Plinko: A Theory-Free Behavioral Measure of Priors for Statistical Learning and Mental Model Updating

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Abstract

Probability distributions are central to Bayesian accounts of cognition, but behavioral assessments do not directly measure them. Posterior distributions are typically computed from collections of individual participant actions, yet are used to draw conclusions about the internal structure of participant beliefs. Also not explicitly measured are the prior distributions that distinguish Bayesian models from others by representing initial states of belief. Instead, priors are usually derived from experimenters’ intuitions or model assumptions and applied equally to all participants. Here we present three experiments using “Plinko”, a behavioral task in which participants estimate distributions of ball drops over all available outcomes and where distributions are explicitly measured before any observations. In Experiment 1, we show that participant priors cluster around prototypical probability distributions (Gaussian, bimodal, etc.), and that prior cluster membership may indicate learning ability. In Experiment 2, we highlight participants’ ability to update to unannounced changes of presented distributions and how this ability is affected by environmental manipulation. Finally, in Experiment 3, we verify that individual participant priors are reliable representations and that learning is not impeded when faced with a physically implausible ball drop distribution that is dynamically defined according to individual participant input. This task will prove useful in more closely examining mechanisms of statistical learning and mental model updating without requiring many of the assumptions made by more traditional computational modeling methodologies.

Keywords: Bayesian Models, Individual Differences, Empirical Priors, Mental Representations, Perceptual Updating

Introduction

Humans have a remarkable ability to learn complex statistical representations of the world (Saffran et al., 1996; Nissen and Bullemer, 1987; Orbán et al., 2008; Turk-Browne et al., 2005). We use this statistical information to build beliefs about our environment, sometimes called mental models, and update these beliefs when contingencies change (Tenenbaum et al., 2011; Fiser et al., 2010; Johnson-Laird, 2013). The way we use such statistical information has become a key feature of many theories of learning and general cognition (Frost et al., 2019), ranging from decision making (Fisk, 2002; Tenenbaum et al., 2011; Summerfield and Tsetsos, 2015) and language development (Saffran et al., 1996), to measuring the cognitive consequences of brain damage (Danckert et al., 2012; Filipowicz et al., 2016; Stöttinger et al., 2014b; Palminteri et al., 2012).

At a minimum, mental models should contain representations of expected outcomes. These expectations ought to be based on prior experiences/beliefs. We argue that an appropriate method for examining mental models and statistical learning should be theoretically agnostic as to how the initial conditions of beliefs are represented. This is not trivial: research shows that the beliefs we use to interpret events can have a significant impact on decision
Our task directly collects priors and current trying to fit current models or theories that may be of insufficient specificity (Frost et al., 2019; Ioannidis, 2005). While such measures reveal how closely participants manage to match task contingencies, they give only limited information as to which beliefs were driving responses. As highlighted by Stöttinger et al. (2014b), data trends from individual responses alone can result from a number of different beliefs that may be unknown to the experimenter.

Recent computational approaches have attempted to infer participant beliefs by modeling their behavior (Nassar et al., 2010, 2012; O’Reilly et al., 2013; McGuire et al., 2014; Sepahvand et al., 2014; Collins and Koechlin, 2012). For example, Bayesian models of human learning represent participant beliefs as probability distributions, representing how likely events are to occur (Glaze et al., 2018; Nassar et al., 2010, 2012; O’Reilly et al., 2013; McGuire et al., 2014; Tenenbaum et al., 2011; Griffiths and Tenenbaum, 2006). These distributions are then updated with each new observation, providing a dynamic representation of the way participant beliefs evolve throughout a task.

However, Bayesian models make important assumptions that are not often accompanied by empirical evidence. Most prominently, the success of a Bayesian model depends largely on the prior, the distribution that represents beliefs participants bring to a task before observing any information. These priors are often selected by the researchers themselves, and are assumed to be the same across participant groups. Critics have noted that these freedoms and assumptions in model design make Bayesian methods too flexible, rendering them essentially unfalsifiable (Bowers and Davis, 2012; Jones and Love, 2011).

What is required, therefore, is a task that allows for flexible representations of participant beliefs without assuming a prior. That is, explicitly collecting a prior from the individual rather than assuming a ‘one size fits all’ approach. In the current article we present such a task. Based on the game ‘Plinko’, participants are given an intuitive environment in which, rather than make single responses, they draw distributions to indicate how likely they believe certain events are to occur. Our task affords the opportunity to collect idiosyncratic priors in a theory-free manner, before any evidence is presented to the participant.

Our task also provides a more realistic representation of participants - as individuals who carry their own idiosyncratic beliefs (priors) or decision-making tendencies into a statistical learning task (Frost et al., 2019; Franken and Muris, 2005). In the case of ‘ecologically valid’ tasks, these differences may be attributed to differences in accumulated knowledge (Siegelman et al., 2018). In the case of more novel or abstract tasks, where previous life experience is less likely to directly inform optimal behavior, individual differences in priors may still exist in the form of individual differences in information processing or perception. Regardless, we neglect a crucial component of human statistical learning when we neglect individual differences in the initial conditions of belief.

Adding richness to the data collected in statistical learning tasks removes researcher degrees of freedom when trying to fit current models or theories that may be of insufficient specificity (Frost et al., 2019; Ioannidis, 2005). Our task directly collects priors and current beliefs without the need to infer them from sequential participant actions. In order to fit a Bayesian model, for example, to our belief measurements, one must fit model parameters without freely setting a prior or decision rule to describe participant actions. In other words, we force ourselves to fit models of beliefs to literal measures of belief (initial and concurrent), rather than inferring the belief driving observed actions through the adoption of un-tested assumptions.

Plinko also affords us the ability to invert the standard operating procedure of theoretical developments in belief updating. Treating participant beliefs as latent states to be inferred by participant actions requires candidate models of belief to be compared. If none of the candidate models correctly capture participants’ latent beliefs, they are either doomed to fail or be wrongly adopted. Plinko instead provides an explicit theory-free measure of beliefs whose data can be used to form the appropriate theory.

In our presented experiments, each event is portrayed as a ball drop landing in one of 40 slots. Participants beliefs are thus represented as histograms of 40 bars, where the relative heights of the bars indicate the participants’ relative expectation of where future ball drops will land. However, the presented events need not necessarily be ball drops. In principle, our presented method can be used for any situation where discrete events could be easily represented on a uni-dimensional ordered spatial domain over which a histogram could be drawn. This affords the opportunity to explore statistical learning in either a domain specific, or a domain general manner. We particularly emphasize the individual nature of participant drawn histograms as the most useful feature of our proposed methodology.

Here we present three experiments that demonstrate Plinko’s utility as a behavioral measure of mental models and statistical learning. Plinko can be used to cluster participants by their priors to predict learning outcomes (Experiment 1), measure how participants update to unannounced distribution changes (Experiment 2), and to measure the capacity to represent physically implausible probability distributions that are dynamically defined.
according to participant input (Experiment 3). We also verify that collected priors are reliable representations that
can meaningfully characterize prior beliefs on an individual level, and do not simply reflect the intuitive physics of
our task (Experiment 3).

**General Method**

**Task Environment**

We developed a computerized version of the game “Plinko”, a modern incarnation of Galton’s Bean Machine
(Galton, 1894) featured on the American game show *The Price is Right*. In Plinko, balls fall through pegs to land
in slots below. In our task, participants view a triangle of 29 black pegs drawn on the computer screen while a red
ball is dropped from the top peg. The ball follows a cascading path to land in one of the 40 slots below the pegs.
The algorithm used to determine ball drop trajectory varied by experiment.

In one version, the participants provided their estimates of the ball drop distribution by clicking and dragging
the mouse below the slots to draw a histogram. In another, participants used a touchscreen to draw histograms
with their fingers. We told participants that the relative heights of their drawn bars are proportionate to the relative
probabilities of where they estimate the ball will land when dropped. That is, they were told that higher bars
represented a higher probability that a ball would fall in a slot, lower bars a lower probability, and that drawing
no bar represented zero probability. Participants could draw bars under one, some, or all slots, provided at least
one bar was drawn on the screen before proceeding to the next ball drop. The total area of participant drawn
histograms was not restricted, so long as the bars fit within the available display (Figure 1). We programmed the
task in Python using the PsychoPy library (Peirce, 2009). A demo and source code for the task is freely available
online at https://osf.io/dwkie.

![Figure 1: Schematic of the Plinko task. A red ball ‘fell’ through a pyramid of to land in one of the 40 slots below. Participants first drew bars using the computer mouse (or their finger on a touch screen) to indicate the most likely locations the ball would land in – higher bars indicate an expectation of higher likelihood. Seven slots are pictured here for illustrative purposes; the number of slots and pegs can be adjusted in the task’s source code.](image)

**Participants**

We analyzed preexisting data that were collected across three separate experiments. All 335 participants across
the three experiments were University of Waterloo undergraduates. The University of Waterloo’s Office of Research
Ethics cleared all study protocols and participants gave informed consent before participating. Some participants
in the dataset had missing or incomplete data. We only analyzed participants with complete trial data. Some par-
ticipants had missing demographic data (noted in each experiment when necessary). We still analyzed participants
with missing demographic data if they had complete trial data.

One experiment has been previously reported elsewhere, including three *other* Plinko Experiments that were
run in the same time frame as the three presented here (Filipowicz et al., 2018; Filipowicz, 2017). We did not
perform any additional analysis that is not presented here, however, data from all six Plinko experiments are
available at https://osf.io/dwkie.
Data Analysis

We measured participant performance by computing the angular similarity between the representative Euclidean vectors (Georgopoulos et al., 1986; Cer et al., 2018; Nominal Animal, 2018) of the participants’ drawn histograms, and the known experimental ball drop distributions. This results in a similarity score that ranges from 0 when participant histograms share no mass with the experimental reference distribution, to 1 when participants’ estimates are proportional to the experimental reference distribution. All aggregate learning curves are fitted local regression curves ($\alpha = 0.1$), a built-in method from the ggplot2 package (Wickham, 2016).

We performed all analyses in R (R Core Team, 2021), using the data.table (Dowle and Srinivasan, 2020), magrittr (Bache and Wickham, 2020), pracma (Borchers, 2021), pply (Wickham, 2011), ggplot2 (Wickham, 2016), ggpupr (Kassambara, 2020a), gridExtra (Auguie, 2017), gridGraphics (Murrell and Wen, 2020), purrr (Henry and Wickham, 2020), GmAMisc (Alberti, 2020), dynamicTreeCut (Langfelder et al., 2008), Matrix (Bates and Maechler, 2021), emdist (Urbanek and Rubner, 2012), and knitr (Xie, 2021) packages.

Experiment 1: Clustering priors

In this experiment, we explore the structure of prior variability and consider the influence a prior may have on statistical learning. The prior beliefs we hold impact how we interpret future events (Green et al., 2010; Hock et al., 2005; Lee and Johnson-Laird, 2013; Patrick and Ahmed, 2014; Bianchi et al., 2020; Stöttinger et al., 2014a). Consequently, some patterns of decision making are necessarily “better” than others given a particular task. In this experiment, we explore the properties of explicitly measured participant priors that may predict learning accuracy. We also consider the degree of mental model smoothing humans employ when integrating new statistical evidence. That is, do participant ball drop estimates approach the literal histogram of presented ball drops, or do they instead approach a smooth idealized distribution that ‘summarizes’ the discretely presented stimuli comparable to perceptual averaging (Ariely, 2001; Corbett and Oriet, 2011; Albrecht et al., 2012)?

Method

We analyzed the data of 266 University of Waterloo undergraduates (3 missing demographic data, 197 female, mean age = 19.96, $SD = 2.30$ years) to measure the predictive value of three prior clustering methods on learning accuracy.

Participants performed a series of tasks as part of a larger study investigating exploratory behaviour as a function of boredom proneness. These included two versions of a virtual berry picking foraging task (Struk et al., 2019), a ‘connect-the-dots’ problem solving task, and a version of a word search task intended to function as a cognitive ‘foraging’ task in which participants searched an array of letters to make words, moving freely between different problem sets. Finally, participants completed a version of the Plinko task. While the berry picking, connect-the-dots, and word search tasks were counterbalanced in order, the Plinko task was always performed last. Each task took around 10 minutes to complete. The series of tasks were completed on a touchscreen placed on a flat table, and inclined at approximately 25 degrees.

We asked participants to provide their estimate of the ball drop distribution before seeing any ball drops. Following collection of the one initial prior, we asked “How confident are you that your bars reflect the likelihood that a ball will fall in any of the slots?” We recorded confidence with a sliding bar from “Not Confident” to “Very Confident”, translating to a confidence score ranging from 0 to 1 (inclusive), where 1 is most confident. One participant did not have accompanying confidence data and was thus omitted from analysis of confidence data.

The task continued for 50 trials. Each trial consisted of a single ball drop, and participants could modify their estimate as they saw new events. Participants were not informed that there was any particular structure to the distribution of ball drops. Each participant observed an identical sequence of ball drops, regardless of their reported prior or trial-by-trial predictions. The sequence of ball drops followed a unimodal distribution centered over the 18’th slot with a standard deviation of 4.84 slots.

We considered two possible candidate “reference” distributions for this analysis. First, the histogram of the literal ball drop sequence given to all participants in this experiment. Second, a normal distribution with the same mean and standard deviation (stated above) as the observed sequence of ball drops (Figure 2A). To compare candidate reference distributions, we plotted aggregate learning curves, and compared final trial learning accuracy with respect to each candidate reference distribution.

We applied three distinct methods for clustering participant priors to explore the relationship between priors and learning accuracy. We adopted the first method from Shu and Wu (2011). Originally created for 2D shape matching and image retrieval, we omitted the initial steps of the algorithm that converts a binary shape image into a contour of points distribution histogram (Shu and Wu, 2011); all other steps were identical. This method emphasizes
similarity based on shape, and is insensitive to changes in scale, translation, and orientation which was important in the context of our task. That is, two participants may represent a prior in the shape of a Gaussian distribution, but do so with different bar heights. Under this method, these two participants would be considered as members of the same cluster which would not necessarily be the case with other clustering algorithms. This method produces a dissimilarity matrix using Earth Mover’s Distance (EMD) (Shu and Wu, 2011). We then performed a hierarchical cluster analysis on the resulting set of pairwise participant prior dissimilarities. We created a dendrogram using the hclust built-in R function, and cut the dendrogram branches to define our clusters using the cutreedydynamicctree R function from the Dynamic Tree Cut package (Langfelder et al., 2008).

The second clustering method was a manual classification performed by author AF. Each prior was visually classified as either a “Gaussian”, “Bimodal”, “Uniform”, “Jagged”, “Skewed”, or “Trimodal”. This method was used to explore our own subjective intuitions about the patterns we saw in the data. For both the EMD and visual clusters, we plot aggregate learning curves and compare the final trial learning accuracy across prior clusters.

Our third clustering method categorized participants in a reverse manner to the first two methods. Here, we clustered participants by their final trial learning accuracy. This requires a method that clusters participants on the basis of a single numerical value (final trial accuracy), rather than a hand drawn ball drop estimate (participant priors) which was required by our first two clustering methods. We elected to use the Jenks’ natural breaks method implemented by the GmAMisc R package (Alberti, 2020) for this purpose. We then visually explored differences in participant priors across the learning accuracy clusters.

Results and Discussion

Participant estimates are idealized and smooth, not literal representations of presented data

Participants’ final trial estimates were more similar to the normal distribution with the same mean and standard deviation as the observed ball drops ($M = 0.67$), than the literal histogram of the observed ball drops ($M = 0.55$), $t(265) = 40.98, p < .001$ (Figure 2). We also performed a two-way repeated measures ANOVA comparing participant ball drop predictions to each candidate reference distribution at the first and final trials. We found an interaction between the reference distribution and trial, $F(1,265) = 62.55, p < .001$. Therefore, the difference in similarity to each reference distribution is greater at the final trial ($M = 0.11, SD = 0.04$) than the first trial ($M = 0.08, SD = 0.05$). The additional accuracy to the smooth reference distribution is not constant. This suggests that participants approach the idealized distribution faster by smoothing trial-by-trial ball drop data and incorporating new evidence into a simplified representative model rather than accumulating literal and discrete events. It is also possible that the higher similarity to the idealized distribution is a result of drawing ball drop estimates by dragging a computer mouse or a finger along a touchscreen; a smooth continuous curve is easier to draw than jagged histogram. However this ease-of-drawing argument does not account for the relative increase in prediction accuracy in later trials for the idealized distribution.
Figure 2: A: A histogram of the 50 presented ball drops (blue) and a normal distribution with the same mean and s.d. as the literal ball drops (green). B: Average participant learning curve with respect to the smoothed normal distribution (green) and the literal ball drop histogram (blue), +/- 95% CI. C: Similarity at final trial to the smoothed normal distribution (green) and the literal ball drop histogram (blue).
Priors can be clustered by shape matching algorithms to disambiguate learning accuracy

Our exploratory hierarchical cluster analysis produced three distinct categories of participant priors: concave unimodal \((n = 97)\), bimodal \((n = 86)\), and convex unimodal \((n = 83)\) (Figure 3A). All participant priors were assigned to a cluster.

We performed a one-way ANOVA comparing participants’ final trial learning accuracy to the idealized reference distribution, grouped by EMD prior cluster. Cluster membership did not indicate a statistically significant difference in final trial accuracy, \(F(2, 263) = 2.69, p = .070\). However, the concave-unimodal cluster presents a visual and numerical separation in learning accuracy from the other two clusters (Figure 3B). Self-reported confidence in priors did not vary between EMD prior clusters \(F(2, 262) = 0.85, p = .427\).
Figure 3: A: Hierarchical clustering results in three clusters of participant priors: concave unimodal, bimodal, and convex unimodal. B: Aggregate learning curves, split by EMD clusters, +/- 95% CI.
Manually clustered priors disambiguate learning accuracy

Our subjective post hoc clustering of participant priors yielded 6 unique clusters: “Gaussian” (n = 123), “Bimodal” (n = 86), “Uniform” (n = 31), “Jagged” (n = 9), “Skewed” (n = 8), and “Trimodal” (n = 6). For our analyses, we collapsed the “Jagged”, “Skewed”, and “Trimodal” into an “Other” cluster (n = 23), resulting in 4 final clusters (Figure 4A). Of the total 266 participants in this study, 3 did not have accompanying manual cluster assignments, and were thus omitted from this analysis.

We performed a one-way ANOVA comparing participants’ final trial similarity to the idealized reference distribution, grouped by manual cluster. Final trial learning accuracy varied across cluster, $F(3, 259) = 5.81, p < .001$ (Figure 4B). Post hoc comparisons using the Tukey HSD test indicated that the mean final trial similarity for the “Gaussian” prior group ($M = 0.69, SD = 0.10$) was greater than the “Uniform” prior group ($M = 0.60, SD = 0.15$), $p < .001$. No other pairwise comparisons were significant, $ps ≥ 0.10$.

Self-reported confidence varied between manual prior clusters $F(3, 258) = 3.53, p = .015$. Post hoc comparisons using the Tukey HSD test indicated that the mean confidence rating for the “Gaussian” prior group ($M = 0.56, SD = 0.23$) was greater than that of the “Other” group ($M = 0.41, SD = 0.15$), $p = .976$. No other pairwise comparisons were significant $ps ≥ 0.27$.

It may be appropriate to treat participants in the “Other” (n = 23) prior cluster as outliers. We repeated the above analysis after excluding participants in the “Other” prior cluster. Final trial learning accuracy varied across cluster, $F(2, 237) = 8.73, p < .001$. Post hoc comparisons using the Tukey HSD test indicated that the learning accuracy for the “Gaussian” prior group ($M = 0.68, SD = 0.10$) was greater than that of the “Uniform” group ($M = 0.58, SD = 0.14$), $p < .001$. Differences between the “Gaussian” and “Bimodal” clusters ($M = 0.63, SD = 0.12$), $p = .061$, and the “Uniform” and “Bimodal” clusters, $p = .055$, were not significant, though visually apparent (Figure 4B). Self-reported confidence between manual prior clusters (excluding “Other”) showed no differences $F(2, 236) = 2.10, p = .124$. 


Figure 4: A: Manual clustering of participant priors: Gaussian, bimodal, uniform, and other. B: Aggregate learning curves, split by manual clusters, +/- 95% CI.
Distinct priors are indicated when participants are clustered by learning accuracy

Unlike the clustering methods above, we reversed our approach and clustered participants by final trial similarity to the reference distribution, not by properties of their priors. We used Jenks’ natural break method to group participants into three clusters on the basis of their final trial similarity (Figure 5). If features of participants’ prior influences learning accuracy, then differences in learning accuracy may also indicate differences in priors. The worst performing cluster (n = 29), contained participants with final trial similarities between 0.175 and 0.528. The middle performing cluster (n = 127) contained participants with final trial similarities between 0.528 and 0.697. The best performing cluster (n = 110) contained participants with final trial similarities between 0.697 and 0.891. The model’s goodness of fit was 0.774, relative to a max goodness of fit of 0.999 reached with 75 clusters.

![Figure 5](image)

Figure 5: Participant priors clustered by final trial accuracy. Clustering participants by performance indicates cluster-specific regularities: worst performing group has irregular priors, middle performing group has unimodal or bimodal priors, best performing group has mostly concave unimodal priors.

By visual inspection, each reverse cluster presents unique features. The first cluster of the worst performers presents fewer regularities than the two better performing clusters. The middle performing group contains both unimodal and bimodal priors, while the best performing group mostly contains concave unimodal priors. Self-reported confidence between reverse prior clusters showed no differences $F(2, 262) = 1.02, p = .363$.

The three presented clustering methods demonstrate the potential for future research using Plinko as a testing methodology. We have demonstrated priors vary across individuals and that some set features of a participant’s prior may indicate a participant’s success in learning a presented probability distribution. Though it is still unclear what features of a prior may be of interest. The convexity of participant drawn curves is an important feature for image recognition and categorization, as demonstrated by our EMD clustering results, but is a feature that may not appear relevant to a human rater. We encourage future research to consider adopting other computational methodologies from other disciplines to categorize participant priors, in addition to human rating.

It is unclear whether differences in self-reported confidence between prior clusters may indicate legitimacy of a given clustering method. Our presented data is also fundamentally limited in its ability to express learning accuracy across prior clusters. By nature of their priors, some individuals may begin closer to the forthcoming distribution of ball drops than others. We therefore elected to not analyze learning rate, as a low learning rate may only indicate a ‘better’ prior, rather than poorer learning. It is also unclear whether the ‘advantage’ some individuals hold by having a particular prior masks or accentuates any difference in learning ability that features of that prior may indicate, even if final trial learning accuracy is used as the measure of learning ability. We propose that future experimenters consider adapting the distribution to be learned to an individual participant’s prior distribution. This would require each participant to learn a different distribution, but one that is a consistent, fixed distance from their original estimate. Such an experiment may necessitate an assumption as to how humans measure similarity between probability distributions, but would also increase the validity of analyses of learning rate and learning accuracy.

**Experiment 2: Updating to changes**

In Experiment 1 we explored how priors may predict the learning ability of participants. In Experiment 2, we investigate participants’ ability to update their mental model of a learned probability distribution, and how experimental environments influence this ability. Previous research indicates that it is a non-trivial problem to determine when and to what extent a mental model has been updated (O’Reilly et al., 2013). In this Experiment, we detect
model updates by measuring participants’ trial-by-trial accuracy, and median ball drop estimates when learning multiple probability distributions presented in sequence. We then compared how updates to ball drop predictions are influenced by the presence of a break from the task before the ball drop distribution changes.

**Method**

We tested 39 University of Waterloo undergraduates (2 missing demographic data, 21 female, mean age = 20.07, SD = 2.06 years) to measure how participants update to changes in the presented ball drop distribution. Due to an error in data collection, the raw bar heights were not recorded - only the relative bar heights scaled to sum to 1 were available in our dataset. Raw participant slot estimates were missing for 24 of the total 39 participants. We therefore performed all analysis for this Experiment using normalized participant estimates rather than unprocessed participant estimates. Since we used the angular similarity between the representative Euclidean vectors of participant estimates and the appropriate reference distribution to define learning accuracy, our analysis is invariant to the scale of our data. Therefore, using the normalized bar heights instead of the unprocessed bar heights of participant estimates had no impact on our results.

We presented participants with four sequences of 100 ball drops (400 trials in total). Each sequence of ball drops was generated from a distinct probability distribution: 1) a wide normal distribution (\( M = \text{slot 17}, SD = 6 \) slots), 2) a narrow normal distribution (\( M = 30, SD = 2 \)), 3) a bimodal distribution made of an equal mix of two normal distributions (\( M = 9, SD = 3 \)) and (\( M = 27, SD = 3 \)), and 4) a positively skewed (Weibull) distribution (\( \alpha = 6, \beta = 1 \)) (Figure 6A). Each participant was exposed to the exact same sequence of ball drops. We assigned participants to one of two conditions. Participants in the “break” condition (\( n = 20 \)) were given a break between each ball drop distribution, but were not told the significance of the break; that is that it signaled a change in the ball drop distribution. Participants pressed the space bar to continue the task. Participants in the “continuous” condition (\( n = 19 \)) observed an identical sequence of ball drops to the “break” group, but were given no breaks between ball drop distributions.

To determine how effectively each group adjusted to each new ball drop distribution, we compared the average similarity of participant estimates at the final trial of each ball drop sequence to the relevant ball drop distribution. We also visually inspected heat maps indicating the median normalized bar heights at each trial.

**Results and Discussion**

We performed a two-way mixed ANOVA to determine whether participants’ ball drop estimate accuracy at the final trial of each 100 trial distribution varied across break conditions. We found a main effect of break condition, \( F(1,37) = 5.73, p = .022 \) and ball drop distribution, \( F(3,111) = 33.88, p < .001 \), and an interaction between the two, \( F(3,111) = 7.38, p < .001 \) (Figure 6B). Pairwise t-tests between break conditions at each ball drop distribution revealed that participants who were given a break between ball drop distributions predicted the narrow unimodal (\( p = .004 \)) and Weibull (\( p = .005 \)) distributions better than participants who got no breaks. There were no group differences in accuracy for wide unimodal (\( p = .484 \)) and bimodal (\( p = .455 \)) ball drop distributions.

Figures 6C and 6D demonstrate a “hangover” or “hysteresis” effect (Hock et al., 2005) in the continuous condition, where participant ball drop estimates of previous distributions are integrated into the next distribution. The effect is not seen in the break condition, where participants appear to treat each sequence of ball drops (separated by a break) independently. No differences in learning accuracy between the break conditions should be expected in the wide unimodal ball drop sequence, since participant experience was identical for both groups until the first distribution change. The observed “hangover” effect may also increase learning accuracy in some cases. A new ball drop distribution that is more similar to the aggregate pattern of all previous ball drops than a participant’s prior will reduce the benefit of “starting fresh” when facing a new distribution of ball drops. This may explain the lack of any difference in learning accuracy between break conditions for the third presented (bimodal) distribution.

The results from this Experiment demonstrate that humans can effectively learn multiple probability distributions presented in sequence. We have also demonstrated that the ability to update previously established beliefs can be manipulated by experimental features other than just the presented ball drop distributions. Future work using this task could explore the role priors play in updating to new ball drop distributions. For example, Plinko provides a convenient mechanism with which to test the true “unbiased” nature of a uniform prior: are participants beginning with a uniform prior (or pushed to a uniform estimate) better able to detect changes in ball drop distributions?
Figure 6: A: Participants were presented with a sequence four ball drop distributions of 100 ball drops each. B: Aggregate learning curves across the four ball drop distributions for each break condition group, +/- 95% CI. Participants who were given a break between ball drop distributions had greater learning accuracy for the Narrow Unimodal, and Weibull ball drop distributions. C: Heatmap of median ball drop estimates for participants in the break condition. Each distribution is treated independently from the last. D: Heatmap of median ball drop estimates for participants in the continuous condition. Unlike in C, participants exhibit hysteresis across ball drop distributions.
Experiment 3: The reliability of prior belief measurements

In Experiment 1, we showed that participant priors vary across individuals, but cluster around prototypical probability distributions. Further, participant success in a statistical learning task may be indicated by properties of their priors, justifying the importance of actually measuring participant priors with our task. Experiment 2 demonstrated that our task can measure mental model updating by changing the presented ball drop distribution, and that task environments can influence participants’ ability to update. In this experiment, we verify that the priors collected by Plinko are reliable and valid representations, stable within each individual and not limited to describing task-specific statistical learning.

A common approach to modeling probabilistic decision-making is to assume that all participants start a task with a homogenous prior. Participant priors are often characterized either as representing maximal uncertainty (i.e., a uniform distribution; (Harrison et al., 2006; Mars et al., 2008; Strange et al., 2005)) or as approximating the process being estimated (Griffiths and Tenenbaum, 2006; Nassar et al., 2010). Recent attempts at empirically deriving priors (Gershman, 2016; Spektor and Kellen, 2018) still require assertions about the appropriate inferential model with which to estimate participant priors from participant data. Regardless, priors are generally assumed to be equivalent across participants. The results of Experiment 1 clearly show this is not the case.

Nevertheless, there is concern that the different priors observed in Experiment 1 are not stable and instead represent noise within this experimental context. We addressed the question of prior stability in this experiment by having participants provide two separate estimates of their prior (using deception - see below). There is also concern that our task, which presents environmental contingencies with a physically plausible ‘correct’ estimation (a binomial distribution), may be principally about itself. That is, asking participants to estimate distributions of balls dropping through pegs may only inform us of how statistical learning operates in this particular task. We address this concern here by presenting participants with a ball drop distribution that changes every trial, and is defined by their previous trial estimate. This produces a task environment that is markedly different than what would be expected in a real physical game of Plinko. We also explicitly ask participants to estimate the ball drop distribution of a physical real-life Plinko game and compare this estimate to the physically plausible binomial distribution.

Method

We tested 30 University of Waterloo undergraduates (20 female, mean age = 20.30, SD = 3.22 years) to measure participant-reported prior beliefs of probabilistic estimates. To explore prior reliability, we twice asked participants to provide their prior estimate of the ball drop distribution before seeing any ball drops. Each prior was separated by a staged computer malfunction: After entering their first prior histogram and pressing the button to advance to the first trial, the screen went blank. The investigator told the participant that this was a common problem they knew how to fix. They then asked the participant to do a secondary task while they fixed the problem. The participant moved to a second computer and read a series of pronounceable non-words, each presented for one second. A large USB microphone with a glowing read LED was in front of the computer to enhance the deception. The distraction task lasted approximately two minutes. Upon completion of this deception, the participant returned to the first computer where the screen resembled the start of the experiment. The participant responded again with a “first” (now second) prior estimate before continuing with the rest of the task. Participants were debriefed at the end as to the nature of the deception, the reason for its inclusion, and were given the opportunity to rescind their permission to use their Plinko data. No participants rescinded permission.

Following collection of both priors, we asked “How confident are you that your bars reflect the likelihood that a ball will fall in any of the slots?”. We recorded confidence with a sliding scale from “Not Confident” to “Very Confident”, translating to a confidence score ranging from 0 to 1 (inclusive), where 1 is most confident. The task continued for 99 trials with no further false malfunctions. Each trial consisted of a single ball drop, and participants could, but were not required to, modify their estimates as they saw new events. Participants were not informed that there was any particular structure to the distribution of ball drops. Each ball drop was drawn from a distribution ‘opposite’ to the participant’s most recent distribution estimation. In this experiment, we cued the ‘opposite’ discrete probability distribution by subtracting the participant slot estimate from 100 (the maximum slot height) for each of the 40 slots. This opposite histogram was then scaled to contain a total area of 100 units to be a valid probability distribution from which to draw future ball drops.

After completing all 99 trials, we had participants draw a distribution, as they had for the previous experimental trials, that represented the distribution of ball drops if this were a real physical game of Plinko with a solid ball and pegs. We compared participant responses to the physically plausible binomial distribution, where the probability of a ball landing in the kth of 40 slots is \( \binom{14}{k} 0.5^k \).

To quantify the reliability of participant priors, we first established the average similarity between each partic-
ipart’s prediction of the ball drop distributions recorded before and after the distraction. We then compared this
average similarity to the means of 1000 random pairings of pre- and post- distraction priors. We also performed
a correlation analysis to compare the participants’ subjective rating of confidence in their priors to the reliability
of their reported priors. Finally, we considered the role that individuals’ understanding of the physics of the game
might play in our results by comparing the similarity of participants’ prediction of the physical Plinko ball drop
distribution to what would be expected in a physical game.

The uniform distribution becomes of particular interest under our conceptualization of ‘opposite’. It acts as
an equilibrium, since the opposite of the uniform is the uniform. If a participant predicts a uniform distribution,
the following ball drop will be drawn from a uniform distribution. If a participant predicts any non-uniform
distribution, the following ball drop will be drawn from the opposite, but similarly non-uniform, distribution.
Assuming a participant incorporates the current ball drop with previous ball drop data, a participant’s prediction of
future ball drops at trial \( n + 1 \) will be more similar to the uniform than their prediction at trial \( n \). We therefore set
the uniform distribution as the benchmark or “reference” distribution when measuring learning accuracy for this
Experiment. We plot aggregate learning curves, comparing the similarity of the participant-drawn distribution to
the reference uniform distribution at every trial. The aggregate learning curve is fitted with a Gompertz sigmoidal
growth function (Silverman, 2017). We elected to use a sigmoidal growth function instead of a standard exponential
learning curve because sigmoidal fits allow for both a slowing in learning rate as maximal accuracy is reached, and
for the maximal learning rate to occur at any point in time. The parameters of the Gompertz function,
\[ A e^{-\frac{\mu}{A} + \frac{\mu}{A} e^{-\frac{\lambda}{A}}} \]
correspond to learning properties of interest. That is, \( A \) defines the maximum similarity value
reached, \( \mu \) defines the maximum increase in similarity, and \( \lambda \) defines the trial where \( \mu \) occurs in the fitted model.

Results and Discussion

Participants present reliable prior beliefs

Figure 7A compares the mean similarity (0.50) between first and second priors when we respect participant identity
to a distribution of 1000 mean similarities of random permutations of first and second priors \((M = 0.33, SD = 0.02)\). By interpreting this distribution as a “null” distribution of chance similarity between priors, we can conclude that
two priors from the same individual are more similar than two randomly selected priors, \( p < 0.001 \). We thus
conclude that 1) participants are heterogeneous in their priors, and that 2) participant reported priors are not merely
‘noise’ as they represent something persistent and unique to the individual.

Figure 7: A: The curve is the distribution of 1000 mean similarites of random permutations of first and second
prior reports. The vertical line indicates the mean similarity for first and second priors when we respect participant
identity. Similarity is higher when a participant’s first reported prior is paired with their own second reported prior
than with some other, randomly chosen, participant’s second prior. This implies a prior has properties that are
persistent over time, and unique to the individual. B: Histogram of paired prior similairties, respecting participant
identity. The mean (vertical line) of this histogram is the vertical line of Panel A. The distribution of prior reliability
is bimodal. The lower mode is an artifact of our measure of similarity that is sensitive to lateral shifts of jagged
distributions. C: There is a positive trend between prior reliability (similarity) and self-reported confidence in prior,
+/- 95% CI.

Participant prior reliability is bimodally distributed (Figure 7B). Results of a Pearson’s product-moment corre-
lation indicates prior reproducibility may trend positively with confidence ratings, \( r(28) = 0.34, p = .068 \) (Figure
7C). Some participants have low measures of similarity between their first and second priors because their priors
are jagged, with sparsely drawn slot estimates. Two jagged distributions are more prone to unusually low similarity values than two smooth distributions. Indeed, this appears to be the case. Participants with “high” (greater than 0.5) prior similarity exhibited smooth and similarly shaped priors (Figure 8A) whereas participants with “low” (less than 0.5) prior similarity made at least one jagged first or second prior (Figure 8B).

![Figure 8: A: Participant first (yellow) and second (blue) priors with a similarity measure greater than 0.50. Most prior pairs are similar in shape and are not jagged. B: Participant first (yellow) and second (blue) priors with a similarity measure less than 0.50. Most prior pairs contain at least one jagged prior, usually the first.](image)

Our mathematical characterization of similarity is sensitive to slight shifts in sparsely drawn jagged probability distributions, resulting in low similarity ratings. Generally, when participants give a non-jagged prior, they tend to stick with it when asked to reproduce their priors. In contrast, sparsely drawn priors are unstable over time. The reason for this is unknown, but may involve the participants’ confidence in their priors (given the result above), eagerness to begin the task, or a reconsideration of task instructions or goals.

**Participant ball drop predictions approach uniform equilibrium**

Participant estimates are more similar to the theoretically expected uniform distribution at the final trial ($M = 0.70, SD = 0.20$) than at the initial trial ($M = 0.50, SD = 0.20$), $t(29) = 6.25$, $p < .001$ (Figure 9).

Participants exhibit gradual learning, integrating observed trial data continuously rather than intermittently. Figure 10 plots the aggregate data of all participants, with a fitted Gompertz growth function. Most informative are the fitted values of $\mu = 0.01$ and $\lambda = -39.74$. A small $\mu$ value indicates that aggregate participant behavior contains no sharp increases in prediction accuracy. A negative $\lambda$ implies the inflection point of the fitted model occurs before the first trial, meaning the learning rate of our participants is monotonically decreasing as they approach the uniform equilibrium.
Figure 9: A: Participant distribution estimates after the first (blue) and final (red) trials. Most participants become more similar to the uniform distribution, which is the theoretically expected equilibrium given our task construction. B: Participant estimate similarity to the theoretical equilibrium uniform distribution is greater on final trial (red) than first trial (blue).
Figure 10: Aggregate participant estimation to uniform reference distribution +/- 95% CI with fitted Gompertz growth function. The parameter $A$ defines the maximum similarity reached, $\mu$ defines maximum increase in similarity, and $\lambda$ defines the trial where $\mu$ occurs. Our fitted parameters indicate participant learning is gradual, integrating observed data continuously rather than intermittently.
The influence of true physics is negligible

Participants’ initial priors did not reflect the distribution expected if this were a physical game of Plinko. The similarity between participants’ initial prior and the physically plausible physical distribution ($M = 0.33$) is no different than the similarity between participants’ first prior and 1000 randomly paired second priors ($M = 0.33$), $t(29) = -0.06, p = .951$. This suggests that participant priors are no more similar to the physically plausible binomial distribution (had this been a physical game of Plinko) than randomly paired priors. Also, participants’ explicit estimations of the distribution of ball drops had this been a physical game of Plinko were no more accurate ($M = 0.33$) than the similarity between participants’ first prior and 1000 randomly paired second priors ($M = 0.33$), $t(29) = -0.07, p = .942$. Again, this suggests that participant estimates of a physical ball dropping through physical pegs are no more similar than randomly paired priors.

Intuitive physics also does not inhibit participants’ ability to learn physically implausible probability distributions. Participants’ ball drop estimations on the final trial were more similar to the uniform distribution ($M = 0.74$) than to the distribution expected had the game been played with a physical ball and pegs ($M = 0.31$), $t(29) = 15.17, p < .001$. This suggests that participant priors are no more similar to the physically plausible binomial distribution, and participants are able to effectively learn a distribution that is not physically plausible (the uniform distribution).

General Discussion

Here we present a novel task that provides a detailed representation of participant beliefs in a dynamic learning environment. We demonstrate the effectiveness of this task in three statistical learning experiments. In Experiment 1, we explored how participant priors indicate learning ability via three prior clustering methods. While these results are preliminary, they highlight the importance of measuring individual priors rather than assuming or retroactively inferring priors to apply uniformly to an entire group. Plinko provides a convenient avenue with which to measure individual participant priors. Matters of perceptual averaging and mental model smoothing can be examined by varying the distribution of present ball drops and the chosen “reference” distribution with which to compare participant estimates. Future work should determine whether this smoothing is merely a function of drawing probability estimates, or truly represents a function of statistical learning and mental model updating, and to what limits this function can be pushed.

Experiment 2 demonstrated that participants are able to learn and represent a number of different probability distribution types. Participants were able to update their estimates when the ball drop distribution changed at unannounced points throughout the task, and this ability to update can be manipulated though experimental features such as breaks. Plinko is therefore an effective tool to examine both the influence of participant prior beliefs on statistical learning, and how effectively mental models can be updated when faced with new contingencies, given the nature of the task’s environment. Future work using this task should explore the role participant priors play in updating to new task contingencies.

In Experiment 3, we verified that the priors collected by our tasks are reliable and meaningful, since participants reproduce a similar prior after being told their original prior was lost and completing an ostensibly unrelated task. We also verified that intuitive physics of a literal Plinko game are not represented in participant behavior, as priors did not resemble a physically plausible distribution, and participants were able to effectively learn a physically implausible distribution. This implies that our task can measure features of statistical learning that generalize beyond the stimulus-specific context of ball drops, since participants are not equally entrenched in the one ‘correct’ physically plausible prior. We suspect this is either due to a lack of knowledge of literal Plinko physics, or a suspension of literal physical expectations in this computerized task, akin to how we are not surprised by superhero flight in a video game despite holding the prior belief that humans cannot fly.

For each experiment, we used angular similarity of participants’ estimates represented as Euclidean Vectors to define learning accuracy to a reference distribution. Despite the benefits of scale invariant similarity measures, we do not commit to our selection as being the single best option. Our results from Experiment 3 demonstrate that our measure is particularly sensitive to lateral shifts, especially when comparing jagged probability distributions. This is true of any measure that assumes a literal interpretation of slot indices - something humans are unlikely to do. However, as seen in Experiment 3, remarkably jagged priors are 1) rare, and 2) may just represent a participant’s lack of confidence in any particular shape of prior. In this case, the concern of a similarity measure’s sensitivity to lateral shifts is mitigated since the most problematic use case is also the case where we are least interested in the literal shape of the participant’s provided prior.
Conceptually, our similarity measure assumes that participant estimates represent what participants were instructed to represent - that the relative heights of the bars are proportionate to the relative probabilities of where the ball will land when dropped. This is likely the safest assumption to make in the absence of any research indicating how humans express internally represented probability distributions with computer mouse-drawn or touchscreen-drawn histograms. Regardless, future users of our Plinko task can elect to circumvent these issues by restricting the total area of participant ball drop estimates (thus removing the need for theoretical motivation of estimate normalizing), or by using another measure of similarity in data analysis that is deemed more appropriate.

Ball drops are a convenient story to ascribe sequential discrete events for the purpose of measuring probabilistic updating, but our Plinko task is not limited to such a narrative. A generalized version of Plinko still affords a researcher the ability to measure individual differences in prior belief structures, and how these beliefs are influenced by new events. Ball drops may be replaced by any other sequence of discrete events that could be easily mapped to a uni-dimensional ordered spatial domain over which a histogram can be drawn. Examples include height, age, duration, color, grades, phonemes, and monetary values.

In summary, we present a novel task that provides an explicit and theory-free measurement tool of individual participant belief updating. Given the importance of statistical learning models in many areas of cognitive research, this task could be used to refine our understanding about the individual differences of priors and how the contextual elements of a task affect our ability to revise prior beliefs. By adapting cognitive models to account for these factors, Plinko can contribute to a better understanding of human learning and updating.

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Conflict of interest

The authors declare that they have no conflict of interest.

Open practices statement

The data and materials for all experiments are available at https://osf.io/dwkie. None of the experiments were preregistered.

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