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Statistical inference in behavioral research: traditional and Bayesian approaches

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Null hypothesis significance testing (NHST) has long been a mainstay of scientific research, more in some scientific fields than others. It persists despite numerous calls across multiple scientific disciplines to abandon or at least modify the practice. In 2016, the American Statistical Association issued a statement decrying the use of the "bright line" $p < .05$ criterion as leading to a "considerable distortion of the scientific process." There are a number of alternatives to NHST that don't share its logical and practical deficiencies. First among them is Bayesian inference, which can be viewed as both a calculus of evidence and of belief. The Bayesian definition of "evidence" differs profoundly from what the p-value represents. In this chapter, we review deficiencies in NHST and provide an introduction to Bayesian reasoning, with particular attention to its relationship to the truth of scientific claims.

Statistics | Inference | Bayesian | Frequentist | JASP | Bayes factor | p-value

The recent crisis of confidence in psychological science (Pashler & Wagenmakers, 2012) has led to repeated calls for better statistical methods, better study designs, and better social incentive structures. The link of the analytic and inferential issues above to research integrity is fairly direct, in that research integrity, writ large, is the fidelity of the scientific process and resulting conclusions to the truth. If it can be shown that the manner in which studies are done, data is analyzed, or conclusions drawn is likely to systematically deviate from the truth, then by definition we have a challenge to research integrity. There has been an evolution of professional and scientific norms within the behavioral sciences that have exaggerated and reified some of the most unfortunate misconceptions and misuses to which standard methods using hypothesis tests and p-values (known as "frequentist") are subject; in particular that statistical significance is an arbiter of truth, that the credibility of a claim can be assessed without considering the prior probability of the claim being true, and that non-significant studies are uninformative. These misconceptions and misuses include but are not limited to:

- ✗ The false belief that a typical experiment yielding $p < .05$ is all that is needed to prove a theory;
- ✗ Resistance to replication of important experiments;
- ✗ The difficulty of publishing research replications;
- ✗ Strong selection pressure at journals for significant findings (i.e., publication bias);
- ✗ Professional advancement dependent on publications in highly cited journals, with acceptance influenced by statistical significance;
- ✗ Widespread use of "p-hacking";
- ✗ Failure to use sample sizes that correspond to a priori plausible or scientifically important effect sizes;
- ✗ Failure to share data;
- ✗ Failure to prepare, share or publish research protocols.

A number of the practices above, particularly those related to replication and publication, can be related directly to practices based on extreme versions of frequentist philosophy. But they also mean, because so many of these practices are deeply entrenched and indeed, institutionalized, that adopting alternative methods (e.g., "Bayesian") cannot solve all of the challenges to scientific integrity in the social and behavioral sciences. Nevertheless, we believe that understanding Bayesian approaches represents a critical piece of a multifaceted strategy that the behavioral sciences must adopt if its findings and claims are to be regarded as reliable. To explain why this is so, in this chapter we will review the fundamentals of both Frequentist and Bayesian philosophies and methods.

The Foundations of Inference: Types of probability

Science starts and ends in uncertainty. As such, it should not be surprising that the properties and indeed integrity of any scientific method depends on how it represents uncertainty. The most basic measure we have for representing uncertainty is probability. Many scientists are surprised to learn that probability is an extraordinarily difficult and complex measure, philosophically and scientifically. We will begin this discussion of methods in statistical inference by outlining how different conceptions of probability lead to different approaches to inference. Many controversies about the proper approaches to statistical inference are in fact derived from controversies about the meaning of probability itself, which we will review here.

The original meaning of probability derived from the same root as "approbation," related to the degree to which an opinion or action was supported by evidence, such as when deciding on the guilt or innocence of a suspect. This kind of probability was "epistemic" in nature; it related to one's degree of belief, or a logical relationship between the opinion and the strength of underlying evidence. This notion was completely distinct from the notion of "chance," as exemplified in games of chance, or gambling. This kind of probability came to be designated "aleatoric", related to games, or "stochastic," which today applies to a random physical process.

Today's dominant approaches to probability can be divided into the "frequentist" and "epistemic" schools. The frequentist notion

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derives from the aleatoric type and epistemic probability is sometimes called “Bayesian.” The frequentist approach to probability is actually the more recent of the two, having only been formalized in the early 20th century. The frequentist approach represented an attempt to make probability as objective and measurable a scientific quantity as physical measurements like height, weight, and mass. This was achieved by defining the probability of event A as being equal to its proportion in a pre-specified, in principle observable, “collective” of repeatable random events – equivalent to a “long-run frequency.” The idea was that if we could observe this proportion, this probability would be objective, uniquely specified and observable. This probability was deductive in nature, in that once the collective was specified, the probability of outcomes within it would be set, or as von Mises famously declared, “First the collective, then the probability” (von Mises, 1957).

Two consequences of this definition are worth noting, as the problems with any probability definition are inevitably shared by the systems of inference built upon them. First, the frequentist notion of probability does not apply to individual events, but rather to the collective itself (i.e., the “long run”). Thus, if an experiment is generating a single outcome to which we want to assign a probability, this definition says that we cannot apply a probability to that individual experiment, only to the “long run” of repetitions. This is why virtually every traditional statistical measure, from p-values to confidence intervals to Type I and Type II error rates, have definitions starting with “if this experiment is repeated.” The second problem is that the long run is not actually observed but constructed through a thought experiment. One can “imagine” what might happen if the experiment were repeated many times, but this differs from having the multiple repetitions in hand. There may not be a consensus on why the experiment stopped or how it was run, with consequent uncertainty about which “long run” is relevant; a given result can be a legitimate member of several different long runs (Goodman, 1999a; Wrinch & Jeffreys, 1919).

To complicate this further, outcomes within a given long run may not be equiprobable; for instance, the border of a “tail area” used to calculate a p-value is almost always the most probable outcome within that tail and grouping them together sometimes violates inferential intuition. So the conditions for using frequentist probability as a foundation for inference come at a price; the resulting numbers cannot be used to apply to an individual experiment, observers must agree on the hypothetical “long run”, and demonstrably different elements of a long run may be treated similarly. These properties generate the requirement for rigid prespecification of all experimental procedures, including outcome measures and stopping rules, and cautions that 95% confidence intervals don’t mean we have 95% confidence in any individual interval.

In contrast, epistemic probability can apply to individual events or to propositions that are not repeatable events. It is a plausibility, or a “degree of justified belief”, with the justification arising from underlying evidence. The “logical” subtype of epistemic probability requires that the correspondence between belief and the evidence be unique, based on logical relationships that can be difficult to get agreement on. The “subjective” subtype allows for variation among individuals but raises the question of whether inter-subjective variability renders it illegitimate as a scientific tool.

What makes probability “scientific?”. The question of what makes methods based on these types of probability “scientific”, or correspond with the truth, is a central issue for science. For the frequentist, a “scientific” or objective probability is based on correspondence with an imagined empirical reality. This reality is not usually observable, and is typically based on statistical models, calculations or simulations. Even if these models are correct, or agreed upon, they apply only to the data, not to hypotheses, so a frequentist has no language or measure of uncertainty about hypotheses giving rise to the data.

The “scientific” property of epistemic probability is not empirical, but logical. It is that probabilities are (a) consistent (i.e., one would never believe simultaneously that $P(A) > P(B)$ and $P(A) < P(B)$), and (b) coherent, in that one would never act based on these beliefs in ways that guaranteed one would be worse off. These properties lead us directly to Bayes theorem: one can only satisfy these conditions if one’s epistemic probabilities are modified by empirical data using Bayes theorem. Bayes theorem further guarantees that with accumulating data, any intersubjective differences (such as interindividual differences in the strength of one’s prior beliefs) will eventually disappear and probability estimates will typically conform to observable reality (Diaconis & Freedman, 1986). So epistemic probability in some sense ends where frequentist probability tries to start – a correspondence with observed reality. As Kendall (1949) stated, “Neither party can avoid using the ideas of the other to set up and justify a comprehensive theory” (p. 107).

Bayes theorem. It was the need to answer fundamental questions about the behavior of games and rational betting strategies that led to developments in the calculus of probabilities (Devlin, 2010). It was the theorem of an amateur scientist, the Rev. Thomas Bayes, that has reverberations today. He set out to answer the question of how much one should bet on one player versus another player in an interrupted game of chance. In solving this, he came up with an equation that is an uncontroversial mathematical expression. It was that the probability of two events occurring together, A and B, could be decomposed into different ways: the probability of A given B, times the probability of B, or the probability of B given A, times the probability of A. This can be written: $P(A\&B) = P(A \mid B) \times P(B) = P(B \mid A) \times P(A)$. Equating the two expressions on the right, this can be rearranged to yield Bayes theorem:

$$P(A \mid B) = \frac{P(B \mid A) \times P(A)}{P(B)}.$$

This is merely the algebra of conditional probability, subject to no more controversy than $1 + 1 = 2$. The difficulty begins when we assign meanings to A or B such that their probabilities cannot be directly observed. If A is a scientific hypothesis and B is data, Bayes theorem becomes:

$$P(\text{Hypothesis} \mid \text{Data}) = \frac{P(\text{Data} \mid \text{Hypothesis}) \times P(\text{Hypothesis})}{P(\text{Data})}.$$

This equation requires us to define a measure that corresponds to the probability of a hypothesis being true (e.g., $P(\text{Hypothesis})$) without data. This kind of probability falls into the “epistemic” category; logically justified, perhaps, but not necessarily empirically confirmable.

So Bayes theorem, when applied to the process of inference—drawing conclusions about nature based on observed data—requires an epistemic probability of a hypothesis. This was historically known as “inverse” probability because it allows us to “invert” $P(\text{Data} \mid \text{Hypothesis})$ to $P(\text{Hypothesis} \mid \text{Data})$, but it is now more commonly called “Bayesian” probability (Fienberg, 2006). The acceptance or rejection of this foundational concept is at the core of the controversy about the use of Frequentist and Bayesian methods in statistics.

Statistical inference. Statistical inference is a subset of the broader subject of scientific inference. An inference from the general (hypothesis) to the specific (data) is called "deductive" and is truth preserving, in that the conclusions are true if the premises are true. This is what makes it an attractive foundation for empirical science; it guarantees—as in pure mathematics—that all statements deriving from the premises are valid if the premises are true. But it comes at the price of not expanding our knowledge beyond what is already in the premises. Making a statement about the truth of a hypothesis based on observed data is a form of inductive inference, also called "ampliative" inference, in that the conclusion (about a hypothesis) has more explanatory power than the premises (the data). So inductive logic "amplifies" our knowledge, but at the price of not knowing if our conclusions about the hypotheses are correct.

In statistical inference, the hypotheses are probabilistic statements about nature, i.e., statistical. Examples of statistical hypotheses are "a response rate is 10%," or "the success rates of two interventions are equal." Under such hypotheses, one can predict the distribution of observations one would expect under specified experimental conditions. A prediction based on probabilistic formulae, of how often various outcomes will arise under a specified statistical hypothesis is deductive, and the attendant probabilities, "frequentist." Any probabilistic statements about the underlying truth are by definition epistemic, or "Bayesian."

Origins of frequentist inference

The central challenge of statistical inference is how to make statements not about observable data, but about the hypotheses that give rise to them, the essence of inductive reasoning. Until the early 20th century, a widely accepted methodology for how to use data to ascertain the truth of underlying hypotheses did not exist. Scientists and statisticians were familiar with the mathematics of probability, but how to use those mathematical properties to draw conclusions about nature from data was far more unsettled. The problem lay in the measure of probability itself; there was a well-known formula that could guide inference about probabilities—Bayes theorem—but its use required the acceptance of epistemic probability that many rejected as a foundation for sound science. The challenge was whether a method could be constructed, based purely on frequentist probability, that could provide a measure of uncertainty about underlying hypotheses without the machinery of Bayes or attendant Bayesian probabilities.

Frequentist inference as we know it today was really born in the 1920s and 1930s, as a reaction to the Bayesian model. The pioneers in this frequentist revolution were Ronald Fisher, Jerzy Neyman, and Egon Pearson. Fisher was a mathematician, geneticist and active experimentalist, the latter in the field of agriculture. Neyman and Pearson were mathematical statisticians. The driving motivation was to develop a new framework of inference that was "objective," in the sense that it was not based on epistemic uncertainty. Fisher believed that "The theory of [Bayesian inference] is founded upon an error, and must be wholly rejected" (Fisher, 1925, p. 10). What was this fatal error? He objected to using Bayes theorem when there was no basis to estimate the prior probability of a hypothesis, nor an acceptable way to assign probabilities if we claimed prior ignorance (Aldrich, 2008; Zabell, 1989). In the 1920s Fisher constructed his own view of how inference could be conducted, without needing to specify prior distributions. These included new approaches to both testing and estimation. He took the idea of a tail-area probability, used by Karl Pearson, and made it his central tool for statistical testing, calling it the "p-value", short for "probability" value, or "associated probability." The use of the p-value sidestepped the topic of prior probabilities by only considering which data might be observed if the null hypothesis were true.

The p-value was originally intended to be used as a measure of evidence against the null hypothesis to be combined with other sources of evidence, and not as an "error rate" associated with a decision. Fisher suggested that the $.05$ level might be a useful benchmark, not for determining whether the null hypothesis was likely to be false, but for deciding whether an experiment was worth repeating (some more history about the origins of the $.05$ level is given in Cowles & Davis, 1982). He stated that the $.05$ threshold represented weak evidence in a single experiment, and that one should consider the null hypothesis to be false only if, upon repeated experimentation, "a properly designed experiment rarely fails to give this level of significance" (Fisher, 1926). So the "one and done" modern practice of declaring theoretical confirmation based on a single significant experiment is antithetical to the practice suggested by its originator.

Fisher's influence expanded immeasurably with the publication in 1925 of his landmark statistical textbook, Statistical methods for research workers (Fisher, 1925). This textbook was the first of its kind, aimed at practicing scientists, filled with practical examples showing how to analyze common experimental designs, and served to popularize the use of the p-value. This book, revised 14 times, was a scientific best seller from the time of its publication until after Fisher's death in 1962.

Fisher's approach to inference had both formal and informal components. Two of Fisher's contemporaries, Jerzy Neyman and Egon Pearson, began to try to reframe Fisher's ideas in a more formal mathematical framework. In 1928 they proposed a modification to the original testing procedure of Fisher (Neyman & Pearson, 1928). Their idea was to introduce an alternative hypothesis to contrast to the null hypothesis used by Fisher, and to propose formal decision rules for accepting and rejecting these hypotheses. They introduced the now familiar notions of Type I and II errors and power. They proposed that statistical properties of various decision rules should be studied, and in 1933 they derived the properties of optimal statistical tests (Neyman & Pearson, 1933). Neyman would later turn his sights to the topic of estimation, proposing the now ubiquitous confidence interval procedure (Neyman, 1937).

The innovations by Fisher, Neyman, and Pearson served as the foundation of modern mathematical statistics. In the following sections we outline the differences between them, the ways in which frequentist testing and estimation is done today, and summarize a number of common criticisms levied at them.

The basics of frequentist testing

In a statistical testing context we are concerned with deciding which of a set of competing hypotheses are true. A statistical hypothesis refers to either population parameter values or the forms of statistical models. Tests of these statistical hypotheses are used as a stand-in for tests of scientific hypotheses. For instance, the statement that "the average height of men in the population is not equal to that of women" is a hypothesis about the difference between the averages of populations of men and women. If we let the parameter δ represent the difference in height between populations of men and women, then a translation of our hypothesis into statistical language would be that $\delta \neq 0$.

In the Neyman and Pearson theory of hypothesis testing there are two competing hypotheses: the null hypothesis and the alternative hypothesis. The alternative hypothesis is chosen so that it corresponds with our hypothesis of interest; the null hypothesis is (typically) constructed such that it represents the complement of the alternative hypothesis. In our heights example, the hypothesis that $\delta \neq 0$ would be our alternative hypothesis and our null hypothesis would be its complement, $\delta = 0$. In the Neyman and Pearson framework, the outcome of a hypothesis test is a binary decision: either reject the null hypothesis and accept the alternative or accept the null hypothesis. This leads to the possibility of making two types of errors: rejecting the null hypothesis when it is actually true (false positive, "α" or Type I error), and not rejecting the null hypothesis when it is actually false (false negative, "β" or Type II error). The set of observations that would lead to rejection of the null hypothesis are called the rejection region of the test. If the observed data are in the rejection region (e.g., $Z > 1.96$) then one is supposed to "reject" the null hypothesis. In this framework, one chooses the rejection region such that there is no more than $(100 \times \alpha)$% chance of making a Type I error, while at the same time keeping the chance of making a Type II error to a minimum. In practice, the use of $.05$ for the Type I error and less than $.20$ for the Type II error has become standard.

Fisher's significance test shared many features with that of Neyman and Pearson, with a few key differences. In Fisher's approach, there is no alternative hypothesis; one only considers which data might be observed if the null hypothesis were true. With the data in hand one computes the p-value, which is the probability of the observation plus the probability of any other observations further from the null hypothesis. For example, if one observes a Z-value of 2.5, then the p-value is the probability of observing a Z-value greater than or equal to 2.5. Fisher felt that the exact level of the p-value was informative, whereas in a hypothesis test the only information to be used was whether the result fell in the rejection region, and a p-value was not calculated at all. If the p-value is small then one has evidence to suggest the null hypothesis is not true, with smaller p-values providing stronger evidence. On the topic of whether to regard a given p-value is considered "significant," in this approach it is "open to the experimenter to be more or less exacting in respect of the smallness of the probability he would require before he would be willing to admit that his observations have demonstrated a positive result" (Fisher, 1971, p. 13). This represented the kind of informality that the Neyman-Pearson hypothesis test was designed to eliminate. As noted previously, Fisher also put a particular premium on replication of significance, an idea revived in the 2000's under the rubric of research reproducibility (e.g., Goodman, Fanelli, & Ioannidis, 2016).

Fisher very strongly rejected hypothesis tests as being too algorithmic and thereby anti-scientific, ironically a criticism that is today aimed at his innovation, the use of p-values. But today, the two approaches are typically taught and practiced as a unified set of methods. For instance, a researcher might set up a null and alternative hypothesis and choose a Type I error rate of $.05$, and then once the data are observed they report: a) whether the data fall into the rejection region and the null hypothesis was rejected, as well as b) the strength of the evidence against the null hypothesis in the form of a p-value. More historical detail on that controversy and the rise of frequentist inference can be found in Hald (2008), Goodman (1993), and Gigerenzer (1993, 2004).

Criticisms of frequentist testing. A number of criticisms have been levied toward the frequentist approaches to hypothesis testing. First, researchers have a tendency to misinterpret p-values and to use them to draw improper inferences from their data (Greenland et al., 2016). A survey by Oakes (1986; replicated by Haller & Krauss, 2002) illustrates these misconceptions. Oakes quizzed psychology researchers' interpretations of frequentist hypothesis tests by presenting an experiment that results in $t(18) = 2.7$ and $p = .01$. In response, 35% of the researchers marked as true the statement, "The probability of the null hypothesis has been found;" 85% endorsed the statement, "The probability that the decision taken is wrong is known;" 60% endorsed the statement "A replication has a $.99$ probability of being significant." Since neither a p-value nor Type I error rate apply to the underlying hypotheses, none of those interpretations are correct.

Other critiques have focused on the statistical properties of the null-hypothesis significance testing procedure. The p-value is defined as the probability, if the null hypothesis were true, of results as or more extreme than those observed in the experiment. That is, a p-value takes into account not only the results that were actually observed in the experiment, but also those that could have potentially been observed but were not. This dependence on unobserved data has been seen as an inherent weakness of the procedure (e.g., Jeffreys, 1961), and many take issue with the ambiguity in the definition of which outcomes are "more extreme" than those observed because this depends critically on the sampling plan (Goodman, 1999a; Lindley, 1993) – and the sampling plan is often arbitrarily chosen (in many research labs) or unknown (in the case of naturally occurring data, meta-analyses, etc.).

Another challenge associated with frequentist testing procedures is that they are not always logically consistent. Schervish (1996) and Royall (1997) demonstrate a number of general cases where both the process of using p-values as measures of evidence, as well as the process of strict reject/accept hypothesis tests, can lead to paradoxical inferences. Consider two researchers, Pat and Oliver, who want to test whether men and women have different heights. Both specify a point null hypothesis that the average difference is zero (i.e., $\delta = 0$), but Oliver is only interested in whether men are taller, so decides to use a one-sided test. They both agree to use $\alpha = .05$ to determine their rejection region, meaning Oliver rejects his null hypothesis if $Z > 1.64$ and Pat rejects her null hypothesis if $|Z| > 1.96$. If the experiment results in $1.64 < Z < 1.96$, then Oliver rejects his one-sided null hypothesis, $p < .05$, and asserts that men are taller on average than women. At the same time, Pat cannot reject her null hypothesis, because her calculated p-value is greater than $.05$, and hence cannot assert the weaker logical claim that men are either taller or shorter than women. Thus we are licensed to conclude that men are taller than women, but, paradoxically, have to withhold judgment about whether they are taller or shorter. Examples like these also challenge the notion that frequentist testing is completely objective; we have the same data, the same null hypothesis, yet we cannot know the p-value (and the decision) without knowing what is in the scientists' minds.

A last issue with p-values as measures of evidence is that they incorporate no information about effect magnitude. A large effect in a small study can have the same p-value as a small effect in a large study. This violates basic scientific intuition that if two observed effects have the same "statistical distance" from the null effect (e.g., the same number of standard errors), the one further from the null contradicts it more strongly. The p-value does not have that property because it is calculated only in relation to one hypothesis. Some statisticians and philosophers (e.g., Evans, 2015; Royall, 1997) object on logical grounds to calling the p-value "evidence," saying that "evidence" must explicitly compare hypotheses, as the purpose of evidence is to modify belief; "evidence" is a construct that elevates data from being a neutral observation to something inferentially relevant. This is the framework for the Bayes Factor, an alternative measure to the p-value, discussed later.

Frequentist estimation

Recently the field of psychology has seen efforts to replace the apparently problematic practice of hypothesis testing with a focus on estimation (Cumming, 2014), a practice long advocated (Gardner & Altman, 1986) and now standard in biomedical research (Altman, Machin, Bryant, & Gardner, 2013). Estimation has a different goal than hypothesis testing. Instead of accepting or rejecting hypotheses, estimation concerns which parameter values of a model are most consistent with the observed data. For example, instead of testing the difference in heights of men versus women, we would just estimate the average difference. Frequentist estimation problems consist of two components: finding a best guess about a parameter and computing an interval of uncertainty around it. The "best guess" is called the point estimate, and the frequentist uncertainty interval is called a "confidence interval." While this seems straightforward, the formal definition of a confidence interval is rather convoluted, because of its foundation in frequentist probability. Namely, "An X% confidence interval for a parameter θ is an interval (L, U) generated by a procedure that in repeated sampling has an X% probability of containing the true value of θ, for all possible values of θ" (Morey, Hoekstra, Rouder, Lee, & Wagenmakers, 2016). It is important to appreciate the subtle implications of this definition. The frequentist paradigm allows us to make statements about the group of estimates generated by the confidence interval procedure, but this differs from making statements about the estimates themselves.

To illustrate the distinction, take the following example (due to D. Basu; Ghosh, 1988). To determine a 95% confidence interval, let us ignore the data and instead generate a random number between 0 and 1 from a uniform distribution, deciding as follows: CI = $\emptyset$ if the number is $.05$ or smaller and CI = $(-\infty, +\infty)$ otherwise. Note that this odd procedure has the same property: with a probability of one in twenty (5%), it will generate the empty interval $\emptyset$, which does not contain the true parameter, and with a complementary probability 95%, it will generate the infinite interval that does contain the true parameter. Hence, according to the definition, any interval generated by this procedure is a valid 95% confidence interval. It is hopefully clear that these intervals are useless for scientific inference.[1]

This example is artificial, but the same principle applies in situations where we believe strongly in the null hypothesis (e.g., the existence of ESP), or any range of hypotheses. We do an experiment that generates a 95% CI on the ESP effect size of, say, 0.3 to 0.6. We would recognize this as very unusual, and would not accord it a 95% probability of including the truth because that would mean that after this one result we would have a 95% or greater belief in the existence of ESP. This shows that we do not accord every observed interval the same 95% chance of including the truth. If we strongly believe in the null, we will accord every observed interval including the null much higher than 95% chance of including the truth, and every CI not including the null a much lower than 95% chance of including the truth. If our prior evidence/belief is justified, this would be confirmed empirically.

From these thought experiments, we recognize that the properties of a confidence interval generating procedure do not necessarily transfer to the confidence intervals themselves, and that we need another inferential approach to know what credibility to apply to any particular observed interval. This is not a new insight. The subtle distinction between properties of an interval and properties of the process that generated an interval is why Neyman used the neologism "confidence," instead of "probability" to describe the interval, as he was aware that the confidence level did not accord with the frequentist notion of probability.

Criticisms of frequentist estimation. Most confidence intervals used in practice can be seen as inversions of one hypothesis test or another, in that the parameter values inside a $1 - \alpha$ confidence interval are precisely those which would not be rejected by a level α hypothesis test. Thus, these confidence intervals necessarily inherit the statistical criticisms of hypothesis tests mentioned above.

Like hypothesis tests, confidence intervals are often misinterpreted. Hoekstra, Morey, Rouder, and Wagenmakers (2014) provided researchers and students with a survey about confidence intervals, analogous to the survey conducted by Oakes (1986) about hypothesis testing. This survey presented the result of an experiment with a 95% confidence interval for the mean ranging from 0.1 to 0.4. In response, 86% of the researchers from this sample marked as true the statement, "The 'null hypothesis' that the true mean equals 0 is likely to be incorrect"; 59% endorsed the statement "There is a 95% probability that the true mean lies between 0.1 and 0.4"; 47% endorsed the statement "The probability that the true mean equals 0 is smaller than 5%"; 58% endorsed the statement "If we were to repeat the experiment over and over, then 95% of the time the true mean falls between 0.1 and 0.4." Just as with the previous survey, none of these statements follow from the result of a confidence interval.

Recognition that the p-value value was both widely misused in a "bright line" fashion and misinterpreted as an inverse probability led the American Statistical Association to issue a remarkable statement about p-values in 2016 (Wasserstein & Lazar, 2016), the first such statement in its 125-year history. The most important two points of the statement were that $p \leq .05$ does not mean that the probability of the null hypothesis is less than $.05$, and that the use of $p \leq .05$ as an indicator of the truth or falsity of a scientific claim represented poor scientific practice.

Bayesian methods

Bayesian updating. The way in which probabilities are expressed in natural language is prone to misunderstanding. The confusion of the inverse is the cognitive illusion that the very different probabilities $P(A \mid B)$ and $P(B \mid A)$ are similar in magnitude, whereas they can be completely divergent. A common illustration is the case where A = "Jane is a US citizen," and B = "Jane is a member of the US Senate." In that case, $P($Jane is a citizen $\mid$ Jane is a Senator$)$ is 1, whereas $P($Jane is a Senator $\mid$ Jane is a citizen$)$ is close to 0. Similarly, if we flip a standard coin 5 times and get 5 heads the probability of getting that is $P(5\text{heads} \mid \text{Fair coin}) = 1/32$, but the probability that the coin is fair given the 5 heads, $P(\text{Fair Coin} \mid \text{Data})$, would still be 1 because—outside of statistics textbooks—people do not carry around biased coins (Gelman & Nolan, 2002).

[1] Note that an exactly analogous procedure can be conceived for null hypothesis significance testing: Reject the null hypothesis if and only if a 20-sided die comes up 1. Such a procedure guarantees that, in the long run, we will falsely reject approximately 5% of all true null hypotheses. The procedure is nevertheless entirely useless.

Bayes theorem provides us a way to properly go from $P(\text{Data} \mid \text{Hypothesis})$, a "direct probability" that we can calculated under any model, to $P(\text{Hypothesis} \mid \text{Data})$, the inverse probability. The inverse probability is called a "posterior" probability since it is the probability of the hypothesis after considering the data, contrasted with the "prior" probability of the hypothesis before seeing the data. Going from the prior probability to the posterior probability is called Bayesian "updating." To show this, we will write Bayes theorem slightly differently than before:

$$\underbrace{P(\text{Hypothesis} \mid \text{Data})}_{\text{Posterior probability}} = \underbrace{P(\text{Hypothesis})}_{\text{Prior probability}} \times \underbrace{\left(\frac{P(\text{Data} \mid \text{Hypothesis})}{P(\text{Data})}\right)}_{\text{Updating factor}}$$

In this equation, P(Hypothesis) is the prior probability of the hypothesis, and $P(\text{Data} \mid \text{Hypothesis})/P(\text{Data})$ is an updating factor that captures how much more likely the hypothesis becomes once the data are factored in (Carnap, 1950; Keynes, 1921; Rouder & Morey, 2019). This updating factor is a measure of the strength of evidence supporting the hypothesis (Berger & Wolpert, 1988; Edwards, Lindman, & Savage, 1963; Royall, 1997; Wagenmakers, Gronau, Dablander, & Etz, in press).

Bayesian methods have a number of attractive properties for use in science. Because they derive directly from epistemic probability theory, they are guaranteed to be internally consistent, and because they are built on a formal system, they do not rely on shortcuts, heuristics, or leaps of logic. Most importantly, because Bayesian methods allow us to calculate the probability that a hypothesis is true, their use is particularly attractive for behavioral scientists (Edwards et al., 1963; Etz & Vandekerckhove, 2018; Vandekerckhove, Rouder, & Kruschke, 2018).

Bayesian testing. The power of Bayesian methods comes with certain requirements. Because the system of inference is formal, the researcher is required to be similarly precise in the specification of their statistical assumptions – as in any formal system, the conclusions are only as good as the assumptions. It is important for the analyst to make only assumptions that are reasonable, defensible, or otherwise tenable (e.g., because it can be demonstrated that the conclusions are invariant under multiple sets of assumptions). This can be challenging to researchers accustomed to statistical analyses that work out-of-the-box and do not appear to demand such efforts, but classical methods are equally or more assumptive, just not transparently so.

How to represent the prior probability of the hypothesis, $P(\text{Hypothesis})$, is often the most contentious in model comparison exercises. Scientists conduct their research to determine the probability that a hypothesis is true (or false), so quantifying that probability before they start is sometimes difficult, particularly if it is an unfamiliar exercise. Of course, there is nothing illogical about factoring prior information into our ultimate evaluation of a hypothesis, but sometimes that information is difficult to quantify. In such cases, it might be desirable to instead limit the scope of the calculation and assess first only how much is learned from the data at hand. This is where a new quantity, the Bayes factor, becomes useful.

Consider the case where there are two competing hypotheses, H_a and H_o, and where we have some relevant data D. For this case, there will exist two posterior probabilities: $P(H_a \mid D)$ and $P(H_o \mid D)$. A handy way of expressing which hypothesis is more likely than the other is the posterior odds $P(H_a \mid D)/P(H_o \mid D)$. Using Bayes theorem, we know that $P(H_a \mid D) = P(D \mid H_a) \times P(H_a)/P(D)$. Substituting Ho into the same equation and dividing the two gives us the "odds form" of Bayes theorem:

$$\underbrace{\left(\frac{P(H_a \mid D)}{P(H_o \mid D)}\right)}_{\text{Posterior odds}} = \underbrace{\left(\frac{P(H_a)}{P(H_o)}\right)}_{\text{Prior odds}} \times \underbrace{\left(\frac{P(D \mid H_a)}{P(D \mid H_o)}\right)}_{\text{Bayes factor}}.$$

We can read this expression in an intuitive way: the relative posterior probability of two hypotheses is their relative prior probability multiplied by the relative strength of evidence provided by the data. This relative strength of evidence is the ratio of the predictive success of the two hypotheses and is called the Bayes factor. In other words, how much better each hypothesis predicts the observed data determines how much more we believe in one than the other after seeing the data.

Mathematical likelihood. The Bayes Factor is the ratio of two probabilities that are important to understand on a deeper level. They are derived from calculating the direct probability of the data under a given model, $P(\text{Data} \mid \text{Hypothesis})$, which is also the basis of the likelihood function (Etz, 2018; Goodman & Royall, 1988; Royall, 1997), sometimes written as $L(\text{Hypothesis} \mid \text{Data})$. The reason we rewrite it in that way is that the likelihood function treats the data we observe as fixed and varies the parameter of interest of the probability model generating the data, whereas a probability density function holds the parameters fixed and calculates the probability of all possible data. We interpret the probability of the observed data under a given model as the support the data give to that model, captured in the simple relationship:

$$L(\text{Hypothesis} \mid \text{Data}) = C \times P(\text{Data} \mid \text{Hypothesis}).$$

The likelihood function has two critical properties that differ from p-values. First, it uses to the probability of the data in hand, not the often unknowable "more extreme data" used in frequentist methods. Second, for inferential purposes, it is always used in a comparative fashion, as in the Bayes factor above. The arbitrary constant C cancels when ratios are taken and shows that the likelihood has no unique value. So instead of being a probability under only one hypothesis (e.g. p-values), we compare instead how well two hypotheses predict the observed data; it is the relative support given to different hypotheses that is interpreted as evidence, not the degree to which the data are incompatible with just one hypothesis. Finally, likelihood functions provide a formal framework (viz, Bayes theorem) for interpreting their inferential meaning, whereas frequentist methods do not.

Bayesian testing with likelihood ratios. Consider the case of bistable perception. In this phenomenon, a single perceptual stimulus can be seen or heard two different ways. A figure might look like a vase one moment but look like two faces the next moment; or a drawing might look like a duck one second, a rabbit the next. Some ambiguous percepts differ between individuals: a dress in a photograph might appear blue and black to some people, gold and white to others; or a sound clip might sound like "YANNY" to some and like "LAUREL" to others.[2] Suppose that a researcher claims that teenagers are more likely to hear YANNY than LAUREL – three times more likely, in fact (i.e., 75% chance of YANNY). Another researcher claims there is no preference (i.e., 50% chance of YANNY). Because both of these claims are quite specific, let us call

[2] This example is based on a real debate caused by an audio clip that went viral on social media in the spring of 2018. In the audio clip, a male voice is clearly heard saying "LAUREL," but many perceived it as saying "YANNY." The clip, some variations, and background, can be found on CNN (2018, May 16).

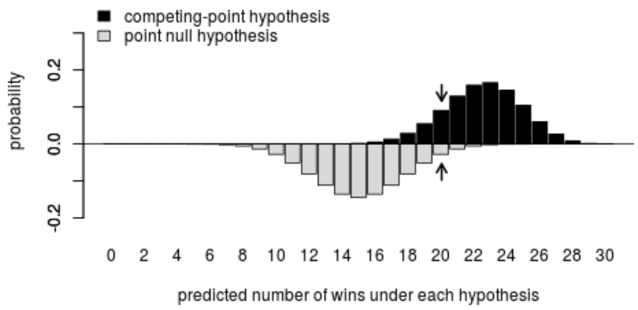


Fig. 1. An illustration of the computation of the Bayes factor using visualizations from Etz et al. (2018). Shown are the predictive distributions of two competing hypotheses: one 'point null hypothesis' under which some event happens with a 50% probability (lighter bars, upside-down) and one 'competing-point hypothesis' under which it happens with a 75% probability (darker bars, upright). The predictions are for an experiment with 30 trials in which the event can either occur or not. The arrows indicate the case where the event happened 20 times out of 30. This outcome is predicted with a probability of $.0280$ under the point null hypothesis and with a probability of $.0909$ under the competing-point hypothesis. The Bayes factor between these two models is simply the ratio of these probabilities, here 3.247.

the latter claim the "point null hypothesis" and the former claim the "competing-point hypothesis." Further suppose that the researchers collected data from 30 teenagers and found that $Y = 20$ heard YANNY while $L = 10$ heard LAUREL.

We can now calculate how strongly either hypothesis had predicted this outcome. In both cases, the probabilities are obtained with the binomial formula. For the point null hypothesis H_p, the probability is

$$\begin{aligned} P(Y = 20, L = 10 \mid H_p) &= \binom{Y+L}{Y} \times 0.50^{Y} \times 0.50^{L} \\ &= \binom{30}{20} \times 0.50^{20} \times 0.50^{10} \\ &= 0.0280, \end{aligned}$$

while for the competing-point hypothesis H_c, the probability is

$$\begin{aligned} P(Y = 20, L = 10 \mid H_c) &= \binom{30}{20} \times 0.7520 \times 0.2510 \\ &= 0.0909. \end{aligned}$$

Hence, the competing-point hypothesis is supported more strongly by these observations by a factor of $.0909/.0280 = 3.247$. Figure 1 illustrates this comparison graphically.

A Bayes factor of 3.247 is generally considered to be only weak evidence (Goodman, 1999b; Kass & Raftery, 1995; Wagenmakers, Marsman, et al., 2018). Combined with a perfectly ambivalent prior (50% on either claim, or a prior ratio of 1), this Bayes factor brings us to a posterior probability of only about 76% in favor of the competing-point hypothesis – not a very high probability.

Bayesian estimation. In the Bayesian framework, the distinction between testing and estimation is less clear-cut than it is in the frequentist framework. It is useful to think of the two practices as the ends of a continuum. The continuum captures how many possible states of the world are being considered. If we are interested in the probability θ that a coin comes up heads, we might limit our possible hypotheses to $A : \theta = 0.5$ and $B : \theta = 1.0$. This has all the bearing of a testing scenario. Alternatively, we might consider $A : \theta = 0.0$, $B : \theta = 0.5$, $C : \theta = 1.0$, which still has the appearance of a testing context. However, if we permit that θ might be any of $(0.01, 0.02, \ldots, 0.99, 1.00)$, then it is less clear if we are estimating a parameter or selecting between 101 models. If we allow θ to be anywhere from 0.5 to 1.0, or from 0.0 to 1.0, then we are more obviously dealing with an estimation scenario. Hence, the Bayesian estimation task can be seen an extension of Bayesian hypothesis testing, in which truth value is reallocated among many possible parameter values.

An example of Bayesian estimation. Behavioral researchers rarely have strong quantitative theories that permit statements such as "the probability that a teenager will hear YANNY is 75%." Instead, much behavioral research is conducted in a context of discovery: we seek to quantify effect sizes or estimate parameters (Cumming, 2014), rather than to discriminate between a set of competing theoretical accounts. The simplest way to illustrate Bayesian estimation is to use conjugate families of distributions. A prior distribution and likelihood for the data are said to be conjugate when the resulting posterior distribution is in the same class as the prior distribution. For example, updating a normal prior distribution with normally distributed data results in a normal posterior distribution with a new mean and standard deviation. In what follows we will illustrate conjugate updating and estimation using the conjugate family that includes beta prior distributions and binomial likelihood functions.

Continuing with the bistable perception phenomenon, we might consider two researchers interested in estimating the fraction θ of teenagers who hear YANNY versus LAUREL. The two researchers, independently from one another, retrieve the data from the previous example (a sample of 30 teenagers, 20 of whom hear YANNY) and use it to estimate θ. However, the two researchers differ in their prior conceptions of this proportion. Researcher 1 believes that teenagers are relatively homogeneous and will to a large extent either all hear LAUREL or all hear YANNY (i.e., θ will be close to 0% or 100%). Researcher 2 believes that the population is more likely to be split, and θ is most likely close to 50%. This difference in prior beliefs is displayed with the dashed lines in both panels of Figure 2. Researcher 1's prior is well captured by a beta$(0.5, 0.5)$ distribution while Researcher 2's prior is best described with a beta$(2, 2)$ distribution. Finding the set of prior parameters that best captures one's prior beliefs is known as prior elicitation. In practice this is often a matter of experience, but intuitions about the effect of parameter changes can be built by visualizing the prior density, by experimenting with its effect on the model's data predictions, and through mathematical analysis – an example of that is given below and Kadane and Wolfson (1998) provide a comprehensive review.

The beta(a, b) prior is defined as

$$P(\theta \mid a, b) = \theta^{(a-1)}(1-\theta)^{(b-1)} \times C_{\text{prior}}$$

where C_{prior} is a scaling parameter independent of θ that ensures the function describes a proper distribution (i.e., one whose mass totals 1). The interpretation of the parameters a and b will be revealed once we compute the posterior distribution.

To obtain the posterior distribution of θ, we multiply the prior with the binomial likelihood of the data, which is:

$$P(Y, L \mid \theta) = \theta^{Y} \times (1-\theta)^{L} \times C_{\text{likelihood}},$$

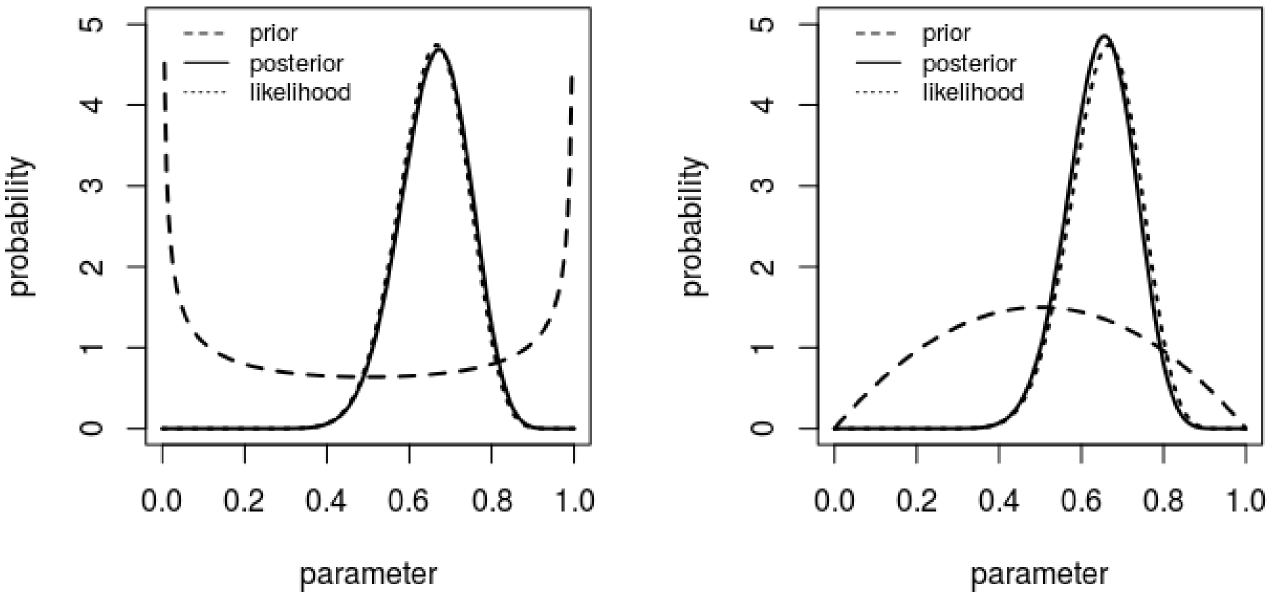


Fig. 2. Prior and posterior distributions of two independent researchers seeing the same data. Researcher 1 (left) expects the ratio parameter to be extreme; either close to 0 or close to 1. Researcher 2 (right) expects the parameter to be closer to $.5$. Both researchers observe the same data: a sample of 30 yields 20 observations in one category and 10 in the other. When these data are factored in, the differences between the two researchers dissipate – even the dramatic difference in priors is easily overwhelmed by a modest amount of data.

where we again capture all factors that do not contain θ into a single scaling parameter. The posterior is then, by Bayes theorem:

$$\begin{aligned} P(\theta \mid Y, L) &= P(Y, L \mid \theta) \times P(\theta \mid a, b) \times C_{\text{posterior}} \\ &= \left[\theta^{Y} (1-\theta)^{L}\right] \times \left[\theta^{a-1}(1-\theta)^{b-1}\right] \times C_{\text{posterior}}. \end{aligned}$$

Some algebraic rearrangement yields

$$\begin{aligned} P(\theta \mid Y, L) &= \theta^{a-1+Y}(1-\theta)^{b-1+L} \times C_{\text{posterior}} \\ &= \theta^{a'-1}(1-\theta)^{b'-1} \times C_{\text{posterior}}, \end{aligned}$$

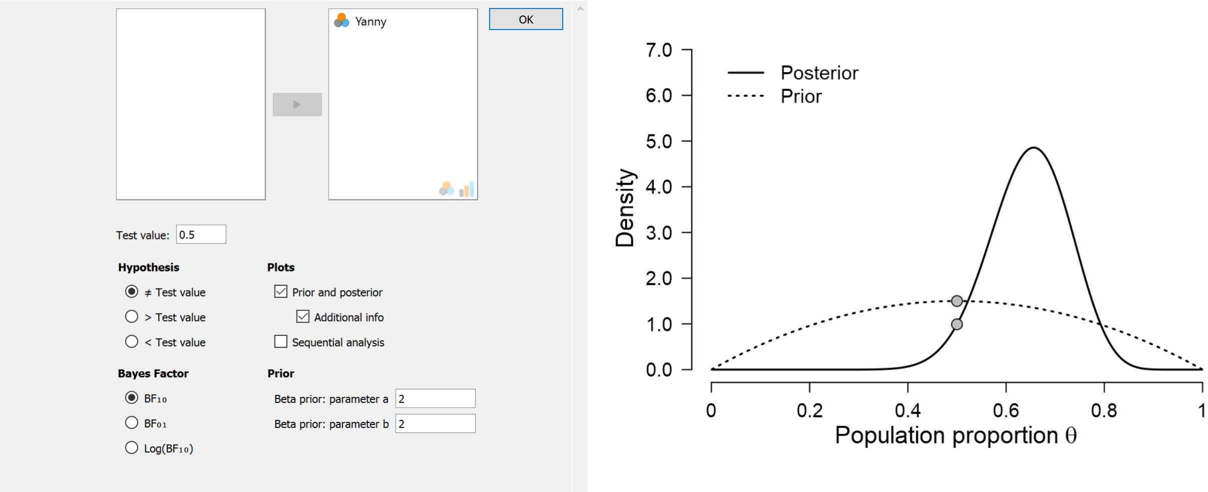
where we have first collected all scaling factors into a new factor $C_{\text{posterior}}$ and then introduced the updated parameters $a' = a + Y$ and $b' = b + L$. This rearrangement illustrates the conjugacy of the beta prior and the binomial likelihood: the posterior distribution of θ again follows a beta distribution. Due to the conjugacy, adding further observations is easy: simply increment a' with the number of new YANNY observations and increment b' with the number of new LAUREL observations.

The way in which a and b absorb the number of observations of each type also reveals an interesting interpretation of these parameters: the value of a and b prior to seeing the data can be interpreted as the (possibly hypothetical) number of times the researcher had already observed YANNY and LAUREL occurrences (respectively). The "effective prior sample size" $a + b$ expresses the strength of the available prior information. Note that this remains true if we were to add a second batch of observations: the effective sample size after the first batch is $a' + b'$, and upon observing new data (Y_2, L_2) the new parameters would be $a'' = a' + Y_2$ and $b'' = b' + L_2$. Updating the probability density with new data is a matter of incrementing the parameters of the distribution and (in this case) does not require complex mathematical exercises.

It is also easy to see how the data will quickly overwhelm the prior: Researcher 1 has the equivalent prior information of one observation and Researcher 2 has the equivalent of four observations. These quickly pale when incremented by 30 observations.

With the parameters of the posterior distribution $P(\theta \mid Y, L)$ in hand, we can now compute a number of interesting quantities, such as the most "plausible" value of θ (the posterior mode): $(a' - 1)/(a' + b' - 2)$, which is $.672$ for Researcher 1 and $.656$ for Researcher 2. We could also compute the posterior probability that $\theta > .5$, which is $.970$ for Researcher 1 and $.960$ for Researcher 2. Here, again, the dramatic difference in the prior distribution makes little difference in the ultimate quantities of interest. Both researchers conclude that θ is close to 2/3rd and is very likely greater than one half.

Software. Some of the most common Bayesian methods require no more computational effort than standard approaches. To illustrate the use of Bayesian computation we will recreate the estimation analysis above using the software JASP (JASP Team, 2018). JASP is a statistical program with a graphical user interface, meaning no knowledge of scripting or coding is necessary to perform a Bayesian analysis. We have created a data file containing 20 YANNY and 10 LAUREL responses, available for download at `https://osf.io/ksvdp/`. If we open this file in JASP and select "Bayesian binomial test" from the frequencies drop-down button we are brought to an options menu. In this menu we can specify



Test value: 0.5

Hypothesis

● ≠ Test value
○ > Test value
○ < Test value

Plots

☑ Prior and posterior
☑ Additional info
□ Sequential analysis

Bayes Factor

● BF_{10}
○ BF_{01}
○ Log(BF_{10})

Prior

Beta prior: parameter a 2
Beta prior: parameter b 2

Fig. 3. A Bayesian binomial test in JASP. **Left:** The interface for the Bayesian binomial test. **Right:** Default output from the test applied to the YANNY/LAUREL data set.

that the success counts are in the YANNY column of the data, and also specify that we wish to use a beta$(2, 2)$ prior distribution for the success parameter(indicating that we expect the population to be split in two groups, as in the previous section). JASP will then generate the results of the Bayesian analysis in the right-most panel, in the form of a plot of the prior and posterior distributions of the probability of success, which we present in Figure 3 (right panel). Note that this posterior distribution exactly matches that from the right panel of Figure 2. JASP provides a 95% credible interval for the parameter, which in this case ranges from $.482$ to $.796$.

Whereas Bayesian analysis of common designs is possible in software such as JASP, Bayesian calculation for complex modeling is often computationally intensive. Fortunately, user-friendly tools for Bayesian analysis have emerged in recent years and have been incorporated into virtually all standard statistical software (e.g., Stata, SPSS, SAS, and R), as well as in new specialized software specifically for Bayesian analyses (e.g., JASP). Introductions to the use of general-purpose Bayesian software can be found in Matzke, Boehm, and Vandekerckhove (2018), van Ravenzwaaij, Cassey, and Brown (2018), and Wagenmakers, Love, et al. (2018), but more tutorials appear on a regular basis.

Conclusion. It is not critical for the entire analytic approach of the behavioral sciences to move to Bayesianism for bad inferential practices to be avoided. It is interesting to look at the methodological evolution within biomedicine, which has not given up frequentist methods, but has avoided some of the particularly egregious practices seen in the behavioral sciences. Most importantly, in clinical biomedicine there is a culture of disbelief in small, one-off single studies, particularly small ones, and knowledge is not regarded as established until a sufficiently large body of data collection of studies generates convincing evidence, typically as shown in systematic reviews and meta-analyses. Poorly informative, underpowered studies are strongly disfavored at the major journals, and strong emphasis is put on estimation together with testing, particularly for nonsignificant studies with equivocal findings. Study protocols are now routinely requested by the major journals, and the law requires that randomized trials must be pre-registered at `clinicaltrials.gov` within 21 days from when the first patient is enrolled, and the RCT results reported in `clinicaltrials.gov` regardless of outcome, with government penalties for noncompliance.

This is not to suggest that the field of clinical research has solved or avoided all of the issues of research integrity or proper statistical design and interpretation that are now plaguing the behavioral and social sciences. As a field, it went down similar paths of awareness and reform starting three to four decades ago and has since adopted a number of practices that have blunted some of the worst potential effects of frequentist philosophy and methods. That said, problems remain. Interestingly, many of the innovations currently being suggested for the behavioral sciences are now being adopted within the biomedical sciences as ways to accelerate progress there, particularly the move to open science.

Short of moving completely to a Bayesian paradigm, major progress can be made within the current frequentist paradigm by eliminating claims based only on statistical "bright lines" (i.e. significance), understanding that marginal significance (e.g. $.01 \leq p \leq .05$) represents fairly weak statistical evidence, moving to a cumulative evidence model, and basing conclusions more on confidence intervals than on "testing."

Changes in the practices of an entire discipline require far more than a change in analytic philosophy; these must be accompanied by changes in education, professional norms and expectations, funding, promotion criteria, and publication. But understanding how the dominant analytic philosophy contributed to some of the most harmful practices is critical to making the right changes and thereby improve the trust that those inside or outside the behavioral sciences put in the field and its findings.

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References

Aldrich, J. (2008). R. A. Fisher on Bayes and Bayes' theorem. *Bayesian Analysis*, *3*(1), 161–170.

Altman, D., Machin, D., Bryant, T., & Gardner, M. (2013). *Statistics with confidence: confidence intervals and statistical guidelines*. John Wiley & Sons.

Berger, J. O., & Wolpert, R. L. (1988). *The likelihood principle (2nd ed.)*. Hayward, CA: Institute of Mathematical Statistics.

Carnap, R. (1950). *Logical foundations of probability*. Chicago: The University of Chicago Press.

CNN. (2018, May 16). *Laurel or yanny? what science has to say.* (Retrieved from https://www.cnn.com/2018/05/15/health/yanny-laurel-audio-social-media-trnd/)

Cowles, M., & Davis, C. (1982). On the origins of the .05 level of statistical significance. *American Psychologist*, *37*(5), 553–558.

Cumming, G. (2014). The new statistics: Why and how. *Psychological Science*, *25*, 7–29.

Devlin, K. (2010). *The unfinished game: Pascal, Fermat, and the seventeenth-century letter that made the world modern*. Basic Books.

Diaconis, P., & Freedman, D. (1986). On the consistency of Bayes estimates. *The Annals of Statistics*, *14*, 1–26.

Edwards, W., Lindman, H., & Savage, L. J. (1963). Bayesian statistical inference for psychological research. *Psychological Review*, *70*, 193–242.

Etz, A. (2018). Introduction to the concept of likelihood and its applications. *Advances in Methods and Practices in Psychological Science*, *1*(1), 60–69.

Etz, A., Haaf, J. M., Rouder, J. N., & Vandekerckhove, J. (2018). Bayesian inference and testing any hypothesis you can specify. *Advances in Methods and Practices in Psychological Science*, *1(2)*, 281–295.

Etz, A., & Vandekerckhove, J. (2018). Introduction to Bayesian inference for psychology. *Psychonomic Bulletin & Review*, *25*, 5–34.

Evans, M. (2015). *Measuring statistical evidence using relative belief*. CRC Press.

Fienberg, S. E. (2006). When did Bayesian inference become "Bayesian"? *Bayesian analysis*, *1*(1), 1–40.

Fisher, R. A. (1925). *Statistical methods for research workers (first edition)*. Edinburgh: Oliver and Boyd.

Fisher, R. A. (1926). The arrangement of field experiments. *Journal of the Ministry of Agriculture*, *33*, 503–513.

Fisher, R. A. (1971). *The design of experiments* (7th ed.). New York: Hafner.

Gardner, M. J., & Altman, D. G. (1986). Confidence intervals rather than p-values: estimation rather than hypothesis testing. *Br Med J (Clin Res Ed)*, *292*(6522), 746–750.

Gelman, A., & Nolan, D. (2002). You can load a die, but you can't bias a coin. *The American Statistician*, *56*(4), 308–311.

Ghosh, J. K. (Ed.). (1988). *Statistical information and likelihood. A collection of critical essays by Dr. D. Basu*. Springer New York. doi:

Gigerenzer, G. (1993). The Superego, the Ego, and the Id in statistical reasoning. In G. Keren & C. Lewis (Eds.), *A handbook for data analysis in the behavioral sciences: Methodological issues* (pp. 311–339). Hillsdale (NJ): Erlbaum.

Gigerenzer, G. (2004). Mindless statistics. *The Journal of Socio–Economics*, *33*, 587–606.

Goodman, S. N. (1993). P values, hypothesis tests, and likelihood: Implications for epidemiology of a neglected historical debate. *American Journal of Epidemiology*, *137*, 485–496.

Goodman, S. N. (1999a). Toward evidence-based medical statistics. 1: The p-value fallacy. *Annals of Internal Medicine*, *130*, 995–1004.

Goodman, S. N. (1999b). Toward evidence-based medical statistics. 2: The Bayes factor. *Annals of Internal Medicine*, *130*, 1005–1013.

Goodman, S. N., Fanelli, D., & Ioannidis, J. P. (2016). What does research reproducibility mean? *Science translational medicine*, *8*(341), 341ps12.

Goodman, S. N., & Royall, R. (1988). Evidence and scientific research. *American Journal of Public Health*, *78*(12), 1568–1574.

Greenland, S., Senn, S. J., Rothman, K. J., Carlin, J. B., Poole, C., Goodman, S. N., & Altman, D. G. (2016). Statistical tests, p-values, confidence intervals, and power: a guide to misinterpretations. *European journal of epidemiology*, *31*(4), 337–350.

Hald, A. (2008). *A history of parametric statistical inference from Bernoulli to Fisher, 1713-1935*. Springer Science & Business Media.

Haller, H., & Krauss, S. (2002). Misinterpretations of significance: A problem students share with their teachers? *Methods of Psychological Research*, *7*, 1–20.

Hoekstra, R., Morey, R. D., Rouder, J. N., & Wagenmakers, E.-J. (2014). Robust misinterpretation of confidence intervals. *Psychonomic Bulletin & Review*, *21*, 1157–1164.

JASP Team. (2018). *JASP (Version 0.9)[Computer software]*. Retrieved from https://jasp-stats.org/

Jeffreys, H. (1961). *Theory of probability* (3rd ed.). Oxford, UK: Oxford University Press.

Kadane, J., & Wolfson, L. J. (1998). Experiences in elicitation. *Journal of the Royal Statistical Society: Series D (The Statistician)*, *47*(1), 3–19.

Kass, R. E., & Raftery, A. E. (1995). Bayes factors. *Journal of the American Statistical Association*, *90*, 773–795.

Kendall, M. G. (1949). On the reconciliation of theories of probability. *Biometrika*, *36*, 101–116.

Keynes, J. M. (1921). *A treatise on probability*. London: Macmillan & Co.

Lindley, D. V. (1993). The analysis of experimental data: The appreciation of tea and wine. *Teaching Statistics*, *15*, 22–25.

Matzke, D., Boehm, U., & Vandekerckhove, J. (2018). Bayesian inference for psychology. Part III: Parameter estimation in nonstandard models. *Psychonomic Bulletin & Review*, *25*, 77–101.

Morey, R. D., Hoekstra, R., Rouder, J. N., Lee, M. D., & Wagenmakers, E.-J. (2016). The fallacy of placing confidence in confidence intervals. *Psychonomic Bulletin & Review*, *23*, 103–123.

Neyman, J. (1937). Outline of a theory of statistical estimation based on the classical theory of probability. *Philosophical Transactions of the Royal Society of London, Series A, Mathematical and Physical Sciences*, *236*, 333–380.

Neyman, J., & Pearson, E. S. (1928). On the use and interpretation of certain test criteria for purposes of statistical inference: Part I. *Biometrika*, *20A*, 175–240.

Neyman, J., & Pearson, E. S. (1933). On the problem of the most efficient tests of statistical hypotheses. *Philosophical Transactions of the Royal Society A*, *231*, 289–337.

Oakes, M. W. (1986). *Statistical inference*. Epidemiology Resources.

Pashler, H., & Wagenmakers, E.-J. (2012). Editors' introduction to the special section on replicability in psychological science: A crisis of confidence? *Perspectives on Psychological Science*, *7*, 528–530.

Rouder, J. N., & Morey, R. D. (2019). Teaching Bayes' theorem: Strength of evidence as predictive accuracy. *The American Statistician*, *73*, 186–190.

Royall, R. M. (1997). *Statistical evidence: A likelihood paradigm*. London: Chapman & Hall.

Schervish, M. J. (1996). P values: What they are and what they are not. *The American Statistician*, *50*, 203–206.

van Ravenzwaaij, D., Cassey, P., & Brown, S. D. (2018). A simple introduction to Markov chain Monte–Carlo sampling. *Psychonomic Bulletin & Review*, *25*, 143–154.

Vandekerckhove, J., Rouder, J. N., & Kruschke, J. K. (Eds.). (2018). Editorial: Bayesian methods for advancing psychological science. *Psychonomic Bulletin & Review*, *25*, 1–4.

von Mises, R. (1957). *Probability, statistics, and truth*. Dover Publications (reprint, original Springer: 1928), New York.

Wagenmakers, E.-J., Gronau, Q. F., Dablander, F., & Etz, A. (in press). The support interval. *Erkenntnis*. Retrieved from https://doi.org/10.1007/s10670-019-00209-z doi:

Wagenmakers, E.-J., Love, J., Marsman, M., Jamil, T., Ly, A., Verhagen, A. J., . . . Morey, R. D. (2018). Bayesian inference for psychology. Part II: Example applications with JASP. *Psychonomic Bulletin & Review*, *25*, 58–76.

Wagenmakers, E.-J., Marsman, M., Jamil, T., Ly, A., Verhagen, A. J., Love, J., . . . Morey, R. D. (2018). Bayesian inference for psychology. Part I: Theoretical advantages and practical ramifications. *Psychonomic Bulletin & Review*, *25*, 35–57.

Wasserstein, R. L., & Lazar, N. A. (2016). The ASA's statement on p-values: Context, process, and purpose. *The American Statistician*, *70*, 129–133.

Wrinch, D., & Jeffreys, H. (1919). On some aspects of the theory of probability. *Philosophical Magazine*, *38*, 715–731.

Zabell, S. (1989). R. A. Fisher on the history of inverse probability. *Statistical Science*, 247–256.