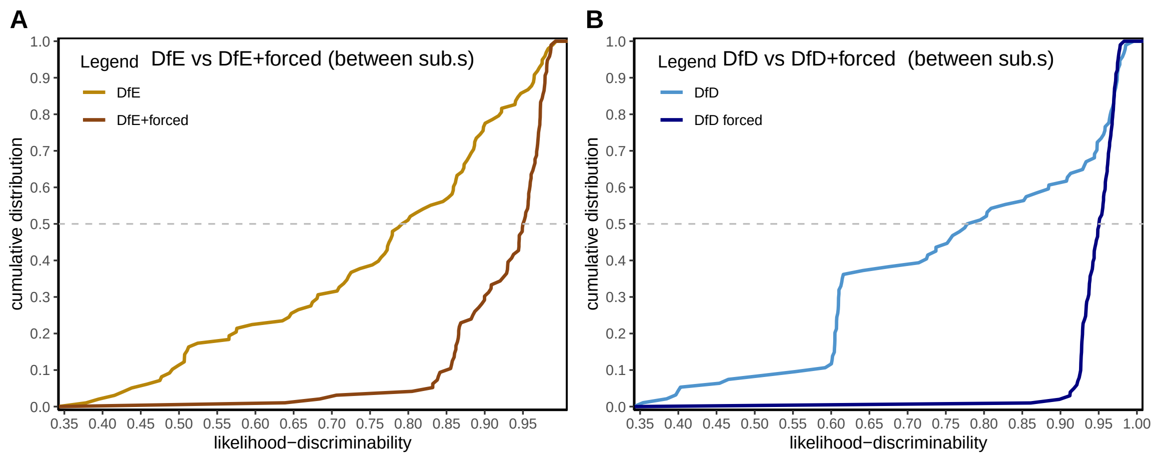

Figure 12: Structural estimates, DfD vs DfD+forced and DfE vs DfE+forced

The figure shows structural estimates of likelihood-discriminability γ. Panel A compares likelihood-discriminability in DfE and DfE+forced. Panel B compares likelihood-discriminability in DfD and DfD+forced.

towards 1 (the unbiased benchmark), suggesting that the intervention influences behavior (as predicted by the model) by severely reducing sampling variance and with it scope for sampling error. In panel A and B of Figure 12 respectively we plot CDFs of estimated individual-level mean γ estimates in DfE[40] and DfD with and without forced sampling.[41] In both cases, forced sampling causes a sharp rightward shift in the γ parameter, with medians in both cases of about 0.95 suggesting a near elimination of coding noise and inference bias.[42]

Third, we show that forced sampling in DfD and DfE – which, recall, caused a convergence in behavior between the two treatments – also causes a convergence in γ. This suggests (as our model predicts) a causal linkage between the two findings: joint convergence of γ in the two treatments towards 1 (signalling the disappearance of inference bias) causes lottery choice patterns to converge, suggesting (as predicted by the model) that coding noise was responsible for their initial divergence. Panel A of Figure 13 directly compares γ in DfD+forced and DfE+forced. Over most of the distribution, the panel shows that discriminability converges across the two treatments, suggesting that subjects are similarly free of inference bias in the two settings – a finding that matches the similar revealed risk

[40] In DfE we plot estimates that assume subjects make sophisticated inferences about the cost-benefit ratio, as discussed above.

[41] For this analysis, we use a between-subject comparison in both cases since DfE vs DfE+forced can only be compared between subjects; in DfD, replacing this with within-subject comparisons yields very similar results (cfr. Online Appendix E).

[42] Estimates also reveal a sharp reduction in cross-subject variance. This too is a prediction of the model, since the treatment is predicted to have similar impacts on both initially high and low noise subjects.

aversion in choices in the two settings. Indeed, non-parameteric tests detect no significant difference between the two distributions ($p = 0.376$).[43]

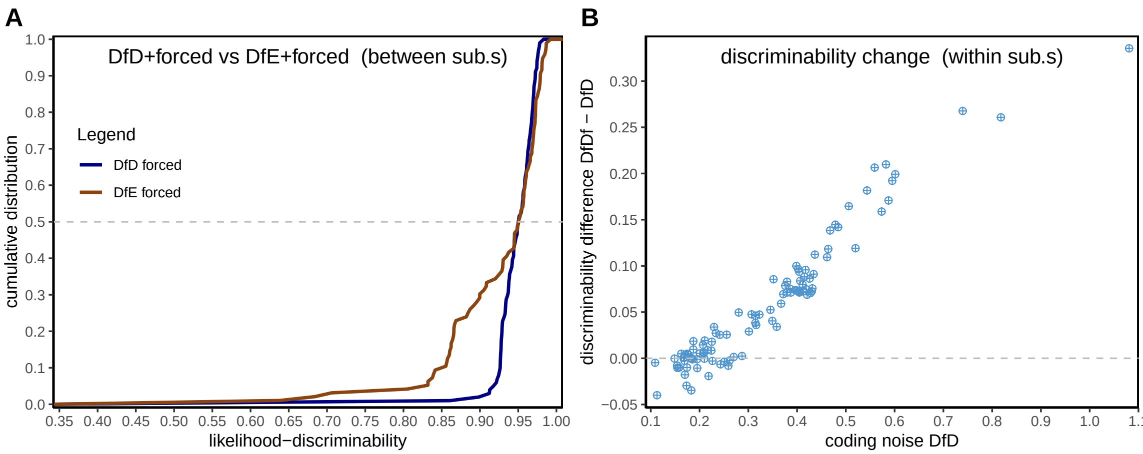

Figure 13: Effects of forced sampling, Structural estimates

The figure shows structural estimates of the model parameters. Panel A directly compares likelihood-discriminability γ in DfD+forced and DfE+forced. Panel B compares likelihood-discriminability, γ, in DfD without and with forced sampling. Panel B plots coding noise in first stage DfD against the change in likelihood-disciminability when forced sampling is introduced.

Panel B of Figure 13 illustrates the reason for this effect by plotting coding noise ν (measured in the DfD choices in Stage 1 of the experiment) against the difference between γ in DfD and DfD+forced (defined as $\gamma_2 - \gamma_1$, with subscripts indicating the stage of the experiment), exploiting our within-subject design. The figure shows clearly that the effect of sampling is most pronounced for those subjects who had the largest coding noise to begin with. These results strongly support an additional prediction of the model: that sampling should have the strongest effect on subjects who have relatively high coding noise to start with (i.e., relatively small 'spike counts' $\widehat{\alpha}$ and $\widehat{\beta}$). This is a consequence of the fact that the reduction in coding noise decreases at a decreasing rate with further samples. The figure thus shows in a particularly sharp way how strong the effect of forced sampling is on likelihood-discriminability in the DfD treatment.

Finally, figure 14 shows the empirical cumulative distribution functions of decision noise, $\gamma\nu_p$, in DfD and DfD+forced. Decision noise in the model is predicted to be maximized at $\gamma = 0.5$. Increasing likelihood-discriminability γ thus ought to have the effect of lowering decision noise. This is exactly what we see in the figure: the decision noise distribution is

[43]Nonetheless, as is clear from the graph, discriminability is somewhat lower in the left hand tail of the DfE distribution. We hypothesize that this is due to limitations on subjects' memory, highlighting the value to subjects of having an explicit description of the outcomes and probabilities on the screen (in DfD+forced) to guard against inattention and working memory limitations.

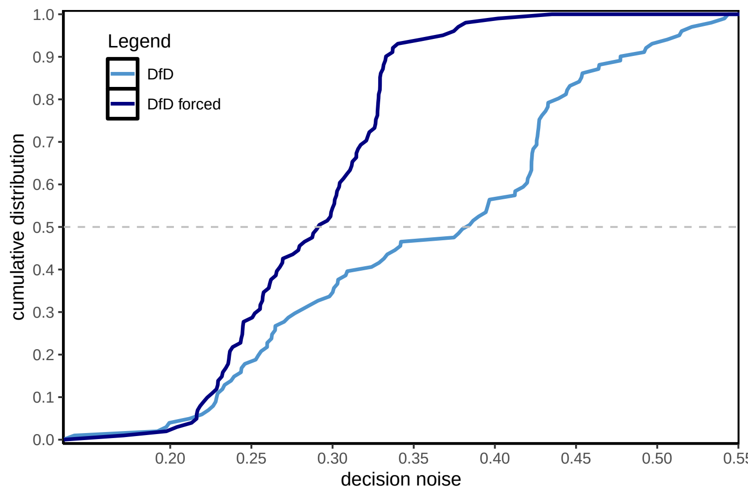

Figure 14: Decision noise in DfD and DfD+forced

Empirical cumulative distribution functions of decision noise $\gamma\nu_p$ in DfD and DfD+forced.

monotonically shifted to the left upon forced sampling, thus supporting a secondary prediction of the model — that increases in likelihood-discriminability ought to concomitantly produce reductions in decision noise. Importantly, this is *not* a purely mechanical effect, given that the model is flexible enough to dissociate decision noise from likelihood-sensitivity — see Vieider (2025) for a detailed discussion.

E Additional results

Additional results on free sampling in DfE

Subjects take relatively few samples in our experiment, something that may be explained by the high opportunity costs faced by subjects on Prolific, who—contrary to students in lab or classroom settings—can leave as soon as they are done with the experiment and move on to other earning opportunities. The average number of samples taken is 8, which puts our study at roughly the first tercile of the distribution summarized in the meta-analysis of Wulff et al. (2018). Samples taken, however, generally tend to be lower in tasks comparing lotteries with sure outcomes, as we use here. The average subject on the average task takes 3.3 samples from the safe option, but 4.3 samples from the risky option. However, samples vary greatly between individuals, ranging from 2 on average (1 per option) to about 40.

Panel A in Figure 5 examines the average samples by probability from the risky option at the

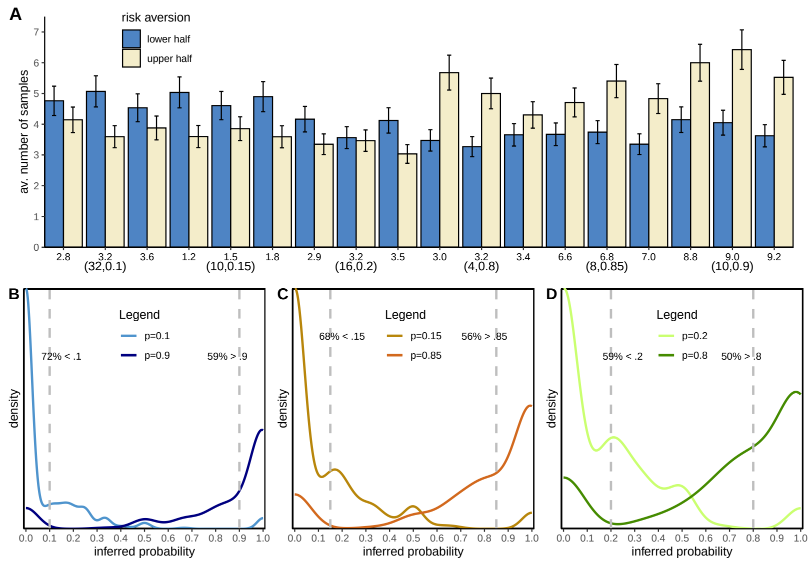

Figure 15: Samples by probability and risk aversion

The figure shows the number of samples taken from the risky option by probability and risk aversion at the task level resolution in Panel A. Risk aversion is assessed as the proportion of safe choice in the first, DfD part of the experiment, after removing repeated tasks. The categorization is obtained using a median split. Error bars show ± 1 standard error. Panels B through D show the distribution of sampled probabilities by different actual probabilities.

task resolution. The samples are presented following a median split on risk aversion in the first, description-based, part of the treatment, implemented as the proportion of choices of the sure amount. This aims to test our model prediction according to which samples should vary with the underlying probability depending on the initial risk aversion of the DM. These predictions are strongly supported by the evidence presented in the figure. Risk averse DMs take few samples from small-probability lotteries, but sample significantly more from large-probability lotteries. For the least risk averse half of the sample, we observe a (somewhat weaker) trend in the opposite direction. This aligns with our prediction, according to which risk averse DMs should have less of a conflict between noise and sampling bias in small probability lotteries, thus reaching a decision more quickly.

The small number of samples taken is reflected in the probabilities people experience. This is illustrated figure 15, panels B through D, which plot distributions of probabilities inferred from the actual samples a DM observed. For small probability lotteries, subjects experience a smaller probability than the true one in 66% of cases overall, while getting a correct picture in some 3.4% of cases. For large probability lotteries this picture is reversed, with 55% of samples over-estimating the true probability, and only 2.2% resulting in a correct estimate. The asymmetry we see between small and large probabilities suggests that the larger samples taken for large probabilities result in a more balanced picture.

Nonparametric within-subject results

Here, we replicate the nonparametric between-subject analysis in the paper by presenting within-subject comparisons wherever this is possible. The descriptions of the figures are self-contained.

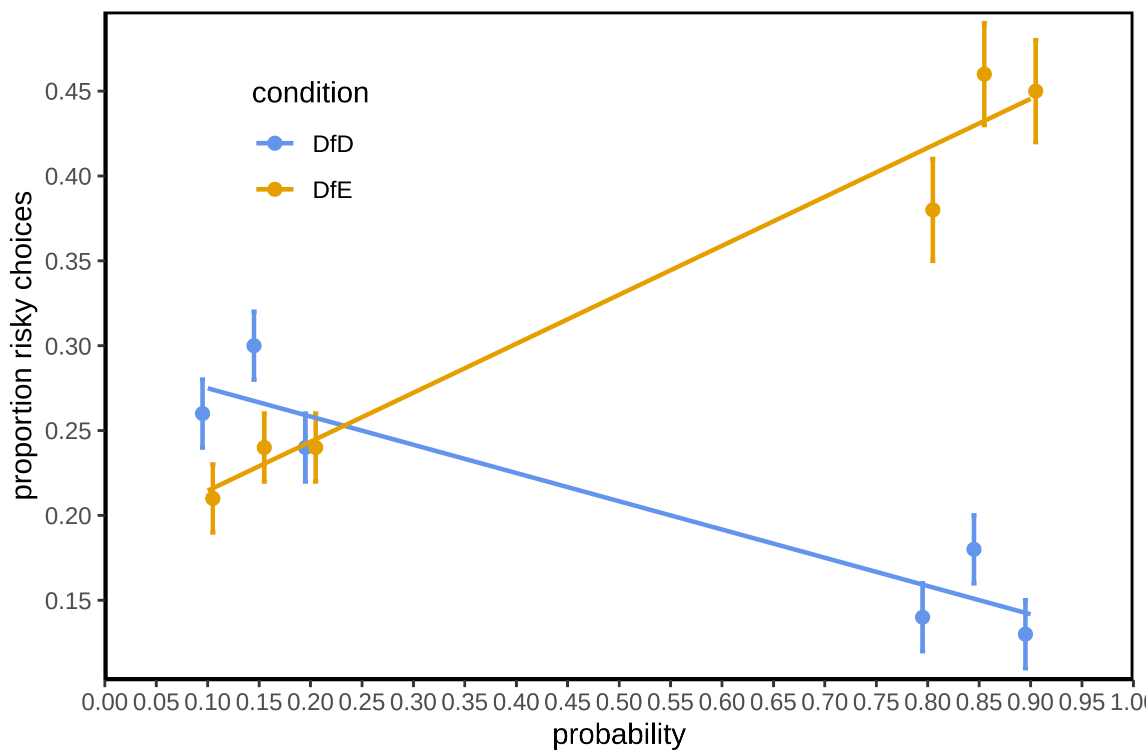

Figure 16: The GAP: within-subject

Choice proportions by probability for the decision-experience gap: DfD versus DfE. Error bars indicate 1 standard error.

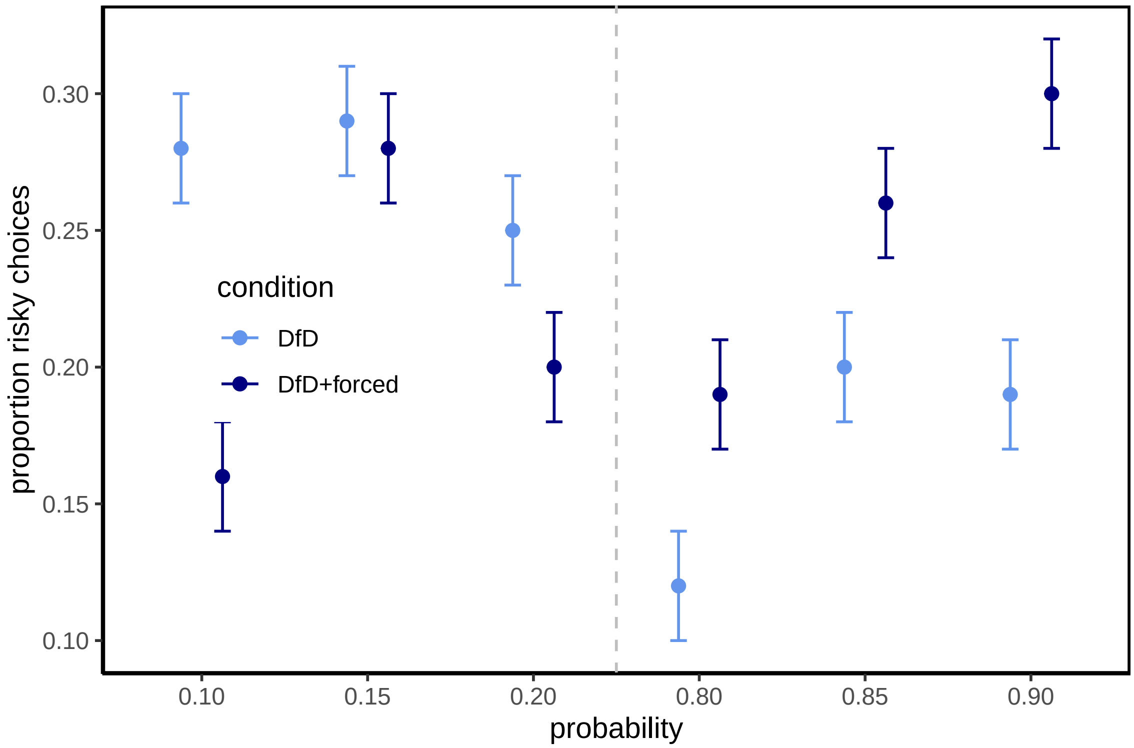

Figure 17: DfD+forced vs DfD within subject

Choice proportions by probability, within-subject comparison between DfD+forced and DfD. Error bars indicate 1 standard error.

Figures at task level

Here, we show all figures for which we averaged across c at the probability level at a task-level resolution. The figure descriptioins are self-contained.

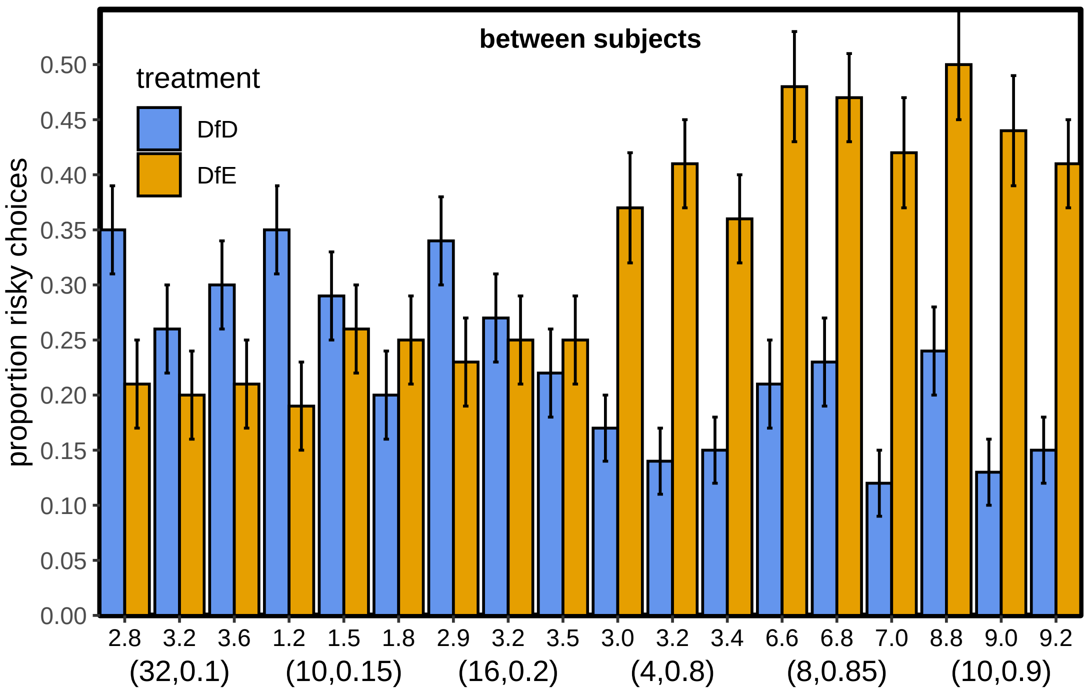

Figure 18: The GAP at the task level (between-subjects)

Choice proportions by task for the decision-experience gap: DfD versus DfE. Error bars indicate 1 standard error.

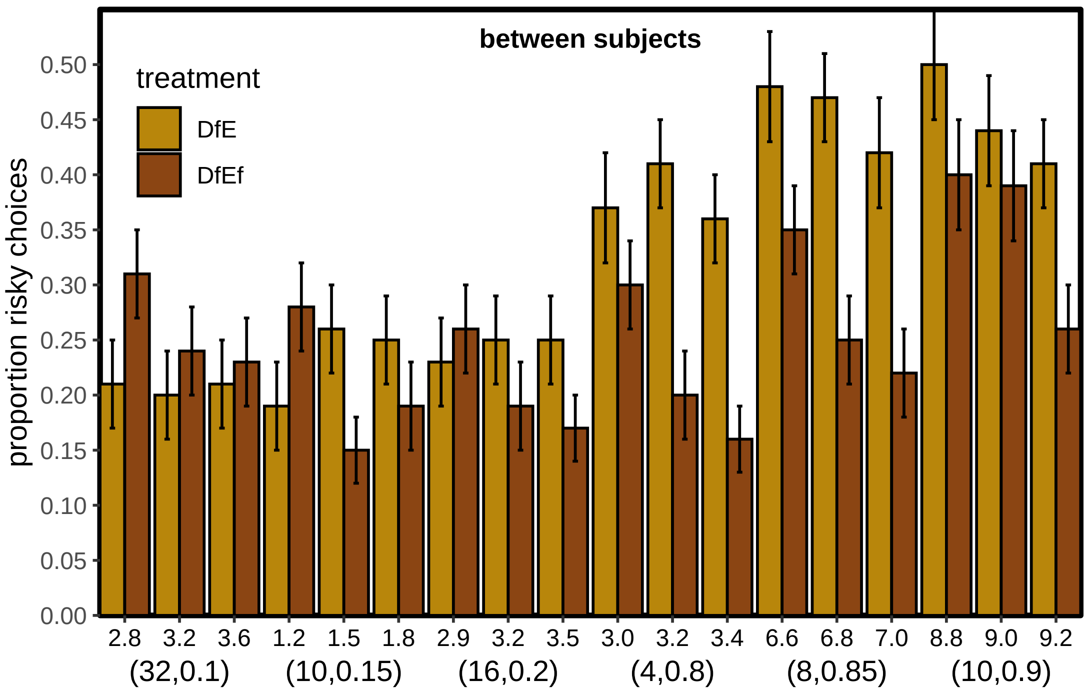

Figure 19: DfE+forced versus DfE at the task level (between-subjects)

Choice proportions by task for DfE+forced compared to DfE. This comparison is only possible between-subjects. Error bars indicate 1 standard error.

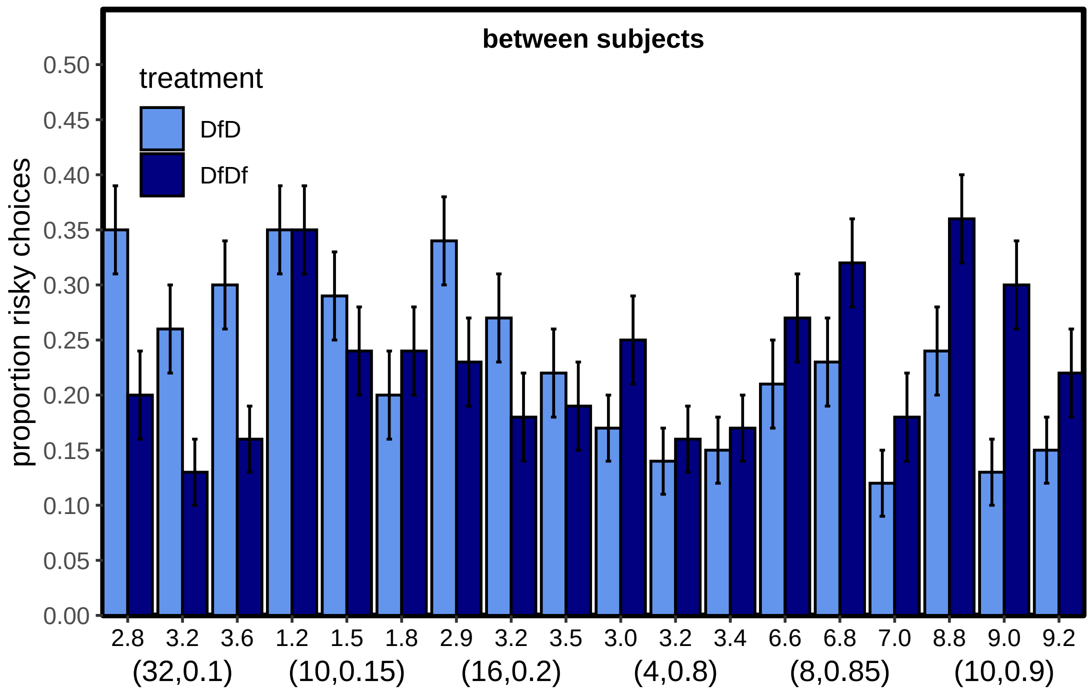

Figure 20: DfD+forced versus DfD at the task level (between-subjects)

Choice proportions by task for DfD+forced compared to DfD. Error bars indicate 1 standard error.

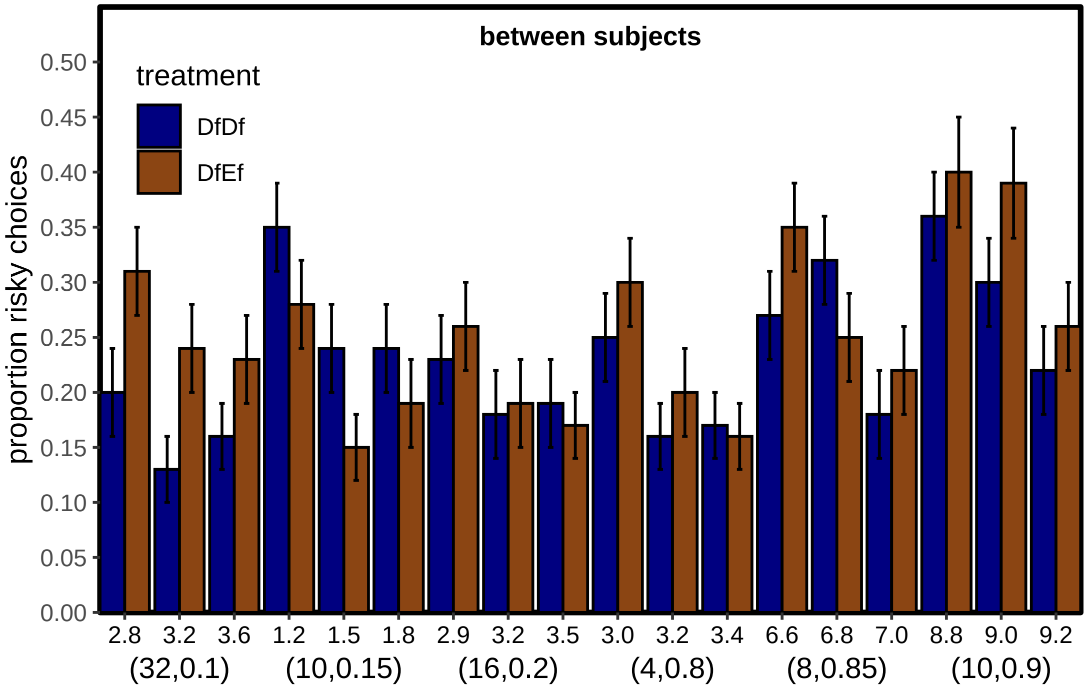

Figure 21: DfD+forced versus DfD at the task level (between-subjects)

Choice proportions by task for DfD+forced compared to DfD. Error bars indicate 1 standard error.

Within-subject structural results

This section contains within-subject structural comparisons for those cases where we used between-subject comparisons in the main text, but within-subject comparisons are possible. The descriptions of the graphs are self-contained.

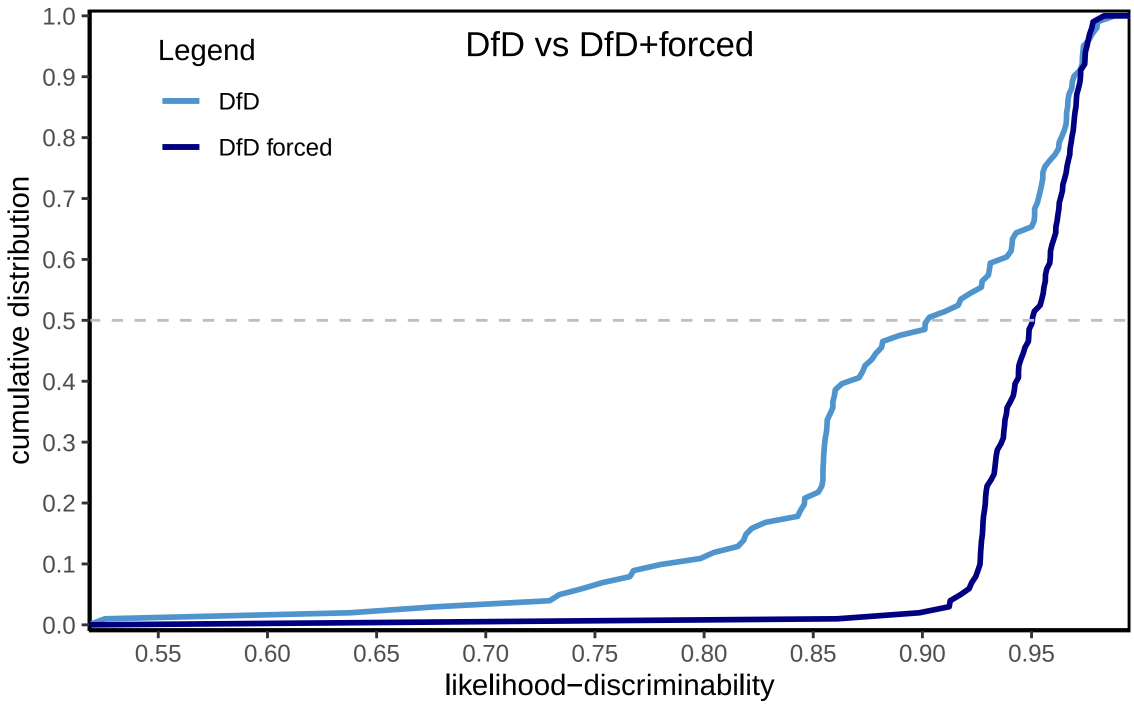

Figure 22: Likelihood-discriminability in DfD vs DfD+forced, within subject

Likelihood-discriminability, γ, empirical cumulative distribution function of individual-level posterior means. Within-subject comparison between DfD and DfD+forced.

F Instructions to Subjects

F.1 Stage 1 Instructions

Subjects in all treatments, were given the following instructions prior to Stage 1.

Instructions: Bonus

Please pay close attention to the following instructions. We will ask you **comprehension questions** about the instructions. Anyone who answers these questions correctly **the first time** will receive a $0.25 bonus.

Part 1 Instructions: Digital Bags

1. There will be two Parts to this experiment.

2. Part 1 will consist of **several Tasks**. In each Task you will choose between two **digital bags** -- Bag A and Bag B

3. Each bag contains **20 coins** and each coin is worth some amount of money to you as a **bonus**.

Bag A	Bag B
80% are worth $2.00 20% are worth $0.00	100% are worth $1.00

Example: *In the example above, 80% of the coins in Bag A (i.e. 16 coins) are worth $2, while 20% of the coins (4 coins) are worth $0. On the other hand, 100% of the coins in Bag B are worth $1.*

4. No coin in any bag is worth more than $35.

5. We will **randomly digitally draw one coin** from one of the two bags (Bag A or Bag B), and use that coin to determine how much money to add to your bonus. Each coin in the bag is **equally likely** to be drawn.

6. Your job is to decide **which bag** you would like us to randomly draw a coin from for your payment, by clicking one of the two buttons as in the example below.

Make Your Choice

Choose Bag A ○ Choose Bag B ○

Example: In the earlier example, if you choose Bag A there is an 80% chance you earn $2 and a 20% chance you earn $0. However, if you choose Bag B there is a 100% chance you earn $1.

Please answer the following comprehension questions about the following pair of bags:

Bag A	Bag B
70% are worth $3.00 30% are worth $0.00	100% are worth $2.00

If you answer all of these questions correctly **on the first try** we will pay you a bonus of $0.25.

In the example above, what is the likelihood (percentage chance) of earning exactly **$3** if you choose **Bag A.**

0%

30%

70%

100%

In the example above, what is the likelihood (percentage chance) of earning exactly **$2** if you choose **Bag A.**

0%

30%

70%

100%

In the example above, what is the likelihood (percentage chance) of earning exactly **$3** if you choose **Bag B.**

- 0%
- 30%
- 70%
- 100%

In the example above, what is the likelihood (percentage chance) of earning exactly **$2** if you choose **Bag B.**

- 0%
- 30%
- 70%
- 100%

Instructions: Details

1. We will give you a total of **22 tasks** in Part 1. In each task, the contents of the bags will be **different**.
2. At the end of the experiment, we will **randomly select 10% of participants** to actually be paid a bonus based on their choices.
3. If you are selected to be paid a bonus, we will randomly select one of the tasks and use your choice to determine your bonus.

F.2 Stage 2 Instructions

In Stage 2, subjects assigned to the DfD treatment were given the following instructions:

Part 2 Instructions

1. The choices in Part 2 will be similar to the choices in Part 1.
2. We will give you a total of **22 tasks** in part 2. In each task, the contents of the bags will be **different**.
3. At the end of the experiment, we will **randomly select 10% of participants** to actually be paid a bonus based on their choices.
4. If you are selected to be paid a bonus, we will randomly select one of the tasks and use your choice to determine your bonus.

Subjects assigned to DfE or DfE+forced were initially given the following instructions:

Part 2 Instructions

In Part 2 tasks, you will be making the same kind of choices you made in Part 1. However, unlike in Part 1, in Part 2 we will not describe what is contained in each bag. Instead you can learn about the contents of the bags by **sampling coins from them**.

Subjects assigned to DfD+forced or DfD+forced were initially given the following instructions:

Part 2 Instructions

In Part 2 tasks, you will be making the same kind of choices you made in Part 1. However, you will also be allowed to **sample coins from each bag** before making your choices.

After this, subjects in DfE or DfD+free were given the following instructions:

Part 2 Instructions: Sampling

1. In this Part, in order to help you make your decision, we will allow you to **"Sample" from each of the bags**. We will show you buttons like the ones below. Each time you click on a button, it will **draw one of the coins** from the corresponding bag and show you how much is on it. This **won't affect your earnings** -- it is just a chance to learn about each bag.

   Sample Each Bag

   $2.00

   Sample Bag A   Sample Bag B

   <u>Example</u>: *In the example above, you have clicked bag A and the computer randomly drew a coin worth $2 from it (shown in green).*

2. You can Sample from each bag **as many times as you like**. Each time you do, the computer will "put the coin back in the bag" before you sample again.

3. When you are finished sampling, just click on a button like the ones below to make your real choice (the choice that actually affects your earnings). The computer will then randomly draw one of the 20 coins from the bag to determine your bonus.

   Make Your Choice

   Choose Bag A   Choose Bag B

while subjects in DfE+forced or DfD+forced were instead given the following instructions:

Part 2 Instructions: Sampling

1. In this Part, in order to help you make your decision, we will allow you to **"Sample" from each of the bags**. We will show you buttons like the ones below. Each time you click on a button, it will **draw one of the coins** from the corresponding bag and show you how much is on it. This **won't affect your earnings** -- it is just a chance to learn about each bag.

   Sample Each Bag

   $2.00

   Sample Bag A   Sample Bag B

   Example: *In the example above, you have clicked bag A and the computer randomly drew a coin worth $2 from it (shown in green).*

2. You must Sample from **each bag 20 times**, drawing each of the 20 coins out of each bag. Each time you sample, the computer will take the sampled coin out of the bag before you sample again.

   $2.00

   Sample Bag A   Sample Bag B

   sampled 8 / 20 times.   sampled 6 / 20 times.

   Example: *In the example above, you have sampled 8 times so far from Bag A and 6 times so far from Bag B. You must sample a total of 20 times from each Bag before you can make your real decision.*

3. When you are finished sampling, just click on a button like the ones below to make your real choice (the choice that actually affects your earnings). The computer will then randomly draw one of the 20 coins from the bag to determine your bonus.

   Make Your Choice

   Choose Bag A   Choose Bag B

Finally, all subjects were given these instructions prior to the beginning of Stage 2:

Part 2 Instructions: Details

1. We will give you a total of **22 tasks** in part 2. In each task, the contents of the bags will be **different**.

2. At the end of the experiment, we will **randomly select 10% of participants** to actually be paid a bonus based on their choices.

3. If you are selected to be paid a bonus, we will randomly select one of the tasks and use your choice to determine your bonus.