

The frequentist implications of optional stopping on Bayesian hypothesis tests

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Abstract Null hypothesis significance testing (NHST) is the most commonly used statistical methodology in psychology. The probability of achieving a value as extreme or more extreme than the statistic obtained from the data is evaluated, and if it is low enough, the null hypothesis is rejected. However, because common experimental practice often clashes with the assumptions underlying NHST, these calculated probabilities are often incorrect. Most commonly, experimenters use tests that assume that sample sizes are fixed in advance of data collection but then use the data to determine when to stop; in the limit, experimenters can use data monitoring to guarantee that the null hypothesis will be rejected. Bayesian hypothesis testing (BHT) provides a solution to these ills because the stopping rule used is irrelevant to the calculation of a Bayes factor. In addition, there are strong mathematical guarantees on the frequentist properties of BHT that are comforting for researchers concerned that stopping rules could influence the Bayes factors produced. Here, we show that these guaranteed bounds have limited scope and often do not apply in psychological research. Specifically, we quantitatively demonstrate the impact of optional stopping on the resulting Bayes factors in two common situations: (1) when the truth is a combination of the hypotheses, such as in a heterogeneous population, and (2) when a hypothesis is composite—taking multiple parameter values—such as the alternative hypothesis in a t -test. We found that, for these situations, while the Bayesian interpretation remains correct regardless of the stopping rule used, the choice of stopping rule can, in some situations, greatly increase the chance of experimenters finding evidence in the direction they desire. We suggest ways to control these frequentist implications of stopping rules on BHT.

Keywords Bayesian statistics · Statistical inference · Model selection

The standard statistical methods of psychological research, such as the ANOVA and the t -test, are examples of null hypothesis significance testing (NHST). These familiar frequentist methods compare a statistic calculated from the data, such as an F -test statistic or a t -test statistic, with a sampling distribution associated with the null hypothesis. If the probability of achieving a result as extreme as or more extreme than the one obtained is lower than a predetermined threshold—the probability of making a type I error, α —then the null hypothesis is rejected.

One of the difficulties of using NHST is that the relevant details of the experimental procedure, such as the reason that the experimenter stopped collecting data, are often unknown to the reader. The reported number of participants could have been collected in a variety of ways. It could be that the number of participants was fixed in advance, here called *fixed stopping*, or the experiment was stopped in response to the data collected, here called *optional stopping*. In the most extreme case of optional stopping, which we call *preferential stopping*, researchers check the data as observations are collected and stop the experiment as soon as a significant result is acquired.

Optional stopping in these various forms is potentially quite common. In a recent survey, 58% of researchers admitted to having collected more data after looking to see whether the results were significant, and 22% admitted to stopping an experiment early because they had found the result that they were looking for (John, Loewenstein, & Prelec, 2012). Participants playing the role of an experimenter have also shown high prevalence of this type of behavior (Yu, Sprenger, Thomas, & Dougherty, 2013). There are often good reasons for optional stopping. One possible reason for optional stopping is a high cost of continuing to collect more data (e.g., in fMRI experiments or medical trials), and thus early stopping following clear results can reduce the overall costs of doing research. In contrast, the data may be noisier than

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expected, and more data may be required to observe an effect when it is present.

The statistical consequence of optional stopping is that without appropriate corrections, the reported results will often be overstated. There are various NHST methods for sequential analysis of the data (e.g., Armitage, 1960; Wald, 1947), but they are rarely used. Instead, psychological researchers almost always use NHST methods that assume that the sample size is fixed in advance of the experiment; this mismatch inflates the probability of erroneously obtaining a significant result when the null hypothesis is true. Table 1 (from Armitage, McPherson, & Rowe [1969], and used later in Jennison & Turnbull [1990], Pocock [1983], and Wagenmakers [2007]) gives a clear example of this inflation. Here, the experimenter tests whether the mean of normally distributed data with known variance is equal to zero. The data are collected in equal-sized batches, and preferential stopping is used: The test is performed after each batch is collected, and if a significant result is found, the experiment is stopped. Using a test that assumes a fixed sample size, the probability of achieving a result of $p < .05$ increases as a function of the number of times that the data are tested. Citing a p -value based on a fixed sample size after repeatedly checking data is a misrepresentation of the evidence and how it was acquired. Most disturbingly, the probability of a single experiment “rejecting the null” approaches unity with increasing numbers of tests; with NHST, experimenters who wish to reject the null hypothesis can be certain to do so if they commit themselves to collecting enough data.

Bayesian hypothesis testing (BHT) has been offered as a solution to many of the problems of NHST and, in particular, to the problem of optional stopping (Edwards, Lindman, & Savage, 1963; Francis, 2012; Kass & Raftery, 1995; Kruschke, 2010; Wagenmakers, 2007; Wagenmakers, Lodewyckx, Kuriyal, & Grasman, 2010). For example, according to Edwards et al., for Bayesian methods, “. . . the rules governing when data collection stops are irrelevant to data interpretation. It is entirely appropriate to collect data

until a point has been proven or disproven, or until the data collector runs out of time, money, or patience” (p. 193). In the same vein, Wagenmakers et al. (2010) noted that “for Bayesian hypothesis testing there is nothing wrong with gathering more data, examining these data, and then deciding whether or not to stop collecting new data—no special corrections are needed” (p. 167).

As we review below, the Bayesian interpretation of the evidence does not depend on the stopping rule used and, thus, is correct no matter the reason used to stop the experiment or whether the stopping rule is known or unknown. However, there is also the practical question of what the frequentist properties of BHT are that govern how easy it is to produce evidence for or against a hypothesis. Indeed, some researchers have investigated the frequentist properties of the standard Bayesian hypothesis test—the Bayes factor—and found mathematical bounds on how often the Bayes factor will reach certain values. As was stated by Edwards et al. (1963), “. . . if you set out to collect data until your posterior probability for a hypothesis which unknown to you is true has been reduced to .01, then 99 times out of 100 you will never make it, no matter how many data you, or your children after you, may collect” (p. 239). These bounds seem to imply that BHT actually has better frequentist properties than NHST and researchers concerned about the frequency with which convincing Bayes factors can be produced can proceed without worrying about what sort of stopping rule the experimenter used. In this article, we will demonstrate that this is not always the case.

In particular, while the above quote about the bounds is correct, it applies only to very narrow situations (Mayo & Kruse, 2001; Royall, 2000, and rejoinder), situations that rarely occur in psychological research. As a consequence, the above bound cannot always be used as a guide for the frequentist implications of using Bayes factors in psychological research, and it is possible that the probabilities of generating convincing Bayes factors are far higher than the quote implies. Even though Bayesian interpretations of Bayes factors do not depend on stopping rules, it may be possible to use optional stopping to influence both the size and direction of the evidence produced in an experiment, which could be used by researchers, inadvertently or not, to generate results that suit their purposes. We feel it is important for researchers to understand these properties, in order to have a more complete understanding of how to best use BHT in practice.

In what follows, we first provide a brief introduction to “the standard Bayesian solution to hypothesis testing” (Lewis & Raftery, 1997, p. 648)—that is, the Bayes factor and its associated Bayesian interpretation. We then present the source of the mathematical guarantee on BHT, which bounds the frequentist probability of generating Bayes factors of a given size, even with preferential stopping. We then show that this mathematical bound is based on strict assumptions that are not met in two situations common to psychology experiments.

Table 1 Probability of rejecting a true null hypothesis as a function of the number of tests

Number of Tests	Probability of Rejecting H_0
1	.05
2	.08
3	.11
4	.13
5	.14
10	.19
20	.25
50	.32
∞	1

These two cases are (1) when the truth is a combination of the hypotheses under consideration, such as with a heterogeneous population, and (2) when the true hypothesis is composite, such as the alternative hypothesis in a t -test. We investigate these cases with simulations to check how much the probabilities of generating convincing Bayes factors can be influenced by the choice of stopping rule, finding some situations in which it has a large effect and others in which the frequentist bound approximately holds. Finally, we discuss the frequentist implications of using Bayesian hypothesis tests and what techniques can be used to control the frequentist properties of these tests.

Bayesian hypothesis testing

BHT provides a way of evaluating two hypotheses by directly comparing how well they explain the data. To do so, a Bayes factor is computed, which gives a convenient and easily interpretable statistic. The Bayes factor is the likelihood ratio of two hypotheses, providing the relative probability that the data could have been produced by one hypothesis or the other. For two competing hypotheses H_1 and H_2 ,

$$BF = \frac{P(X_n|H_1)}{P(X_n|H_2)}, \quad (1)$$

where X_n are the data collected over n trials. Thus, the Bayes factor tells us how much more likely it is that the data were generated by H_1 than by H_2 . This follows from the fact that the Bayes factor is composed of a ratio of likelihood functions, each of which represents conditional probability distributions associated with how likely the observed data would be given a specific hypothesis. According to the *likelihood principle*, the likelihood function contains all the information relevant to inferences about H (Birnbaum, 1962; Hacking, 1965).

The computation of the likelihood, $P(X_n|H)$, depends on the type of hypothesis, of which there are two types. A *simple hypothesis* has no free parameters and consists of a single probability distribution defined over the possible data. For example, a coin with a fixed probability of heads (e.g., .55) is a simple hypothesis, and one can easily compute the likelihood of any combination of heads or tails, given this hypothesis. Null hypotheses can be simple hypotheses, an example of which is “the difference between two means is exactly zero.”¹

The second type of hypothesis is a *composite hypothesis* with one or more free parameters, each of which can take a

number of possible values. It consists of a set of probability distributions, with the values of the parameters determining which probability distribution is used. Alternative hypotheses are commonly composite hypotheses—for example, “the probability of heads does not equal one half” or “the difference between two means is not zero.” Computing the Bayes factor for a composite hypothesis makes use of a prior distribution over the free parameters. If θ is a continuous free parameter, the prior $P(\theta|H_1)$ indicates the belief in different values of the parameter before the experiment begins. Given this prior distribution, computing the likelihood is straightforward: The probability of the data given a hypothesis is essentially the average of the probabilities of the data over all values of θ , weighted according to their prior probabilities,

$$P(X_n|H_1) = \int_{\theta} P(X_n|\theta, H_1)P(\theta|H_1)d\theta. \quad (2)$$

After data are collected, the Bayes factor can be directly reported as the strength of the evidence in favor of one hypothesis or the other, and the Bayes factor contains all the information about the experiment needed for researchers to update their prior beliefs about the hypotheses. As we review in more detail in the [Discussion](#) section, because of the likelihood principle, the intention of the experimenter or the use of certain stopping rules is irrelevant: The data are the data, and the likelihoods are not subject to anything but the data (Berger & Wolpert, 1988; Good, 1982; Lindley, 1957). Moreover, the Bayes factor is a measure of relative evidence about two hypotheses, not an indication of how frequently a particular result would be attained given repeated experiments. The continuous nature of the evidence is perhaps confusing for researchers more familiar with the fixed p -value thresholds used in NHST. Possibly because of this, researchers have developed qualitative labels indicating the strength of the evidence for different thresholds of Bayes factors. In what follows, we use one set of three thresholds, 3:1, 10:1, and 100:1, which have been respectively described as “substantial,” “strong,” and “decisive” evidence in favor of one hypothesis over another (Wetzels et al., 2011), although the somewhat arbitrary nature of label assignment has resulted in other, slightly different schemes (Jeffreys, 1961; Kass & Raftery, 1995; Royall, 1997).

Optional stopping and the universal bound

A often-cited frequentist guarantee for Bayes factors is that there is a bound on how often any stopping rule, including preferential stopping, can result in evidence that supports a false hypothesis over the true hypothesis (Birnbaum, 1962; Kerridge, 1963; Robbins, 1970; C. Smith, 1953). Because of its generality, this is known as the *universal bound* (Royall,

¹ This example is only a simple hypothesis when the variance is known. If the variance is unknown, the null hypothesis is referred to as a *point hypothesis*, because it is a composite hypothesis in which the difference between the means is fixed at a particular point. This difference becomes relevant in our investigation of the Bayesian t -test.

2000). Let us assume that an experimenter preferentially stops the experiment as soon as the Bayes factor reaches the value k in support of his or her favored hypothesis, where $k > 1$, or otherwise continues until he or she runs out of resources. If the experimenter is seeking evidence for the false hypothesis over the true hypothesis, the probability that he or she will be successful, no matter how long he or she continues, has a maximum upper limit,

$$p\left(\frac{P(X_n|F)}{P(X_n|T)} > k\right) \leq \frac{1}{k}, \quad (3)$$

where F is the false hypothesis and T is the true, or data-generating, hypothesis. Thus, if an experimenter aimed to find 10:1 evidence favoring a false hypothesis over a true hypothesis, in only 10% of all experiments could preferential stopping achieve this goal. This is certainly an advantage over NHST (see Table 1), for which the chances are 100%.

The proof of the universal bound is reproduced in Appendix 1, and it holds for any stopping rule. It is the basis for the quote by Edwards et al. (1963) in the introduction, stating that no matter how long an experiment is run and no matter what stopping rule is used, there is a high probability that strong evidence for the false hypothesis can never be obtained. Even for the weakest criterion we consider, 3:1, there is only a one-third chance that the experimenter will ever be successful in stopping with evidence against a true hypothesis. This bound on the frequency of obtaining particular Bayes factors is a powerful tool for researchers interested in controlling the frequentist implications of BHT.

Importantly, however, this bound is based on very specific assumptions. The first is that one of the hypotheses is true. The second is that the true hypothesis is a simple hypothesis. As we will show, these assumptions are rarely met in psychology experiments. Indeed, the limitations on the universal bound to tests between simple hypotheses has been noted before in the statistics literature (Mayo & Kruse, 2001; Royall, 2000, and rejoinder), but here we investigate the quantitative consequences of the limitations, as well as go into greater depth as to where the universal bound remains a good guide. In the following two sections, we explore the consequences of preferential stopping when the universal bound's assumptions are not satisfied. In both sections, we demonstrate the violation first with a coin-flipping example and then with a more practically relevant example from the psychological literature.

When the truth is a combination of the hypotheses

Researchers often propose hypotheses that reflect a particular theoretical outlook. For example, researchers might believe that people are using heuristic or rational models of decision

making (Gigerenzer & Todd, 1999; Lee & Cummins, 2004, or perhaps they are using an exemplar or prototype representation to generalize their experiences (Medin & Schaffer, 1978; Nosofsky, 1986; Reed, 1972). These hypotheses are tested against one another to explain empirical data, but it is commonly understood that in describing such a complex system as the mind, no theory is ever completely true (Nester, 1996). It could well be that the population is heterogeneous and that some hypotheses describe some individuals but not others. Indeed, it has been found that some individuals are better described by heuristics and others by more compensatory decision strategies (Lee & Cummins, 2004) and that some individuals are better described by a prototype model and others by an exemplar model (Bartlema, Lee, Wetzels, & Vanpaemel, 2013). Heterogeneity can occur even within an individual: People can use different strategies at different times (e.g., Rieskamp & Hoffrage, 2008). Researchers often test “pure” hypotheses, while the true generating model might be a mixture of the hypotheses under consideration: For some trials or individuals, one hypothesis is true, and for the remaining trials or individuals, the other hypothesis is true.

A related possibility is that the true generating model is actually an integrated combination of both of the hypotheses under consideration. One relevant example is human estimation of the joint probability of two independent events. The normative rule is to multiply the probabilities of the independent events together, but some researchers have instead proposed that people respond with the weighted average of the probabilities of the two independent events (Fantino, Kulik, Stolarz-Fantino, & Wright, 1997; Nilsson, Winman, Juslin, & Hansson, 2009). The normative rule and the weighted average rule are the usual hypotheses that are compared in these experiments. However, other researchers have found that the best-fitting model is an average of these two hypotheses (Jarvstad & Hahn, 2011; Wyer, 1976). In general, the behavior produced could well be a weighted average of the behavior predicted by each of the hypotheses; that is, the data-generating hypothesis may be an integrated combination of the two hypotheses considered.

Here, we evaluate the probability of reaching particular Bayes factors when neither hypothesis describes the data well alone and, instead, the truth is a combination of the hypotheses. In particular, we are interested in examining the consequences of preferential stopping. Our examples below are framed in terms of an integrated combination of hypotheses, meaning a weighted average of the predictions of each of the hypotheses. However, if the data are binary and binomially distributed, a mixture of the hypotheses and a weighted average of the hypotheses are mathematically indistinguishable (shown in Appendix 2). Thus, our simulations apply to both situations described above. We begin with a toy example based on coin flipping to demonstrate the general principle, and then we move to a more realistic example based on forgetting functions.

Deciding between two biased coins

Our first example will test between two specific hypotheses about the bias of a coin and investigate the frequency of achieving Bayes factor thresholds for a range of different biases that are not necessarily equal to the hypotheses that are considered. One of the hypotheses, C_H , is that the coin is biased toward producing heads, with $P(H | C_H) = .75$. The other hypothesis, C_T , is that the coin is equally biased toward producing tails, where $P(H | C_T) = .25$. Given that the experimenter independently flips a coin n times, the probability of s heads for either of these coins follows a binomial distribution,

$$P(s|n) = \binom{n}{s} P(H)^s (1-P(H))^{n-s}. \quad (4)$$

Suppose that data are generated such that each hypothesis is a combination of the hypotheses, meaning either that one of the two coins from the hypotheses is selected randomly with some probability on each trial or (equivalently, as we have shown) that a single coin with a bias that lies between the two hypotheses is flipped on every trial. Let w indicate the weight for each hypothesis, with the probability of heads in the data being $P(H) = wP(H|C_H) + (1-w)P(H|C_T)$. Figure 1a shows how the Bayes factors change as the data are collected for three sample experiments, with each path showing the trial-by-trial value of the Bayes factor as the heads and tails are accumulated. These paths are shown relative to the thresholds for substantial, strong, and decisive evidence for each hypothesis. We show two cases: one for which C_H is true [$P(H) = .75$] and one for which $w = .5$ (the two hypotheses are equally weighted, and the coin is fair). When C_H is true and $P(H) = .75$, the assumptions of the universal bound hold. The Bayes factor for each example

experiment drifts rapidly toward dominance for the true hypothesis, and only one third of the sample experiments exceed the 3:1 threshold for the false hypothesis. This example illustrates the principle of *consistency*: As the amount of data increases, the Bayes factor converges on the correct hypothesis.

For $w = .5$ and $P(H) = .5$ in Fig. 1b, the evidence lies exactly between the two hypotheses, on average, and for these two models, neither of which generated the data (or includes a component that generated the data), the consistency principle no longer holds. Here, the Bayes factors in the sample experiments do not remain close to the indifference line of 1:1 but, instead, swing wildly as the data accumulate: The Bayes factor for a single experiment can, at times, favor one hypothesis decisively and later favor the other decisively. Because neither model exactly generated the data, the universal bound also no longer holds. These sample experiments illustrate the problem when truth is a combination of both hypotheses: Preferential stopping would mean that the experimenter could find decisive evidence for his or her favored hypothesis.

Although we will discuss how the universal bound does not hold for composite hypotheses in a subsequent section, to avoid readers being left with the impression that the inconsistent behavior for the simple hypotheses in Fig. 1b is characteristic of BHT more generally, we show here that for composite hypotheses that both include $P(H) = .5$ as an equally likely component (as shown in Fig. 1c), the consistency principle does hold, although the universal bound does not. Bayes factors are generally consistent if the true model is a component of one or more of the hypotheses considered (Breiman, LeCam, & Schwartz, 1964; Doob, 1949), or even if the data are closer to one of the hypotheses (Bernardo & Smith, 1994). However, even this is not always the case; Diaconis and Freedman (1986) describe how nonparametric priors can overwhelm the data and cause inconsistency.

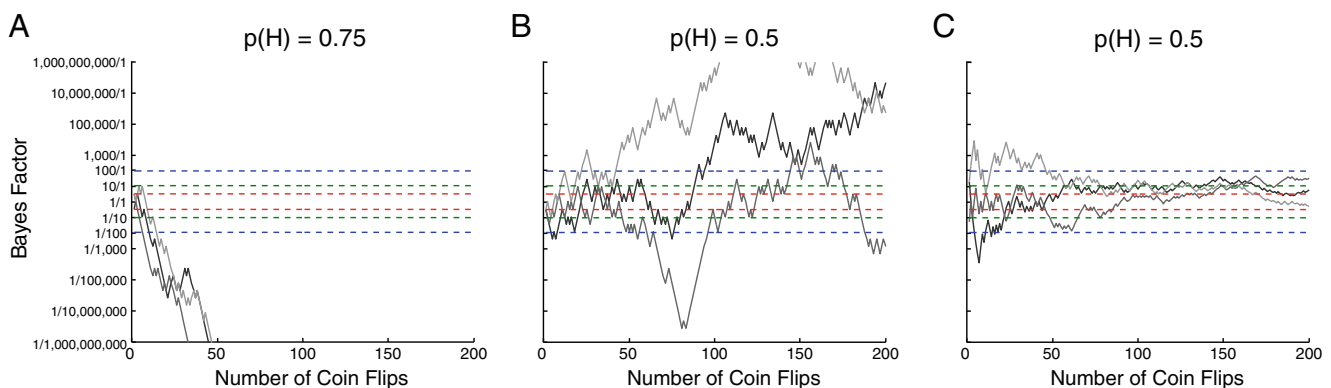


Fig. 1 Changes in the Bayes factor as data are accumulated in example coin-flipping experiments. In each plot, the three gray lines represent the Bayes factors in three example experiments. Values above one indicate evidence that favors the biased hypothesis C_T , and values below one favor the biased hypothesis C_H . The dashed lines indicate standard Bayes factor thresholds for substantial (3:1 in red), strong (10:1 in green), and

decisive (100:1 in blue) evidence. a The probability of heads being generated is .75 (C_H is true), where $P(H | C_H) = .75$ and $P(H | C_T) = .25$. b The probability of heads being generated is .5 (C_H and C_T are equally true). c The probability of heads being generated is again .5, but two composite hypotheses that equally weight $P(H) = .5$, C_H with $P(H|C_H) \sim \text{Beta}(20,1)$ and C_T with $P(H|C_T) \sim \text{Beta}(1,20)$, are tested

Returning to our two simple hypotheses, we investigated the probability of achieving Bayes factor thresholds systematically by imagining two experimenters: one who prefers C_H and stops the experiment if the Bayes factor favors C_H by 10:1, and the other who prefers C_T and stops if the Bayes factor favors C_T by 10:1. Assume that our experimenters are willing to perform up to 100,000 coin flips, stopping the experiment as soon as the favored hypothesis reaches a Bayes factor of 10:1 but continuing otherwise. Because an experimenter can stop as soon as the threshold is reached, in many cases, the experimenter will not need to collect all 100,000 trials.

Figure 2a presents the probability of success for our experimenters within 100,000 trials when C_H is true and $P(H) = .75$. Here, the probability of success for the experimenter who favors the true hypothesis is near one. Alternatively, for the experimenter who favors the false hypothesis, the probability of success is quite low ($<.1$). To see the advantage of preferential stopping, we compare these results with those of experimenters who choose the number of trials in advance of the experiment (fixed stopping rule). While preferential stopping does increase the probability of success for each of the experimenters over fixed stopping, the probability of success for the false hypothesis is far below the universal bound.

However, if each hypothesis is equally weighted, as in Fig. 2b with $w = .5$, the results change considerably. First, each of the experimenters who preferentially stop has a very high probability of success, with either experimenter able to claim strong evidence for the preferred hypothesis with near certainty within 100,000 trials. Second, even using a fixed stopping rule is reasonably likely to result in evidence that strongly supports one hypothesis or the other, although the

direction of the evidence is not determined in advance, as it is for preferential stopping. The effect of preferential stopping can be seen as the difference between fixed and preferential stopping, which is here quite large. This is a striking departure from the case in which one of the hypotheses generated the data and the universal bound applies.

The high probability of generating the desired evidence in favor of a chosen hypothesis is not limited to cases in which the two hypotheses are equally weighted. Figure 3a generalizes the findings of Fig. 2, showing the probability of success for our two experimenters using preferential stopping, for various values of $P(H)$ that range between C_H being true [$P(H) = .75$] and C_T being true [$P(H) = .25$] and for our three threshold criteria. The universal bounds hold when one of the hypotheses is true, but the probability of generating the desired evidence rises sharply as $P(H)$ approaches .5. Critically, even when the data-generating hypothesis is closer to one hypothesis, evidence that supports the other hypothesis can be generated with higher probability than one would expect if using the universal bound as a guide. For example, looking at the case of $P(H) = .55$, for which the evidence is slightly in favor of C_H , the experimenter who favors hypothesis C_T will be successful more than half of the time for the strong evidence (10:1) threshold.

In many areas of psychology, researchers would not be able to collect 100,000 samples and would also not be able to report a very small sample. It could be that these Bayes factor thresholds are achieved mainly with either very large or very small sample sizes and, thus, these results would not be practically relevant to most researchers in psychology. Unfortunately, this is not the case. Figure 3b shows that collecting data over a range more typical of psychological

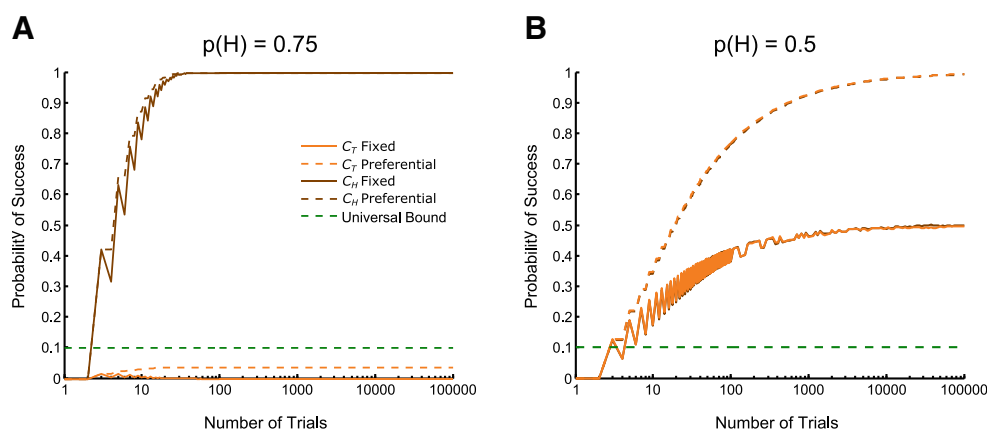


Fig. 2 The probability of successfully using preferential stopping to support a hypothesis as the number of trials increases from 1 to 100,000, using a Bayes factor threshold of strong evidence (10:1). For the fixed stopping rule, the threshold is checked only at that particular trial number. For the preferential stopping rule, the probability of success is the proportion of experiments that have reached the threshold in favor of the preferred hypothesis at that particular trial or earlier. The dashed green

represents the universal bound for the criterion. The jaggedness of the plots is mainly due to the discrete nature of the data, but above 100 trials not every trial is plotted, which smooths the plot. **a** The probability of generating heads is .75 (C_H is true). **b** The probability of generating heads is .5 (C_H and C_T make equal contributions). The brown and orange lines lie on top of one another in this plot

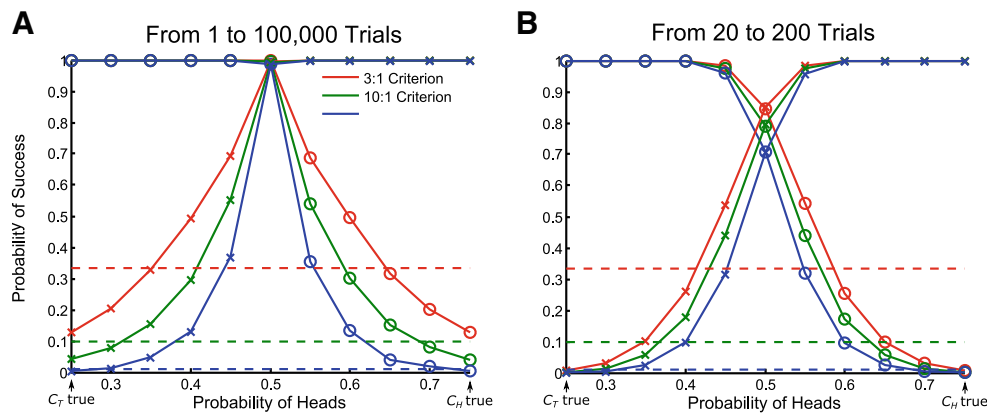


Fig. 3 The probability of successfully using preferential stopping to find evidence for C_H (x) or evidence for C_T (o) given a specific true probability of heads and a specified Bayes factor threshold. The dashed lines are the universal bounds for each threshold. **a** Probability of success

when checking the Bayes factor after every trial from the first to the 100,000th trial. **b** Probability of success when checking the Bayes factor after every trial from the 20th until the 200th

research, between 20 and 200 samples, reduces the probability but does not reduce it below the values given by the universal bound. Our experimenters can still find decisive evidence (100:1) through preferential stopping for their favorite hypothesis more than half of the time, even if both hypotheses are equally weighted.

Exponential or power law models of memory decay

The above case is artificial by design, demonstrating in a “toy” environment that preferential stopping can greatly change the probability of achieving desired Bayes factors. Here, we extend these findings to a problem more firmly in the domain of psychology—specifically, the mathematical description of memory. Establishing the mathematical form of forgetting (i.e., the forgetting curve) involves relating the probability of memory retention, R , to the time interval between study and test, t . The specific mathematical form of the relationship between R and t was investigated more than 100 years ago by Ebbinghaus (1974) but remains an open question in memory research, with researchers comparing exponential and power law forms of memory decay (e.g., Wixted & Ebbesen, 1991), among other forms. Recent elegant work by Averell and Heathcote (2011) revisits this question, using hierarchical Bayesian model selection to compare exponential decay and power law decay, finding evidence for power law decay. In the present analysis, we artificially place the truth somewhere between exponential and power law decay models. We have no particular reason to suspect that this reflects reality, but the example of forgetting functions suits our purpose by producing binary data (recalled or not recalled) and for having the tradition of testing various models against one another.

Specifically, we assume that the properties of forgetting are driven by a combination of both exponential and power law

processes. Following Averell and Heathcote (2011), we let the probability of retention for exponential forgetting be

$$p_e(t) = a + (1-a)be^{-\alpha t} \quad (5)$$

and for power law forgetting

$$p_p(t) = a + (1-a)\frac{b}{(1+t)^\beta}, \quad (6)$$

where for both models t is the time delay, b is the maximum probability of recall, and a is the minimum probability of recall.

We further assume that the observed probability of forgetting, for a given set of experiments, is a weighted function of the above two forgetting functions,

$$R(t) = wp_e(t) + (1-w)p_p(t). \quad (7)$$

To simulate a typical study of this kind, we let each participant be tested for recall at five different time points ($t = 1, 2, 3, 4, 5$). The number of items recalled at each time point was generated as the outcome of a random binomial sample with 20 possible items to recall, where the probability of recall for each item was equal to $R(t)$, from Equation 7. We let each participant have the same underlying forgetting functions, fixing the parameters at $a = 0.2$, $b = 0.8$, $\alpha = .5$, and $\beta = 1$. We then simulated the outcomes of the two different experimenters as before, with one favoring the exponential hypothesis and the other favoring the power law hypothesis. The two experimenters each collect independent data, first evaluating the Bayes factor after collecting 20 participants. From there forward, they evaluate the resulting Bayes factor after each participant until they reach the desired threshold Bayes factor or until they have sampled 200 participants.

Figure 4 presents the probability of success for each of the experimenters, for different values of w between 0 and 1. Notably, for values of w that are 0 or 1 and where one of the hypotheses generates the data, the universal bound applies. However, for cases near $w = .5$ where neither hypothesis generates the data, in more than half of experiments, preferential stopping can produce Bayes factors of up to 100:1 in support of whichever hypothesis the experimenter favors.

Once again, preferential stopping plays a considerable role in creating such favorable outcomes for our experimenters. As can be seen for the case of $w = .5$, presented in Fig. 5a, the results for fixed stopping lie considerably above the bound, with “strong” evidence for each of the hypotheses more than a third of the time. However, including preferential stopping nearly doubles an experimenter’s chances of successfully finding support for the favored hypothesis. Even if the data slightly favor the exponential hypothesis ($w = .6$, presented in Fig. 5b), the chance of finding strong evidence for the power law hypothesis exceeds 10% for small numbers of trials and is greatly increased by preferential stopping.

Although the present case is still artificial in many respects (e.g., using simple hypotheses, meaning that each parameter is fixed), assuming more realistic scenarios—such as participants with different-shaped forgetting functions and performing a hierarchical Bayesian analysis to account for heterogeneous behavior—does not remove the problem. Indeed, introducing free parameters (i.e., composite hypotheses) also violates the assumptions used to derive the universal bound. However, before we examine the consequences of introducing such composite hypothesis, we first briefly discuss the bounds on generating evidence when the truth is a combination of the hypotheses.

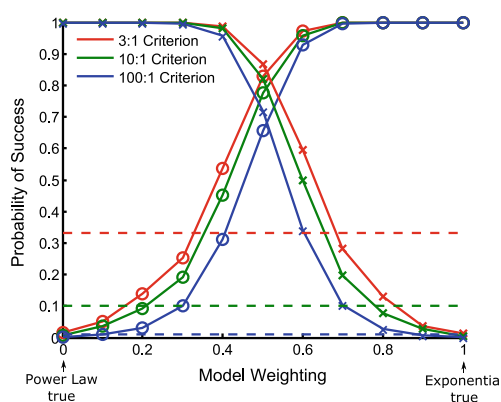


Fig. 4 Probability of finding threshold-level Bayes factor using preferential stopping in favor of either the exponential (o) or the power law (x) hypothesis, for varying contributions of each of the two models to the generating data (w). Data are checked after each trial from the 20th and 200th trials. Dashed lines are the universal bounds for each criterion

Bounds on generating evidence when the truth is a combination of the hypotheses

One of the compelling problems with NHST is that preferential stopping is eventually certain to result in rejection of the null hypothesis if enough data are collected (see Table 1). Figure 3a shows that when C_H and C_T contribute equally to the data, BHT can lead to similar results: There is a near certainty that strong Bayes factors in favor of either hypothesis can be generated by preferential stopping.

We can obtain a slightly more general result by relating our Bayes factor for C_H versus C_T to a random walk. Combining Equations 1 and 4 with our two hypotheses $P(H | C_H) = .75$ and $P(H | C_T) = .25$, we can derive the log Bayes factor,

$$\log BF = \log(3)(s - (n - s)), \quad (8)$$

which is simply the number of heads minus the number of tails multiplied by a constant. This represents an unbiased random walk, with step lengths equal to the constant $\log(3)$. When collecting infinite data, an unbiased random walk will eventually cross every threshold an infinite number of times (Feller, 1968, p. 360). For our situation, this means that if C_H and C_T contribute equally to the data-generating hypothesis, any Bayes factor threshold is achievable in a single (potentially very long) experiment.

While the result is clear for the case of C_H and C_T and both hypotheses make equal contributions, deviations from this simple situation mean that the answer is more complex. One deviation is when the probabilities of the two hypotheses are not symmetric around .5, as is the case for the forgetting functions. Here, we cannot construct such a simple random walk. A second deviation is when neither hypothesis generated the data but one hypothesis is closer to the data-generating process than the other. Here, the Bayes factor will eventually converge to support the closer hypothesis (Bernardo & Smith, 1994, p. 287), so it is likely that the probability of generating evidence is governed by another, unknown bound. This is an area for future research.

When the truth is part of a composite hypothesis

In many sciences, including psychology, our hypotheses are not very precise. The most salient examples of this are the alternative hypotheses of NHST methods such as t -tests or ANOVAs. These alternative hypotheses state that there is some nonzero difference between conditions, without specifying precisely what the size of the difference might be. Because these alternative hypotheses are an aggregation of multiple possible states of the world, they are composite hypotheses.

Bayesian alternatives to these NHST tests often take the form of a comparison between nested models (Rouder, Speckman, Sun, Morey, & Iverson, 2009; Wagenmakers et al., 2010).

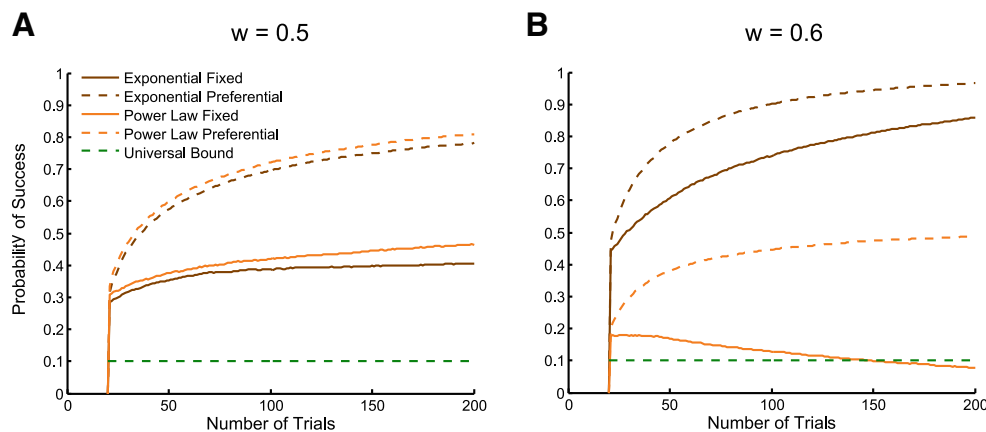


Fig. 5 The probability of successfully using preferential stopping to support the hypothesis as the number of trials increases from 20 to 200, using a Bayes factor threshold of strong evidence (10:1). For the fixed stopping rule, the threshold is checked only at that particular trial number. For the preferential stopping rule, the probability of success is the proportion of experiments that have reached the threshold in favor of the

preferred hypothesis at that particular trial or earlier. The *dashed green* represents the universal bound for the criterion. **a** The exponential and power law models equally contribute to the generated data ($w = .5$). **b** The exponential model contributes slightly more than the power law model to the generated data ($w = .6$)

Nested models are used to determine whether a parameter is necessary by comparing a null hypothesis that assumes that the parameter is fixed at a particular value (often zero) with an alternative hypothesis that allows the parameter to vary. As in the NHST cases above, this alternative hypothesis is always a composite hypothesis. We examine two examples of nested models in this section. The first is a coin-flipping example in which we compare the hypothesis that the coin is fair with the hypothesis that the coin has an unknown bias. The second example investigates the properties of the Bayesian t -test, in which the hypothesis that the effect size is zero is compared with the hypothesis that the effect size is nonzero but unknown. Note that unlike in the previous section, in which we investigated a situation in which the truth was a combination of the hypotheses, in the following two cases, a component of one of the two hypotheses always generates the data. However, the universal bound is violated as a result of employing composite hypotheses.

Fair and biased coins

A simple example of comparing nested hypotheses is comparing the hypothesis that a coin is fair, with $P(H) = .5$, with the hypothesis that it is biased. The biased coin hypothesis is a composite hypothesis, with $P(H)$ unknown and allowed to range between zero and one, and we assume that each of these values is equally probable before the start of the experiment. To compute the Bayes factor in this case, we need to compute the likelihood of the data given each of the hypotheses. The likelihood of the data for the fair coin is the same as the likelihood that we used in Equation 4 with $P(H) = .5$. For the biased coin, each value of $P(H)$ produces a different likelihood, and to find a single marginal likelihood value,

we integrate over the likelihood of the data from each of the possible values of $P(H)$ as in Equation 2. As in our previous examples, we simulated the results of two different experimenters who preferentially stop, one favoring the biased coin hypothesis and the other favoring the fair coin hypothesis.

The probabilities of success of the two experimenters are shown in Fig. 6a for the case in which $P(H) = .5$ and the experimenters are willing to perform up to 100,000 coin flips. The results are reassuring in that the experimenter favoring the fair coin is nearly guaranteed to find strong evidence (10:1) for the fair coin hypothesis. On the other hand, the efforts of the experimenter favoring the biased coin are met dominantly with failure and, even with preferential stopping, are clearly limited by the universal bound. This is an important case for which the universal bound holds, because (as is illustrated in the figure) if the experimenter favoring the alternative hypothesis uses p -values ($p < .01$), the probability of success continues to grow with additional trials and will eventually, as in Table 1, reach unity.

Figure 6b shows comparative results for the case in which the coin is slightly biased, $P(H) = .55$, and now we find that results are substantially different. Although the experimenter favoring the true biased coin hypothesis can eventually find strong evidence (10:1) with near certainty, preferential stopping has a considerable effect on the probability of success for the experimenter favoring the fair coin hypothesis. Even more surprisingly, by allowing up to 200 coin flips, the experimenter favoring the fair coin hypothesis and using preferential stopping has a higher probability of success than the experimenter favoring the correct hypothesis that the coin is biased. The universal bound no longer applies for the false “null” hypothesis, and, indeed, even use of a fixed stopping rule produces a probability of success that exceeds our expectations from this bound.

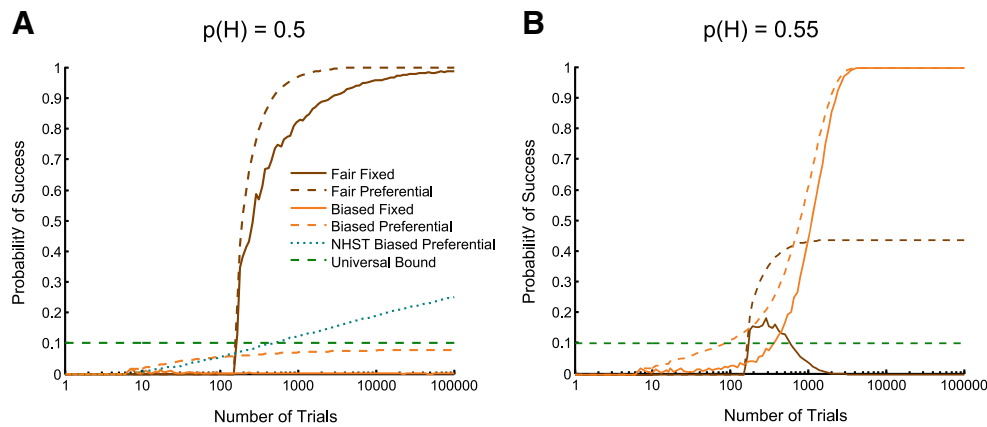


Fig. 6 The probability of successfully using preferential stopping to support a hypothesis as the number of trials increases from 1 to 100,000, using a Bayes factor threshold of strong evidence (10:1). For the fixed stopping rule, the threshold is checked only at that particular trial number. For the preferential stopping rule, the probability of success is the proportion of experiments that have reached the threshold in favor of the

preferred hypothesis at that particular trial or earlier. The *dashed green* represents the universal bound for the criterion. The jaggedness of the plots is mainly due to the discrete nature of the data, although at larger trial numbers, not every trial is plotted, which smooths the plot. **a** The probability of generating heads is .5 (the coin is fair). **b** The probability of generating heads is .55 (the coin is biased)

This nonlinear behavior in support of the null hypothesis requires additional comment. When the true state of the world is a slightly biased coin, for low numbers of trials, it is impossible to produce strong evidence for the fair coin hypothesis, and it is more likely that strong evidence for the biased coin hypothesis can be found. For an intermediate number of trials, it becomes possible to produce strong evidence for the fair coin hypothesis, and for this range, this false hypothesis is more likely to be supported than the alternative hypothesis. It is not until a very large number of trials have been collected that it is almost certain that strong evidence will be found for the correct biased coin hypothesis. The favoring of the null hypothesis at intermediate trials has been pointed out by other researchers (Atkinson, 1978; Matthews, 2011; Rouder et al., 2009; A. Smith & Spiegelhalter, 1980), and we consider it further later in this section and in the Discussion section.

Now consider the probability of success for our experiments over a range of $P(H)$ values from truly fair, $P(H) = .5$, to completely biased, $P(H) = 1$. Here, we look at a more limited range of sample sizes from 20 to 200 and present results for all three criteria (see Fig. 7a). As in Fig. 6a, the bounds hold when the null hypothesis generates the data. However, when the null hypothesis is false, the experimenter favoring the null hypothesis can find substantial evidence (3:1) in support of the null in excess of the universal bound with coins that have a bias of up to $P(H) = .7$. For comparison, we also provide comparable p -values in Fig. 7b. Again, we can see that when the null hypothesis generated the data, the probabilities of success exceed the true p -values. For slightly biased coins, the number of trials is not large enough for either BHT or NHST to often find evidence that the coin is biased.

Although the results again show violations of expectations based on the universal bound, they also demonstrate another interesting property of BHT. Despite the apparent favorable outcome for the experimenter favoring the fair coin hypothesis when the coin is in fact fair, it can often take many trials before strong evidence can be found for the fair coin hypothesis. For example, Fig. 7 shows that even the experimenter who preferentially supports the null hypothesis cannot find decisive evidence (100:1) within 20–200 trials. Indeed, sometimes evidence for the fair coin hypothesis will lag behind evidence for the biased coin hypothesis. In Fig. 6, when the coin is fair, it is possible to find strong evidence (10:1) for the biased coin within 10 trials, while it takes more than 150 trials before strong evidence can be found for the fair coin hypothesis. This result comes about because, until a large number of trials have been observed, there is no number of heads that produces strong evidence in favor of the fair coin hypothesis, while there are many possibilities (unlikely as they may be) that can produce strong evidence in favor of the biased coin hypothesis.²

Bayesian t -tests

We now extend the results of fair and biased coins to the more practically relevant case of the t -test. The t -test is a basic statistical tool in psychology and is one of the first inferential methods taught to psychology students. As an alternative to the NHST t -test, a default Bayesian t -test has been developed

² Johnson and Rossell (2010) noted the slow accumulation of evidence for the null hypothesis and proposed priors for the more complex hypothesis that assign a vanishingly small probability to the parameter that corresponds to the less complex hypothesis.

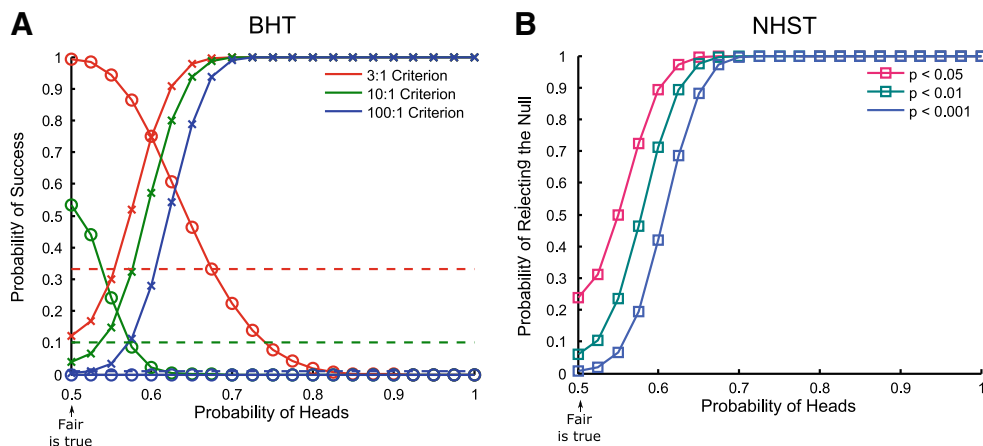


Fig. 7 The probability of successfully supporting either the fair coin or the biased coin hypothesis with preferential stopping between the 20th and 200th trials. The fair coin hypothesis is true only if $P(H) = .5$. **a** The probability of using BHT to successfully support the biased coin hypothesis (x) or support the fair coin hypothesis (o) given a specific true

probability of heads and a specified Bayes factor threshold. The *dashed lines* are the universal bounds for each threshold. **b** The probability of using NHST to successfully support the biased coin (*squares*) given a specific true probability of heads and a specified NHST threshold

by Rouder et al. (2009) and has recently been put in practice to compute Bayes factors for a wide range of previously published studies in psychology (Wetzels et al., 2011). This Bayesian t -test uses the same null hypothesis as the NHST t -test, an effect size of zero ($\delta = 0$), but instead of merely specifying a nonzero effect size ($\delta \neq 0$) as the composite alternative hypothesis, a prior distribution is placed on the possible effect sizes. This allows researchers to compare the relative evidence for the alternative hypothesis against the null hypothesis and, unlike for NHST, allows researchers to quantify support for the null hypothesis, using Bayes factors.

To investigate the consequences for preferential stopping for this case, we again assume two experimenters, one interested in finding support for the null hypothesis and the other interested in finding support for the alternative hypothesis. We assume that the experimenters take a reasonable number of sequential samples (from 20 to 200) from a population with a particular mean, μ , and a standard deviation equal to one, such that the effect size, δ , is equal to the mean, μ . Then we compute the Bayes factor using the Bayesian t -test (Equation 1 from Rouder et al., 2009). Experimenters stop if, after any sample, the Bayes factor exceeds their preferred criterion threshold.

Figure 8 shows the probability that the experimenters could find various levels of support for their favored hypothesis over a range of effect sizes. These effect sizes, like the thresholds for the Bayes factor, have been assigned labels associated with particular subjective values: $\delta = 0.2$ is a considered a “small” effect size, $\delta = 0.5$ is a “medium” effect size, and $\delta = 0.8$ is a “large” effect size (Cohen, 1988). These labels ground the results of the simulations so that the probabilities of success can be associated with psychological effects for which the effect sizes are known. The same pattern of results was found

as in the fair versus biased coin example; evidence for the alternative gradually increases with effect size but, reassuringly, lies below the universal bounds when the null hypothesis generated the data (see Fig. 8a).

This is an interesting result, both because it demonstrates where the probability of success appears bounded for BHT, but not for NHST, and because the null hypothesis is not simple, as it was in the fair coin case. Instead, it is a particular kind of composite hypothesis, known as a point hypothesis, which fixes the effect size but allows the standard deviation to vary. This point null hypothesis becomes very similar to the simple hypothesis that generated the data after a number of trials, because the point null hypothesis is a t distribution, which, after a few hundred trials, will be hard to distinguish from the data-generating Gaussian distribution. Here, this unknown parameter has little effect; evidence against the null hypothesis remains below expectations based on the universal bound.

Once again, however, as δ rises above 0, the results in Fig. 8a indicate that evidence for the null hypothesis can be above expectations based on the universal bound even when the effect size is nonzero. The probability of rejecting the null hypothesis using NHST is shown in Fig. 8b as a comparison. We can also observe the nonmonotonic mean path of Bayes factor in Fig. 9, which shows how, with a small effect size, the Bayes factor initially favors the null hypothesis and later begins to favor the alternative hypothesis, an effect we explain in the next section and in the Discussion section.

Figure 10 compares fixed and preferential stopping for data generated from the null hypothesis, $\delta = 0$, and using small effect sizes, $\delta = 0.2$, echoing the results of Fig. 6. For data generated from the null hypothesis (Fig. 10a), BHT is well behaved, with the caveat that more than 50 trials are needed

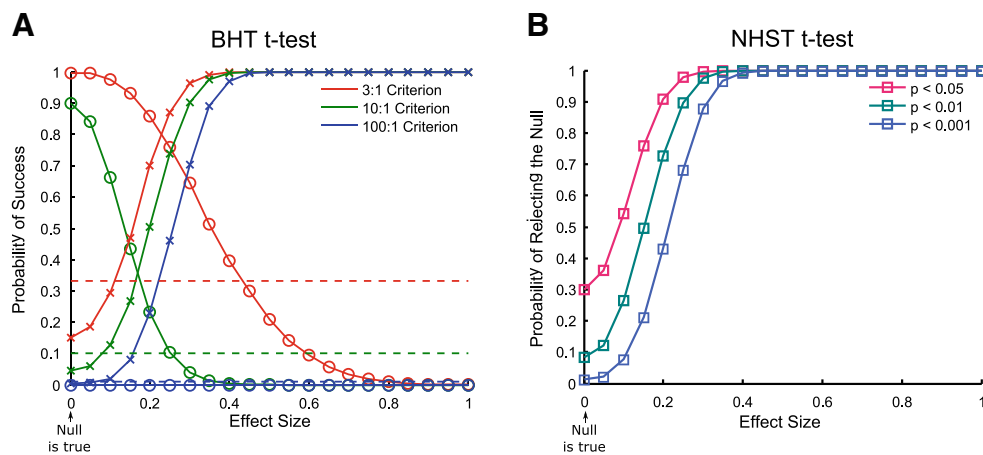


Fig. 8 The probability of successfully supporting either the null or the alternative hypothesis with preferential stopping between the 20th and 200th trials. The null hypothesis is an effect size of zero ($\delta = 0$). **a** The probability of using the Bayesian t -test to successfully support the alternative hypothesis (x) or support the null hypothesis (o) given a specific

data-generating effect size and a specified Bayes factor threshold. The *dashed lines* are the universal bounds for each threshold. **b** The probability of using NHST to successfully support the alternative hypothesis (*squares*) given a specific data-generating effect size and a specified NHST threshold

before strong evidence for the null hypothesis can be found. The probability of generating strong evidence for the alternative hypothesis, even with preferential stopping, is below the values given by the universal bound. In contrast, when the effect size is small (Fig. 10b), preferential stopping again influences the likelihood of an experimenter's success beyond what would be expected if the universal bound applied.

Bounds on generating evidence when the truth is part of a composite hypothesis

Here, we address how high the probability of achieving Bayes factor thresholds can be when the hypotheses are nested. An

informal argument can be made in this case. When the effect size is nonzero but close to the null hypothesis, the Bayes factor will tend to initially favor the null hypothesis but eventually will tend to reverse course and support the alternative hypothesis. This can be seen in Fig. 9 (also Fig. 5 of Rouder et al., 2009, and Fig. 2 of Matthews, 2011), where the Bayes factor initially rises in support of the null before dropping off again as the sample size increases. If the true state of the world is very close to the null hypothesis, this initial rise will be larger. Because the null hypothesis is a point prior and the alternative is diffuse (the prior probability of any precise value in the alternative hypothesis is essentially zero), as the true hypothesis approaches the null hypothesis, the average rise in favor of the null can become arbitrarily large. Luckily, researchers cannot often choose their effect sizes, but the fact remains that for any arbitrarily chosen Bayes factor criterion, one can choose a nonzero effect size that will reach that criterion in support of the null with high probability (although it is not certain to do so, as was shown by Kadane, Schervish, & Seidenfeld, 1996).³ Helpfully, bounds on extensions to the fair and biased coins case have been proposed by Good (1967).

There are a couple of arguments that have been put forward defending the intermediate favoring of the null hypothesis. The first is that researchers should not worry, because the effect would have to be so small as to have no practical relevance. For example, A. Smith and Spiegelhalter (1980)

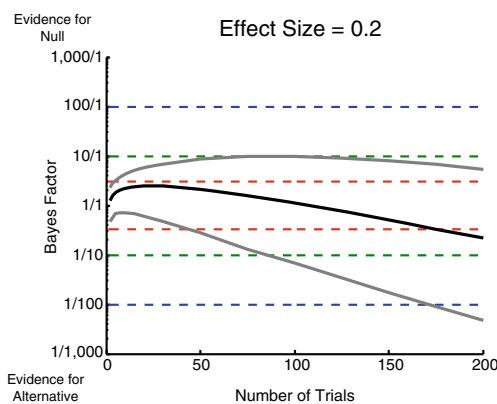


Fig. 9 Changes in the Bayes factor as data are accumulated in example Bayesian t -test experiments. The *black line* represents the mean log Bayes factor across 10,000 example experiments. The *gray lines* give 80% confidence intervals around the mean log Bayes factor, so the *upper line* illustrates the lower bound on the highest 10% of experiments. Values above one indicate evidence that favors the null hypothesis, and values below one favor the alternative hypothesis. The *dashed lines* indicate standard Bayes factor thresholds for substantial (3:1, in red), strong (10:1, in green), and decisive (100:1, in blue) evidence

³ Note that this holds to a lesser degree if the null hypothesis was not a point prior on an effect size of zero, instead covering a range of possibilities that are narrower than the alternative. This is because the shrinking width of the likelihood with larger numbers of trials would exclude values that are probable under the null hypothesis as well. The point prior has been criticized (Cohen, 1994; Nester, 1996), and an alternative has been developed by Morey and Rouder (2011).

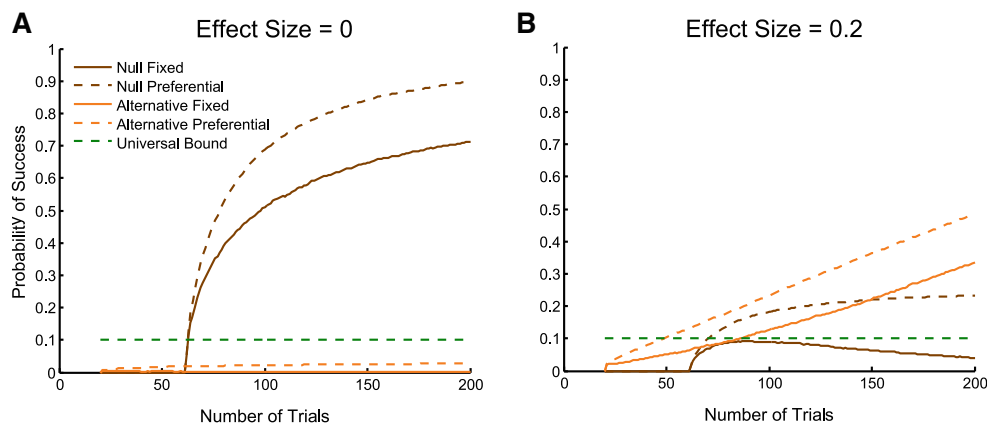


Fig. 10 The probability of successfully using preferential stopping to support a hypothesis as the number of trials increases from 20 to 200, using a Bayes factor threshold of strong evidence (10:1). For the fixed stopping rule, the threshold is checked only at that particular trial number. For the preferential stopping rule, the probability of success is the proportion of experiments that have reached the threshold in favor of the

preferred hypothesis at that particular trial or earlier. The *dashed green* represents the universal bound for the criterion. **a** The true effect size is zero (the data were generated from the null hypothesis). **b** The true effect size is $\delta = 0.2$ (the data were generated from a component of the alternative hypothesis)

claimed that this behavior was not a problem, because it would not have much of an effect on prediction.⁴ We argue that the level of concern about the Bayesian *t*-test's treatment of small effect sizes should depend on the implications of the hypotheses being tested. For hypotheses that are mainly of practical interest, such as whether a particular type of therapy is effective, small effect sizes are likely of little utility, and methods that favor the null hypothesis should not be worrisome. However, some effects are of theoretical importance even if they are small. To take a recent extreme example, evidence that people could predict the future (Bem, 2011) would be interesting even if it affected only one in a billion trials. Although such a small effect would not be of practical use, it would still impact our understanding not just of psychology, but also of biology and physics (Wagenmakers, Wetzels, Borsboom, & van der Maas, 2011). The second argument appeals to Occam's razor, stating that simpler hypotheses should be favored when the data are weak and more complex hypotheses only if the data are strong (Rouder et al., 2009). This argument has wider applicability and is reviewed in the Discussion section.

Finally, we note that in a particular sense, the universal bound can apply to composite hypotheses. In some situations, prior distributions can be constructed so that the prior probability on the parameters reflects the long-run frequencies of these values occurring (Cox & Hinkley, 1974). For example, in psychology, it may be possible to construct a distribution of how often effect sizes of various magnitudes have occurred to use as the prior for the alternative hypothesis in a Bayesian *t*-test. We show in Appendix 1 that for such a composite

hypotheses, the universal bound holds, on average, across experiments for these types of prior distributions. As an example, assume that a very thorough researcher interested in the reliability of an effect decides to replicate a variety of published studies that differ from one another and replicates each of these studies many times. Assume as well that this researcher uses the distribution of effect sizes across these published experiments (and assuming that the effect sizes are all perfectly estimated) as the prior of his alternative hypothesis in a Bayesian *t*-test. If the researcher examines only the set of replications of a single study, the universal bound will not necessarily hold. However, on average across the entire set of replications of different types of materials, the universal bound will hold. There is no equivalent guarantee for NHST.

Discussion

We have shown that there are two common cases in psychological research for which the probabilities of finding convincing Bayes factors violate expectations based on the universal bound. These are (1) where the true hypothesis is not among the proposed hypotheses and (2) where a more general "composite" hypothesis is true but the data lie close to the more constrained hypothesis. In both cases, even fixed stopping can produce Bayes factors with probabilities greater than those given by the universal bound. Optional stopping and, particularly, preferential stopping can be used to further influence these frequencies. Although previous work has noted that the universal bound does not apply to composite hypotheses—for example, in statistics (Mayo & Kruse, 2001; Royall, 2000, and rejoinder)—and the initial favoring of the null hypothesis in Bayes factors has been noted before in

⁴ More recent work has shown how prediction accuracy can be better maximized in this sort of situation (van Erven, Grünwald, & de Rooij, 2012).

psychology and statistics (Atkinson, 1978; Matthews, 2011; Rouder et al., 2009), the work presented here provides a more thorough and general investigation of these findings, showing explicitly how they are related to violations of the assumptions of the universal bound. In particular, the present work demonstrates how large the influence of the choice of stopping rule can be.

We have also shown an important case for which the universal bound does hold: finding evidence against a null hypothesis. Here, BHT certainly has better frequentist properties than does NHST if the null hypothesis is simple, as in the fair versus biased coin example. Even with preferential stopping, the probability of convincing evidence against the null hypothesis follows the universal bound, while for NHST, preferential stopping will eventually always result in rejection of the null hypothesis. BHT also did not exceed the universal bound in the case of the Bayesian t -test where the null hypothesis is a point hypothesis, which differs from a simple hypothesis only in that there is a nuisance parameter in the prior to account for the unknown variance. In our simulations, the nuisance parameter did not cause the universal bound to be violated. This situation needs further investigation but, again, appears to be an important advantage for BHT.

Real Bayesians don't need frequentist guarantees . . .

For Bayesians, Bayes factors represent nothing more than the relative likelihood of one hypothesis over another with respect to a particular set of data. Bayes factors obey the likelihood principle (Berger & Wolpert, 1988), meaning that they depend only on the data that have been actually observed, rather than unobserved data, as frequentist methods do. Following from the likelihood principle is the stopping rule principle: No matter what stopping rule is used by the experimenter, the relative evidence for the hypotheses (summarized by the Bayes factor) remains unchanged⁵ (Berger & Wolpert, 1988; Bernardo & Smith, 1994; Good, 1982; Jaynes, 2003; Lindley, 1957; O'Hagan & Forster, 2004). This is because Bayes factors are conditioned directly on the data and the data already tell us what the Bayes factor was at the time the experiment was stopped.

Even when the universal bound does not apply, the Bayes factors are correct. No modification should be made even if the experimenter used preferential stopping. Take, for example, the case in which neither hypothesis is true, such that the prior distribution over hypotheses gives zero weight to the

process that generated the data. The Bayes factor still correctly describes the relative evidence for each of the hypotheses considered, whether or not the data-generating hypothesis is also considered. The same is true for cases involving composite hypotheses. Furthermore, if the priors used match the beliefs of the researcher, the initial favoring of the null hypothesis for small effect sizes is a feature, not a bug. As was pointed out in Rouder et al. (2009), this initial favoring of the null hypothesis is an application of Occam's razor: More data need to be added into the same analysis for the more general alternative hypothesis to be supported. Comfortingly, enough data will eventually overcome most prior beliefs; consistency means that the best hypothesis will eventually win out.

. . . But it is hard in practice to ignore frequentist implications

Despite the stopping rule being irrelevant to the interpretation of the evidence provided by Bayes factors, researchers may still not feel comfortable with the frequentist implications of the stopping rule choice. Academic researchers are rewarded by publishing, and publication is often based on whether theoretically interesting results are obtained. If the stopping rule used—whether fixed at a certain number of trials or preferential—can greatly change the magnitude or even the direction of the Bayes factors produced, there is scope for experimenters to produce the evidence for which they would be most rewarded even if it contradicts the nature of the underlying phenomenon.

To illustrate, take the case of the Bayesian t -test and assume that we have a new, theoretically relevant, experimental paradigm in which the effect size, unknown to the experimenter, happens to be small. Now assume that the experimenter uses a default prior distribution on the alternative hypothesis that is fairly broad. If the experimenter believes that preferential stopping is irrelevant, he or she may—as a result of theoretical motivations—pursue strong evidence for the null hypothesis, with the consequence that he or she will have a good chance of finding it (as shown in Fig. 8). In addition, onlookers may well also use broad priors to reflect their uncertainty and will interpret the results as strong evidence in favor of the null hypothesis, even if the experimenter reveals his or her stopping rule (further results about onlookers are discussed in Kadane et al., 1996). An unprincipled experimenter has an even larger scope for affecting the results. If an unprincipled experimenter knows that the effect size in a paradigm is small, through either pilot experiments or extensive reading, he or she can design a study to change the beliefs of more naïve onlookers in the direction he or she wishes.

Fortunately, there are some defenses against inadvertent or unprincipled use of stopping rules to change beliefs. The most broadly applicable is for reviewers to insist on a robustness analysis (Berger, 1993; Gallistel, 2009; Wagenmakers et al., 2011; see the online Appendix). An investigation of

⁵ The exception is for cases in which the stopping rule tells you something about the hypothesis that is not available in the data. A colorful example given by Edwards et al. (1963, p. 239) was of an experimenter attempting to count the number of lions drinking at a watering hole who was chased away by lions before observing any of them drink. In this example, the reason for stopping contains information about the hypothesis that is not available in the observed data.

the effects of a range of prior distributions on the resulting Bayes factors can provide a sense of how robust the results are to changes in the prior distribution. Using a narrower prior for an alternative hypothesis can make it more difficult to achieve convincing Bayes factors for the null hypothesis through optional stopping, although at the expense of requiring more trials to produce convincing evidence when the null hypothesis is correct. Another preemptive defense could be implemented by the experimenter. If the experimenter carefully chooses the stopping rule, the frequentist properties of an experiment can be controlled (Berger, Boukai, & Wang, 1997, 1999; Berger, Brown, & Wolpert, 1994; Yu et al., 2013). A final defense is the interest of other researchers in the field who simply collect more data using a paradigm; the posterior distribution on effect sizes from the first experiment should then be used as the prior distribution for the new experiment, and with enough data, the best hypothesis will be favored.

The first case we introduced where the universal bound does not hold, in which the data-generating hypothesis is a combination of the hypotheses under consideration, presents additional problems for preventing experimenters from using stopping rules to produce evidence that suits them. In our first case, the prior distribution has no weight placed on the true generating model. An experimenter who was either unaware that there was a heterogeneous population or aware but unprincipled could test pure hypotheses and could, given some luck, use preferential stopping to produce the desired results. Here, the issue is not uncertainty in the prior, which can be addressed by a robustness analysis, but that the prior puts no weight on the generating model. Fortunately, reviewers in psychology have a strong tendency to ask whether results hold up at an individual participant level, which would prevent between-participants heterogeneity from being an issue. However, intraindividual heterogeneity or data generated from an integrated combination of the hypotheses remain problematic. These situations can be addressed somewhat by measures of model adequacy (Gelman, Meng, & Stern, 1996; Rubin, 1984), but they represent a real problem for any statistical approach and deserve further study.

Conclusions

The long history of debate around BHT and NHST has highlighted the many advantages that BHT enjoys over NHST. In particular, BHT is a coherent and unified account of probabilistic inference for which the choice of stopping rule is irrelevant (Kruschke, 2010; Lee, 2011; Wagenmakers, 2007). Bayesian approaches are one method “to rule them all,” so to speak, and do not require the menagerie of statistical prosthetics needed to make NHST functional under varying methodological assumptions. Bayesian methods also offer

approaches to cognitive modeling that are appealing because they allow for comparisons among complicated models that correctly penalize for model complexity and easily invite hierarchical approaches (Lee, 2011). Moreover, Bayesian methods allow researchers to provide evidence both for and against the null model, a practice that is very difficult in NHST. For these reasons, BHT is preferable to NHST. Indeed, our goal here has not been to show that optional stopping can make BHT incorrect—it cannot—but rather to show where frequentist properties of Bayes factors are dependent on optional stopping and where they are not and to show that these properties can have practical implications for users of BHT. We hope that this work highlights these potential problems and provides researchers with useful suggestions for how these frequentist implications can be controlled.

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Appendix 1 Proofs of the universal bound and its exceptions

The universal bound has been noted by many authors (e.g., Birnbaum, 1962; C. Smith, 1953). The claim is that the probability of observing data that produce a likelihood ratio of k in favor of a false hypothesis is less than or equal to $1/k$. Here, we give a proof, following C. Smith (1953). The first step of this proof is to calculate the expected value of the likelihood ratio over all possible data. Let T be the simple true hypothesis and F the simple false hypothesis; then,

$$E_T \left[\frac{P(X_n|F)}{P(X_n|T)} \right] = \sum_{X_n} \frac{P(X_n|F)}{P(X_n|T)} P(X_n|T) = \sum_{X_n} P(X_n|F) = 1, \quad (9)$$

where E_T indicates taking the expected value over the probability of each data set given the true hypothesis; the first equality is the expected value written explicitly. The third equality holds because the total probability of all the possible data under any probability distribution is one.

Using this fact, it is straightforward to derive the universal bound. Every likelihood ratio and probability must be non-negative, so the constraint on the expected value can be thought of as a budget that must be spent on various likelihood ratios. Each likelihood ratio has a certain cost (the likelihood ratio itself); the purchaser buys some amount of each (the probability of each likelihood ratio), but the purchaser has only one unit to spend over all the likelihood ratios (the constraint on the expected value). If the probability of the likelihood ratio equaling k is equal to $1/k$, then the expected

value will be $k \times (1/k) = 1$, and the budget will be exhausted, leaving nothing left to spend on any other likelihood ratios. Thus, $1/k$ is the maximum probability for this likelihood ratio, so it does not exceed the expected value constraint; this makes it the universal bound. Indeed, if there is a probability of $1/k$ of the likelihood ratio equaling k , every other data set (which together have probability $(k-1)/k$ of occurring) must have a likelihood ratio of zero (Royall, 2000), which is infinite evidence for the true hypothesis.

The above proof covers experiments with a fixed number of trials but was generalized to cases of optional stopping by Kerridge (1963) and, more rigorously, by Robbins (1970). Here, we give a simplified version of Kerridge's proof that is restricted to one simple true and one simple false hypothesis. Let us assume that an experimenter uses preferential stopping and ends the experiment as soon as the likelihood ratio in favor of the false hypothesis exceeds k . Let \sum^* denote summation over the sets of data X_n for which sampling has stopped, meaning sets of data for which the likelihood ratio is greater than or equal to k . If sampling has stopped, then $X_n \equiv X_r$, where r is the first trial on which the likelihood ratio is greater than or equal to k .

Before, we found that the expected value of the likelihood ratio was equal to one when taken over all possible data outcomes. Here, we note that by only summing over a portion of the probability distribution, we may find a value less than one,

$$\sum^* \frac{P(X_n|F)}{P(X_n|T)} P(X_n|T) = \sum^* P(X_n|F) \leq 1. \quad (10)$$

Next, we use the fact that we are summing over likelihood ratios that are greater than or equal to k to find a lower bound for the partial expected value:

$$\sum^* \frac{P(X_n|F)}{P(X_n|T)} P(X_n|T) \geq k \sum^* P(X_n|T). \quad (11)$$

Combining Equations 10 and 11, we find the bound on the probability that the experiment was stopped at trial n or earlier:

$$\sum^* P(X_n|T) \leq \frac{1}{k}. \quad (12)$$

Because the sum is a monotonic increasing function of n and is bounded, $\lim_{n \rightarrow \infty} \sum^* P(X_n|T)$ exists and is less than or equal to $1/k$. Thus, no matter how long the experiment runs, the probability that an experiment that uses optional stopping can produce a likelihood ratio that achieves a threshold is less than or equal to the universal bound.

The proofs of the universal bound given above rely on a test of a false hypothesis against the hypothesis that generated the data. Equations 9 and 10 cancel the data-generating distribution against the true hypothesis because they are the same. If the truth is a combination of the hypotheses or a composite hypothesis is true, this cancellation does not hold, and neither does the bound.

The universal bound can hold on average for composite hypotheses

A composite hypothesis is a collection of simple hypotheses, and to compute the Bayes factor, each simple hypothesis is given a prior probability. Let us assume that the prior on the simple hypotheses is an accurate reflection of the environment: The long-run frequency of each simple hypothesis occurring is proportional to its prior probability. In this case, the universal bound holds on average, although it does not necessarily hold for particular simple component hypotheses. From Equation 2, we have $P(X_n|T) = \int_i P(X_n|\theta_i, T) P(\theta_i|T) d\theta_i$, where θ_i is a simple hypothesis and $P(\theta_i|T)$ is the prior distribution over the simple hypotheses in the composite hypotheses. We note that taking a weighted average over the partial expected values of the likelihood ratios when the data are generated from a simple component hypothesis can, of course, be written as the partial expected value of the likelihood ratio when the data are generated from the true composite hypothesis:

$$\int_i \left(\sum^* \frac{P(X_n|F)}{P(X_n|T)} P(X_n|\theta_i, T) \right) P(\theta_i|T) d\theta_i = \sum^* \frac{P(X_n|F)}{P(X_n|T)} \left(\int_i P(X_n|\theta_i, T) P(\theta_i|T) d\theta_i \right). \quad (13)$$

We can then follow the proof above to show that the average probability of generating evidence against a true hypothesis follows the universal bound. However, this is now a bound over a variety of possible experiments, rather

than a bound for an actual experiment when one of the simple component hypotheses is true because the data-generating distribution will not cancel with the true composite hypothesis in the likelihood ratio in Equations 9 and 10.

Appendix 2 Equivalence of binomial data generated from randomly selecting probabilities and from a weighted average of probabilities

Assume two data-generating hypotheses that produce binomially distributed successes with probability p_1 and p_2 . Let w be the binomial probability of choosing p_1 on a given trial and $1-w$ be the binomial probability of choosing p_2 . Here, we

$$\begin{aligned} P(s|n) &= \sum_{s_1=0}^s \sum_{f_1=0}^f \binom{n}{s_1, s-s_1, f_1, f-f_1} (wp_1)^{s_1} ((1-w)p_2)^{s-s_1} (w(1-p_1))^{f_1} ((1-w)(1-p_2))^{f-f_1} \\ &= \binom{n}{s} \left(\sum_{s_1=0}^s \binom{s}{s_1} (wp_1)^{s_1} ((1-w)p_2)^{s-s_1} \right) \left(\sum_{f_1=0}^f \binom{f}{f_1} (w(1-p_1))^{f_1} ((1-w)(1-p_2))^{f-f_1} \right) \\ &= \binom{n}{s} [wp_1 + (1-w)p_2]^s [w(1-p_1) + (1-w)(1-p_2)]^f \\ &= \binom{n}{s} (p_w)^s (1-p_w)^f \end{aligned}$$

where the third line comes from using the binomial formula: $\sum_{a=0}^b \binom{b}{a} x^a y^{b-a} = (x+y)^b$. The final line is the probability mass equation for a binomial distribution (as in Equation 4) for which the probability parameter is $p_w = wp_1 + (1-w)p_2$.

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show that the output of this data-generating process is a third binomial distribution.

Let s_1 and f_1 be the number of successes and failures generated using p_1 , and similarly, s_2 and f_2 are the number of successes and failures generated using p_2 . We want the overall probability of s successes and f failures from n trials ($n = s + f$). We first write the multinomial probability of s_1, s_2, f_1 , and f_2 , summing over all of the ways that $s_1 + s_2 = s$ and $f_1 + f_2 = f$:

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