

Why optional stopping is a problem for Bayesians*

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Abstract

Recently, optional stopping has been a subject of debate in the Bayesian psychology community. Rouder (2014) argues that optional stopping is no problem for Bayesians, and even recommends the use of optional stopping in practice, as do Wagenmakers et al. (2012). This article addresses the question whether optional stopping is problematic for Bayesian methods, and specifies under which circumstances and in which sense it is and is not. By slightly varying and extending Rouder’s (2014) experiment, we illustrate that, as soon as the parameters of interest are equipped with default or pragmatic priors — which means, in most practical applications of Bayes factor hypothesis testing — resilience to optional stopping can break down. We distinguish between four types of default priors, each having their own specific issues with optional stopping, ranging from no-problem-at-all (Type 0 priors) to quite severe (Type II and III priors).

1 Introduction

P-value based null-hypothesis significance testing (NHST) is widely used in the life and behavioral sciences, even though the use of p -values has been severely criticized for at least the last 50 years. During the last decade, within the field of psychology, several authors have advocated the Bayes factor as the most principled alternative to resolve the problems with p -values. Subsequently, these authors have made an admirable effort to provide practitioners with *default Bayes factors* for common hypothesis tests (Rouder et al. (2009); Jamil et al. (2016); Rouder et al. (2012) and many others).

We agree with the objections against the use of p -value based NHST and the view that this paradigm is inappropriate (or at least far from optimal) for scientific research, and we agree that the Bayes factor has many advantages. However (cf. also Gigerenzer and Marewski (2014)), it is not the panacea for hypothesis testing that a lot of articles make it appear. The Bayes factor has its limitations, and it seems that the subtleties of when those limitations apply sometimes get lost in the overwhelming effort to provide a solution to the pervasive problems of p -values.

In this article we elucidate the intricacies of handling optional stopping with Bayes factors, primarily in response to Rouder (2014). *Optional stopping* refers to ‘looking at the results so far to decide whether or not to gather more data’, and it is a desirable property of a hypothesis test to be able to *handle optional stopping*. The key question is whether Bayes factors can or cannot handle

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optional stopping. Yu et al. (2014), Sanborn and Hills (2014) and Rouder (2014) tried to answer this question from different perspectives and with different interpretations of the notion of handling optional stopping. Rouder (2014) demonstrates on the basis of computer simulations that optional stopping is not a problem for Bayesians, also citing Lindley (1957) and Edwards et al. (1963) who provide mathematical results to a similar effect. Rouder used the simulations to concretely illustrate more abstract mathematical theorems; these theorems are indeed formally proven by Deng et al. (2016) and, in a more general setting, by Hendriksen et al. (2018). Other early work indicating that optional stopping is not a problem for Bayesians includes (Savage, 1972) and (Good, 1991); a recent simulation study reporting good results for Bayesian inference under optional stopping is (Schönbrodt et al., 2017). We briefly return to all of these in Section 5.

All this earlier work notwithstanding, we maintain that optional stopping can be a problem for Bayesians — at least for *pragmatic Bayesians* who are willing to use so-called ‘default’, ‘objective’ or ‘convenience’ priors, and who think that it is meaningful to perform robustness analyses, where one checks what happens with the results of an analysis if one varies the prior, or checks what happens if data are sampled from a fixed distribution within a region that has nonnegligible prior density. Only a subjective Bayesian who uses a prior that perfectly, or at least pretty adequately represents her beliefs concerning all parameters of interest in her models, can really say that she can fully handle optional stopping. ‘Perfect representation of beliefs’ means that she would be willing to accept certain quite specific bets about data — see Section 5. In practice, most Bayesians use ‘objective’ and ‘pragmatic’ priors that do not directly translate into a willingness to bet. Such priors play a central role in the Bayes factor approach to testing that has recently become popular in the psychology literature, so there is a real issue here.

Rouder (2014) was written mainly in response to Yu et al. (2014), and one of his main goals was to show that Bayesian procedures retain a clear interpretation under optional stopping. The main content of this article is to repeat variations of his simulations for common testing scenarios involving default priors. We shall encounter three types of default priors, and we shall see that Rouder’s check whether a Bayesian procedure can deal with optional stopping — which one may call *prior-based calibration* — while indeed having a clear interpretation whenever defined — is either of limited relevance (Type I priors) or undefined (Type II and III priors). We consider a strengthening of Rouder’s check which we call *strong calibration*, and which remains meaningful for all default priors. Then, however, we shall see that strong calibration breaks down under optional stopping for all default priors except, interestingly, for a special type of priors (which we call “Type 0 priors”) on a special (but common) type of nuisance parameters. Since these are rarely the only parameters incurring in one’s models, one has to conclude that optional stopping is usually a problem for pragmatic Bayesians — at least under Rouder’s definition of handling optional stopping. Two other popular definitions of handling optional stopping are roughly (see Section 5 for details) (a) ‘the procedure for testing does not depend on the stopping rule used, i.e. it gives the same result for data x_1, \dots, x_n independently of whether n was fixed in advance or was determined based on the data itself’ and (b) the (Bayesian) procedure handles optional stopping under a frequentist, non-Bayesian definition of ‘optional stopping’ (Wagenmakers, 2007, Appendix). Under the (not-so-stringent) definition (a), only Type II and III priors constitute a problem. Under definition (b), there can be problems with Type I priors as well, but the extent of these problems varies from situation to situation; for example, Schönbrodt et al. (2017) show that in a certain default Bayesian setting (the Bayesian t -test), under a certain interpretation of the results, the Bayes factor with optional stopping can (the above problems notwithstanding) still be preferable to standard p -value based NHST in a frequentist sense, as we discuss in Section 5. In that section we also explain why

for fully subjective Bayesians, the situation is better than for objective and pragmatic Bayes — although not entirely unproblematic. As explained in the conclusion (Section 6), the crux is that default and pragmatic priors represent *tools* for inference just as much or even more than *beliefs* about the world, and should thus be equipped with a precise prescription as to what type of inferences they can and cannot be used for. A first step towards implementing this radical idea is given by one of us in the recent paper *Safe Probability* (Grünwald, 2018).

Readers who are familiar with Bayesian theory will not be too surprised by our conclusions: It is well-known that what we call Type II and Type III priors violate the *likelihood principle* (Berger and Wolpert, 1988) and/or lead to (mild) forms of *incoherence* (Seidenfeld, 1979) and, because of the close connection between these two concepts and optional stopping, it should not be too surprising that issues arise. Yet it is still useful to show how these issues pan out in simple computer simulations, especially given the apparently common belief that optional stopping is *never* a problem for Bayesians. The simulations will also serve to illustrate the difference between the subjective, pragmatic and objective views of Bayesian inference, a distinction which matters a lot and which, we feel, has been underemphasized in the psychology literature — our simulations may in fact serve to help the reader decide what viewpoint he or she likes best.

In Section 2 we explain important concepts of Bayesianism and Bayes factors. Section 3 explains, repeats and extends Rouder’s optional stopping simulations and shows the sense in which optional stopping is indeed not a problem for Bayesians. Section 4 then contains additional simulations indicating the problems with default priors as summarized above. In Section 5 we discuss conceptualizations of ‘handling optional stopping’ that are different from Rouder’s; this includes an explication of the purely subjective Bayesian viewpoint as well as an explication of a frequentist treatment of handling optional stopping, which only concerns sampling under the null hypothesis. We illustrate that some (not all!) Bayes factor methods can handle optional stopping in this frequentist sense. We conclude with a discussion of our findings in Section 6. In the appendix we provide details about an additional simulation with discrete data that confirms our findings.

2 Bayesian probability and Bayes factors

Bayesianism is about a certain interpretation of the concept *probability*: as *degrees of belief*. Wagenmakers (2007) and Rouder (2014) give an intuitive explanation for the different views of frequentists and Bayesians in statistics, on the basis of coin flips. The frequentists interpret probability as a limiting frequency. Suppose we flip a coin many times, if the probability of heads is $3/4$, we see a proportion of $3/4$ of all those coin flips with heads up. Bayesians interpret probability as a degree of belief. If an agent believes the probability of heads is $3/4$, she believes that it will be 3 times more likely that the next coin flip will result in heads than tails; we return to the operational meaning of such a ‘belief’ in terms of betting in Section 5.

A Bayesian first expresses this belief as a probability function. In our coin flipping example, it might be that the agent believes that it is more likely that the coin is biased towards heads, which the probability function thus reflects. We call this the *prior distribution*, and we denote¹ it by $\mathbb{P}(\theta)$, where θ is the parameter (or several parameters) of the model. In our example, θ expresses the

¹With some abuse of notation, we use \mathbb{P} both to denote a generic probability distribution (defined on sets), and to denote its associated probability mass function and a probability density function (defined on elements of sets); whenever in this article we write $\mathbb{P}(z)$ where z takes values in a real-valued scalar or vector space, this should be read as $f(z)$ where f is the density of \mathbb{P} .

bias of the coin, and is a real number between 0 and 1. After the specification of the prior, we conduct the experiment and obtain the data D and the likelihood $\mathbb{P}(D|\theta)$. Now we can compute the *posterior distribution* $\mathbb{P}(\theta|D)$ with the help of *Bayes' theorem*:

$$\mathbb{P}(\theta|D) = \frac{\mathbb{P}(D|\theta)\mathbb{P}(\theta)}{\mathbb{P}(D)}. \quad (1)$$

Rouder (2014) and Wagenmakers (2007) provide a clear explanation of Bayesian hypothesis testing with Bayes factors (Jeffreys, 1961; Kass and Raftery, 1995), which we repeat here for completeness. Suppose we want to test a null hypothesis \mathcal{H}_0 against an alternative hypothesis \mathcal{H}_1 . A hypothesis can consist of a single distribution, for example: ‘the coin is fair’. We call this a *simple hypothesis*. A hypothesis can also consist of two or more, or even infinitely many hypotheses, which we call a *composite hypothesis*. An example is: ‘the coin is biased towards heads’, so the probability of heads can be any number between 0.5 and 1, and there are infinitely many of those numbers. Suppose again that we want to test \mathcal{H}_0 against \mathcal{H}_1 . We start with the so called *prior odds*: $\mathbb{P}(\mathcal{H}_1)/\mathbb{P}(\mathcal{H}_0)$, our belief before seeing the data. Let’s say we believe that both hypotheses are equally probable, then our prior odds are 1-to-1. Next we gather data D , and update our odds with the new knowledge, using Bayes’ theorem (Eq. 1):

$$\text{post-odds}|D = \frac{\mathbb{P}(\mathcal{H}_1|D)}{\mathbb{P}(\mathcal{H}_0|D)} = \frac{\mathbb{P}(\mathcal{H}_1)\mathbb{P}(D|\mathcal{H}_1)}{\mathbb{P}(\mathcal{H}_0)\mathbb{P}(D|\mathcal{H}_0)}. \quad (2)$$

The left term is called *posterior odds*, it is our updated belief about which hypothesis is more likely. Right of the prior odds, we see the *Bayes factor*, the term that describes how the beliefs (prior odds) are updated via the data. If we have no preference for one hypothesis and set the prior odds to 1-to-1, we see that the posterior odds are just the Bayes factor. If we test a composite \mathcal{H}_0 against a composite \mathcal{H}_1 , the Bayes factor is a ratio of two likelihoods in which we have two or more possible values of our parameter θ . Bayesian inference tells us how to calculate $\mathbb{P}(D|\mathcal{H}_j)$: we integrate out the parameter with help of a prior distribution $\mathbb{P}(\theta)$, and we write Eq. (2) as:

$$\text{post-odds}|D = \frac{\mathbb{P}(\mathcal{H}_1|D)}{\mathbb{P}(\mathcal{H}_0|D)} = \frac{\mathbb{P}(\mathcal{H}_1)\int_{\theta_1}\mathbb{P}(D|\theta_1)\mathbb{P}(\theta_1)d\theta_1}{\mathbb{P}(\mathcal{H}_0)\int_{\theta_0}\mathbb{P}(D|\theta_0)\mathbb{P}(\theta_0)d\theta_0} \quad (3)$$

where θ_0 denotes the parameter of the null hypothesis \mathcal{H}_0 , and similarly, θ_1 is the parameter of the alternative hypothesis \mathcal{H}_1 . If we observe a Bayes factor of 10, it means that the *change* in odds from prior to posterior in favour of the alternative hypothesis \mathcal{H}_1 is a factor 10. Intuitively, the Bayes factor provides a measure of whether the data have increased or decreased the odds on \mathcal{H}_1 relative to \mathcal{H}_0 .

3 Optional stopping: First Experiments

Validity under optional stopping is a desirable property of hypothesis testing: we gather some data, look at the results, and decide whether we stop or gather some additional data. Informally we call ‘peeking at the results to decide whether to collect more data’ *optional stopping*, but if we want to make more precise what it means if we say that a test can handle optional stopping, it turns out that different approaches (frequentist, subjective Bayesian and objective Bayesian) lead to different interpretations or definitions. In this section we adopt the definition of handling optional stopping that was used in Rouder’s simulations, and show, by repeating and extending Rouder’s original

simulation, that Bayesian methods do handle optional stopping in this sense. In the next section, we shall then see that for ‘default’ and ‘pragmatic’ priors used in practice, Rouder’s experimental setup may not always be appropriate and may even become undefined — indicating there are problems with optional stopping after all².

3.1 Example 0: Rouder’s example

We start by repeating Rouder’s (2014) second example, so as to explain his method and re-state his results. Suppose a researcher wants to test the null hypothesis \mathcal{H}_0 that the mean of a normal distribution is equal to 0, against the alternative hypothesis \mathcal{H}_1 that the mean distributed as a standard normal. We take the variance to be 1, such that the mean under \mathcal{H}_1 equals the effect size. We set our prior odds to 1-to-1: This expresses a priori indifference between the hypotheses, or a belief that both hypotheses are really equally probable. To see how this performs in practice, we first generate some data from the null hypothesis: 10 observations from a normal distribution with mean 0 and variance 1. Now we can observe the data and update our prior beliefs. We calculate the posterior odds via Eq. (2) for data $D = (x_1, \dots, x_n)$:

$$\text{post-odds}|D = \frac{1}{1} \cdot \frac{\exp\left\{\frac{n^2\bar{x}^2}{2(n+1)}\right\}}{\sqrt{n+1}} \quad (4)$$

where n is the sample size (10 in our case), and \bar{x} is the sample mean. We repeat this procedure 20,000 times, and we see the distribution of the posterior odds plotted as the blue histogram on the log scale in Figure 1a. We also sample data from the alternative distribution \mathcal{H}_1 : first we sample a mean from a standard normal distribution, and then we sample 10 observations from a normal distribution with this just obtained mean, and variance 1. Next, we calculate the posterior odds from Eq. (4). Again, we perform 20,000 replicate experiments of 10 samples each, and we obtain the pink histogram in Figure 1a. We see that for the null hypothesis, most samples favour the null (the values of the Bayes factor are smaller than 1), for the alternative hypothesis we see that the bins for higher values of the posterior odds are higher.

Calibration Rouder writes: ‘If a replicate experiment yielded a posterior odds of 3.5-to-1 in favour of the null, then we expect that the null was 3.5 times as probable as the alternative to have produced the data.’ That means that we look at a specific bin of the histogram, say at 3.5, i.e. the number of all the replicate experiments that yielded approximately a posterior odds of 3.5, and if the posterior odds are interpretable as claimed, then the bin from \mathcal{H}_1 should be about 3.5 times as high as the bin from \mathcal{H}_0 . Rouder calls the ratio of the two histograms the *observed posterior odds*: the ratio of the binned posterior odds counts we observe from the simulation experiments we did. What we expect the ratio to be for a certain value of the posterior odds, is what he calls the *nominal posterior odds*. We can plot the observed posterior odds as a function of the nominal posterior odds, and we see the result in Figure 1b. The observed values agree closely with the nominal values: all points lie within simulation error on the identity line. Henceforth, we call this phenomenon *calibration of the posterior odds*.

Rouder (2014) repeats this experiment under optional stopping: he ran a simulation experiment with exactly the same setup, except that in each of the 40,000 simulations, sampling occurred until the posterior odds were at least 10-to-1 for either hypothesis, unless a maximum of 25 samples

²In some cases though, it is impossible to carry out simulations, but calibration may remain, as Hendriksen et al. (2018) mathematically prove.

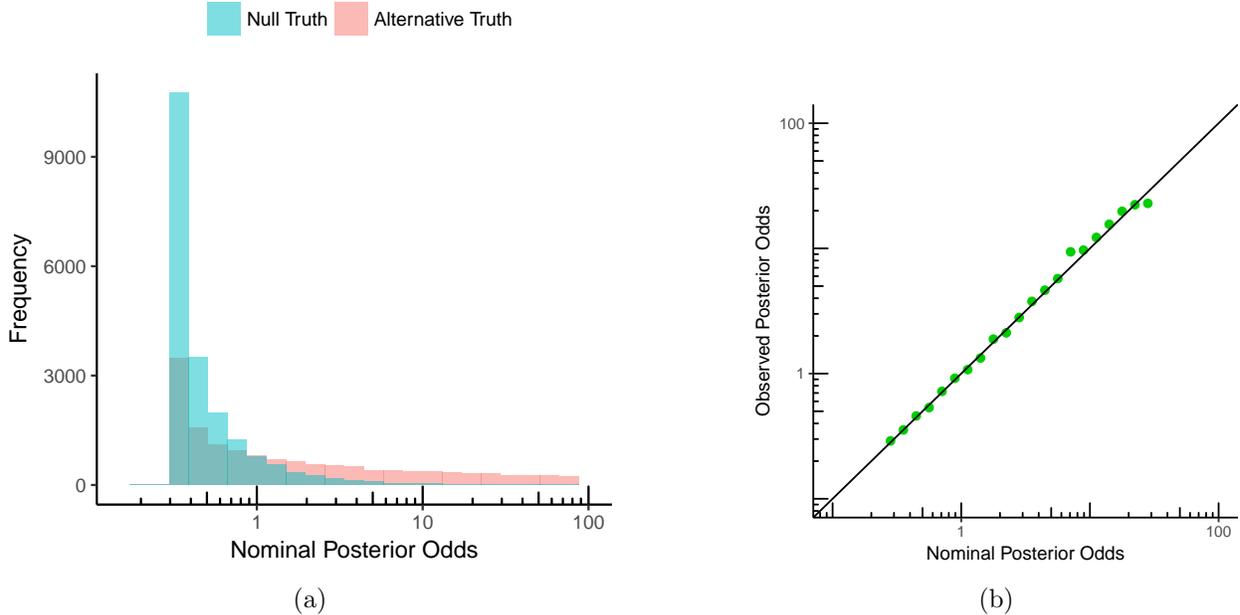


Figure 1: The interpretation of the posterior odds in Rouder’s experiment, from 20,000 replicate experiments. (a) The distribution of the posterior odds as a histogram under \mathcal{H}_0 and \mathcal{H}_1 . (b) Calibration plot: the observed posterior odds as a function of the nominal posterior odds.

was reached. This yielded a figure indistinguishable from Figure 1b, from which Rouder concluded that ‘the interpretation of the posterior odds holds with optional stopping’; in our language, *the posterior odds remain calibrated under optional stopping*. From this and similar experiments, Rouder concluded that Bayes factors still have a clear interpretation under optional stopping (we agree with this for what we call below Type 0 and I priors, not Type II and III), leading to the claim/title ‘optional stopping is no problem for Bayesians’ (for which we only agree for Type 0 and purely subjective priors).

3.2 Example 1: Rouder’s example with a nuisance parameter

We now adjust Rouder’s example to a case where the mean of the alternative hypothesis is distributed according to a normal distribution, but the variance σ^2 is unknown. Posterior calibration will still be obtained under optional stopping; the example mainly serves to gently introduce the notions of *improper prior* and *strong vs. prior calibration*, that will play a central role later on. Thus, \mathcal{H}_0 now expresses that the data are independently normally distributed with mean 0 and some unknown variance σ^2 , and \mathcal{H}_1 expresses that the data are normal with variance σ^2 , and mean distributed according to a normal distribution with mean zero and variance σ^2 as well (this corresponds to a standard normal distribution on the effect size). If $\sigma^2 = 1$, this reduces to Rouder’s example; but we now allow for arbitrary σ^2 . We call σ^2 a *nuisance parameter*: a parameter that occurs in both models, is not directly of interest, but that needs to be accounted for in the analysis. The standard prior to use for this nuisance parameter is Jeffreys’ prior for the variance: $\mathbb{P}_J(\sigma) := 1/\sigma$ (Rouder et al., 2009). To obtain the Bayes factor for this problem, we integrate out the parameter σ cf.

Eq. (3). Again, we assign prior odds of 1-to-1, and obtain the posterior odds:

$$\begin{aligned} \text{post-odds}|D &= \frac{1}{1} \frac{\int_0^\infty \frac{1}{\sigma} \prod_{i=1}^n \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(-\frac{x_i^2}{2\sigma^2}\right) d\sigma}{\int_0^\infty \frac{1}{\sigma} \int_{-\infty}^\infty \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(-\frac{\mu}{2\sigma^2}\right) \prod_{i=1}^n \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(-\frac{(x_i-\mu)^2}{2\sigma^2}\right) d\mu d\sigma} \\ &= \frac{1}{\sqrt{n+1}} \left(1 - \frac{\left(\frac{1}{n+1} \sum_{i=1}^n x_i\right)^2}{\frac{1}{n+1} \sum_{i=1}^n x_i^2}\right)^{-\frac{n}{2}} \end{aligned}$$

To repeat Rouder’s experiment, we have to simulate data under both \mathcal{H}_0 and \mathcal{H}_1 . To do this we need to specify the variance σ^2 of the normal distribution(s) from which we sample. We seem to run into a problem: we cannot sample from the prior ‘distribution’ $\mathbb{P}_J(\sigma) = 1/\sigma$. The reason is that it does not integrate to a finite value, since clearly

$$\int_0^\infty \mathbb{P}_J(\sigma) d\sigma = \int_0^\infty \frac{1}{\sigma} d\sigma = \infty, \quad (5)$$

so this is not really a probability distribution — for that it would have to integrate to 1 (formally, Jeffreys’ prior on σ is a ‘measure’ rather than a distribution). Priors that integrate to infinity are called *improper*. Use of such priors for nuisance parameters is not really a problem for Bayesian inference, since one can typically plug such priors into Bayes’ theorem anyway, and this leads to proper posteriors, i.e. posteriors that do integrate to one, and then the Bayesian machinery can go ahead. However, it does make Rouder’s original simulation undefined: recall that, if \mathcal{H}_j , for $j = 1, 2$, is a composite model, then Rouder samples data by first drawing the parameters θ from the prior and then the data from θ . But we cannot directly sample σ from an improper prior. As an alternative, we could sample from an almost improper prior, i.e. with high spread, and see what happens. Fortunately, it turns out that Rouder’s conclusions remain valid if we do this. In fact, they even remain valid if we pick any particular fixed σ^2 to sample from, as we now illustrate. Let us first try $\sigma^2 = 1$. Like Rouder’s example, we sample the mean of the alternative hypothesis \mathcal{H}_1 from the aforementioned normal distribution. Then, we sample 10 data points from a normal distribution with the just sampled mean and the variance that we picked. For the null hypothesis \mathcal{H}_0 we sample the data from a normal distribution with mean zero and the same variance. We continue the experiment just as Rouder did: we calculate the posterior odds from 20,000 replicate experiments of 10 samples for each hypothesis, and construct the histograms and the plot of the ratio of the counts to see if calibration is violated. In Figure 2a we see the calibration plot for the experiment described above. In Figure 2b we see the results for the same experiment, except that we performed optional stopping: we sampled until the posterior odds were at least 10-to-1 for \mathcal{H}_1 , or the maximum of 25 samples was reached. We see that the posterior odds in this experiment with optional stopping are calibrated as well.

Prior Calibration vs. Strong Calibration Importantly, the same conclusion remains valid whether we sample data using $\sigma^2 = 1$, or $\sigma^2 = 2$, or any other value — we invariably end up with the same graphs (we tried many!): even though calculation of the posterior odds given a sample makes use of the prior $\mathbb{P}_J(\sigma) = 1/\sigma$, calibration is retained under sampling under arbitrary σ . We may say that the posterior odds are *prior-calibrated* for the parameter of interest μ given any σ (note that the prior on μ depends on σ^2), and *strongly calibrated* for the parameter σ^2 : we draw the μ parameter from the prior given σ^2 , and for σ^2 we can take any value we like. Notably, strong calibration is a special property of the chosen prior; if we had chosen another proper or improper

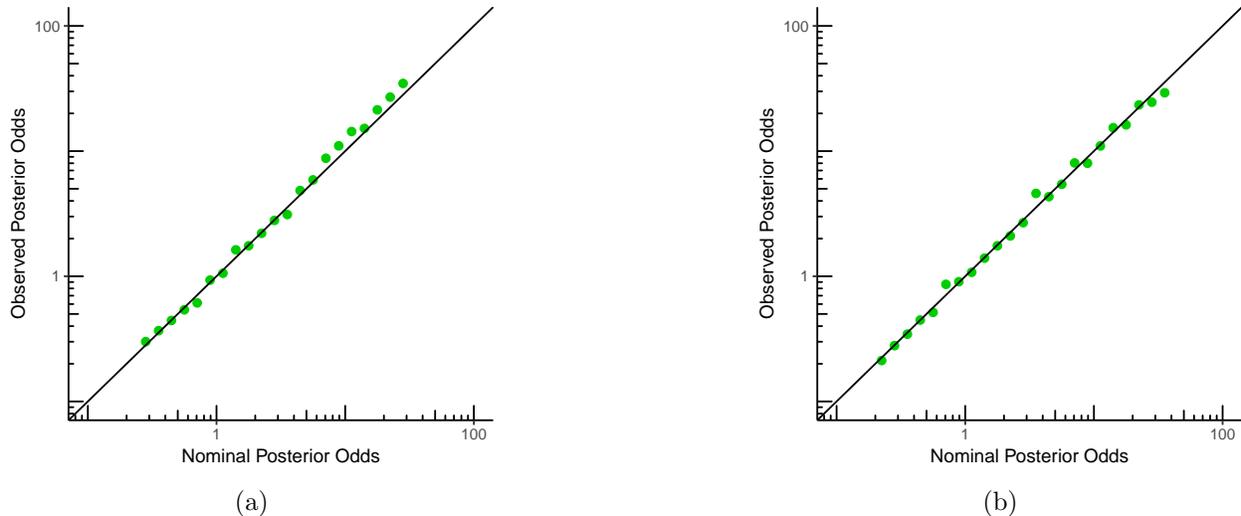


Figure 2: Calibration of the experiment of Section 3.2, from 20,000 replicate experiments. (a) The observed posterior odds as a function of the nominal posterior odds. (b) The observed posterior odds as a function of the nominal posterior odds with optional stopping.

prior to calculate the posterior odds then the property that calibration under optional stopping is retained under any choice of σ^2 will cease to hold; we will see examples below. The reason that $\mathbb{P}_J(\sigma) \propto 1/\sigma$ has this nice property is that σ is a special type of nuisance parameter for which there exist a suitable group structure, relative to which both models are invariant (Eaton, 1989; Berger et al., 1998; Dass and Berger, 2003). We thus can re-express our model in terms of group invariance, in which our model is invariant under the action of the group. In our example, the group action is multiplication; informally, this means that if we divide all outcomes by any fixed σ (multiply by $1/\sigma$), then the Bayes factor remains unchanged.

If such group structure parameters are equipped with a special prior (which, for reasons to become clear, we shall term *Type 0 prior*), then we obtain strong calibration, both for fixed sample sizes and under optional stopping, relative to these parameters.³ Jeffreys’ prior for the variance $\mathbb{P}_J(\sigma)$ is the Type 0 prior for the variance nuisance parameter. Dass and Berger (2003) show that such priors can be defined for a large class of nuisance parameters — we will see the example of a prior on a common mean rather than a variance in Example 3 below; but there also exist cases with parameters that (at least intuitively) are nuisance parameters, for which Type 0 priors do not exist; we give an example in the appendix. For parameters of interest, including e.g. any parameter that does not occur in both models, Type 0 priors never exist.

4 When Problems arise: Subjective vs. Pragmatic and Default Priors

Bayesians view probabilities as degree of belief. The degree of belief an agent has before conducting the experiment, is expressed as a probability function. This *prior* is then updated with data

³Technically, the Type 0 prior for a given group structure is defined as the right-Haar prior for the group: a unique (up to a constant) probability measure induced on the parameter space by the right Haar measure on the related group. Strong calibration is proven in general by Hendriksen et al. (2018), and Hendriksen (2017) for the special case of the 1-sample *t*-test.

from experiments, and the resulting *posterior* can be used to base decisions on. For one pole of the spectrum of Bayesians, the pure *subjectivists*, this is the full story (De Finetti, 1937; Savage, 1972): any prior capturing the belief of the agent is allowed, but it should always be interpreted as the agent’s personal degree of belief; in Section 5 we explain what this means. On the other end of the spectrum, the *objective Bayesians* (Jeffreys, 1961; Berger, 2006) argue that degrees of belief should be restricted, ideally in such a way that they do not depend on the agent, and in the extreme case boil down to a single, rational, probability function, where a priori distributions represent indifference rather than subjective belief and a posteriori distributions represent ‘rational degrees of confirmation’ rather than subjective belief. Ideally, in any given situation there should then just be a single appropriate prior. Most objective Bayesians do not take such an extreme stance, recommending instead *default* priors to be used whenever only very little a priori knowledge is available. These make a *default* choice for the functional form of a distribution (e.g. Cauchy) but often have one or two parameters that can be specified in a subjective way. These may then be replaced by more informative priors when more knowledge becomes available after all. We will see several examples of such default priors below. Recent papers that advocate the use of Bayesian methods within psychology such as Rouder et al. (2009, 2012); Jamil et al. (2016) are mostly based on default priors. Within the statistics community, nowadays a pragmatic stance is by far the most common, in which priors are used that mix ‘default’ and ‘subjective’ aspects (Gelman, 2017) and that are also chosen to allow for computationally feasible inference. Very broadly speaking, we may say that there is a scale ranging from completely ‘objective’ (and hardly used) via ‘default’ (with a few, say 1 or 2 parameters to be filled in subjectively, i.e. based on prior knowledge) and ‘pragmatic’ (with functional forms of the prior based partly on prior knowledge, partly by defaults, and partly by convenience) to the fully subjective. Within the pragmatic stance, one explicitly acknowledges that one’s prior distribution may have some arbitrary aspects to it (e.g. chosen to make computations easier rather than reflecting true prior knowledge). It then becomes important to do sensitivity analyses: studying what happens if a modified prior is used or if data are sampled not by first sampling parameters θ from the prior and then data from $\mathbb{P}(\cdot | \theta)$ but rather directly from a fixed θ within a region that does not have overly small prior probability.⁴

The point of this article is that Rouder’s experiments are tailored towards a fully subjective interpretation of Bayes; as soon as one allows default and pragmatic priors, problems with optional stopping do occur (except for what we call Type 0 priors). We can distinguish between three types of problems, depending on the type of prior that is used. We now give an overview of type of prior and problem, giving concrete examples later.

1. *Type I Priors*: these are proper (default or pragmatic) priors that do *not* depend on any aspects of the experimental setup (such as the sample size) or the data (such as the values of covariates). Here the problem is that posterior calibration under optional stopping breaks down under a sensitivity analysis — an example is the Cauchy prior in Example 2 of Section 4.1 below.
2. *Type II Priors*: these are proper (default and pragmatic) priors that are not of Type I. Such priors are quite common in the Bayesian literature. Here the problem is more serious: as we shall see, Rouder’s experiments cannot be performed for such priors, and ‘handling optional stopping’ is in a sense impossible in principle. An example is the g -prior for regression as in Example 3 below or Jeffreys’ prior for the fixed-sample-size Bernoulli model as in Section 4.3 below.

⁴To witness, one of us recently spoke at the bi-annual OBAYES (Objective Bayes) conference, and noticed that a substantial fraction of the talks featured such fixed θ -analyses and/or used priors of Type II below.

3. *Type III Priors*: these are priors whose form depends on the stopping rule, priors that may be proper for some stopping rules, and improper for others, and improper priors that are not Type 0 priors in the sense above (an example is Jeffreys’ prior for Bernoulli which becomes improper when stopping after a fixed number of 1s as in Section 4.3 below).

We illustrate the problems with Type I and Type II priors by further extending Rouder’s experiment to two extensions of our earlier setting, namely the Bayesian t -test, going back to [Jeffreys \(1961\)](#) and advocated by [Rouder et al. \(2009\)](#), and objective Bayesian linear regression, following [Liang et al. \(2008\)](#). Both methods are quite popular and use default Bayes factors based on default priors, to be used when no clear or very little prior knowledge is readily available.

4.1 Example 2: Bayesian t -test — The Problem with Type I Priors

Suppose a researcher wants to test the effect of a new fertilizer on the growth of some wheat variety. The null hypothesis \mathcal{H}_0 states that there is no difference between the old and the new fertilizer, and the alternative hypothesis \mathcal{H}_1 states that the fertilizers have a different effect on the growth of the wheat. We assume that the length of the wheat is normally distributed with the same (unknown) variance under both fertilizers, and that with the old fertilizer, the mean is known to be $\mu_0 = 1$ meter. We now take a number of seeds and apply the new fertilizer to each of them. We let the wheat grow for a couple of weeks, and we measure the lengths. The null hypothesis \mathcal{H}_0 is thus: $\mu = \mu_0 = 1$, and the alternative hypothesis \mathcal{H}_1 is that the mean of the group with the new fertilizer is different from 1 meter: $\mu \neq 1$.

Again we follow Rouder’s experiment: we generate data from both models and update our prior beliefs from this data. We do this using the *Bayesian t -test*, where Jeffreys’ prior $\mathbb{P}_J(\sigma) = 1/\sigma$ is placed on the standard deviation σ within both hypotheses \mathcal{H}_0 and \mathcal{H}_1 . Within \mathcal{H}_0 we set the mean to $\mu_0 = 1$ and within \mathcal{H}_1 , a standard Cauchy prior is placed on the effect size $(\mu - \mu_0)/\sigma$; details are provided by [Rouder et al. \(2009\)](#). Once again, the nuisance parameter σ is equipped with an improper Jeffreys’ prior, so, like in Experiment 1 above and for the reasons detailed there, for simulating our data, we will choose a fixed value for σ ; the experiments will give the same result regardless of the value we choose.

We generate 10 observations for each fertilizer under both models: for \mathcal{H}_0 we sample data from a normal distribution with mean $\mu_0 = 1$ meter and we pick the variance $\sigma^2 = 1$. For \mathcal{H}_1 we sample data from a normal distribution where the variance is 1 as well, and the mean is determined by the effect size above which is sampled from a standard Cauchy distribution. We follow Rouder’s experiment further, and set our prior odds on \mathcal{H}_0 and \mathcal{H}_1 , before observing the data, to 1-to-1. We sample 20,000 replicate experiments with 10 + 10 samples each, from one of the hypotheses, and we calculate the Bayes factors. Then, we bin the Bayes factors and construct a histogram. In [Figure 3a](#) we see the distribution of the posterior odds when either the null or the alternative are true in one figure. In [Figure 3b](#) we see the calibration plot for this data from which Rouder checks the interpretation of the posterior odds: the observed posterior odds is the ratio of the two histograms, where the width of the bins is 0.1 on the log scale. The posterior odds are calibrated, in accordance with Rouder’s experiments. We repeated the experiment with the difference that in each of the 40,000 experiments we sampled more data points until the posterior odds were at least 10-to-1, or the maximum number of 25 samples was reached. The histograms for this experiment are in [Figure 3c](#). In [Figure 3d](#) we can see that, as expected, the posterior odds are calibrated under optional stopping as well.

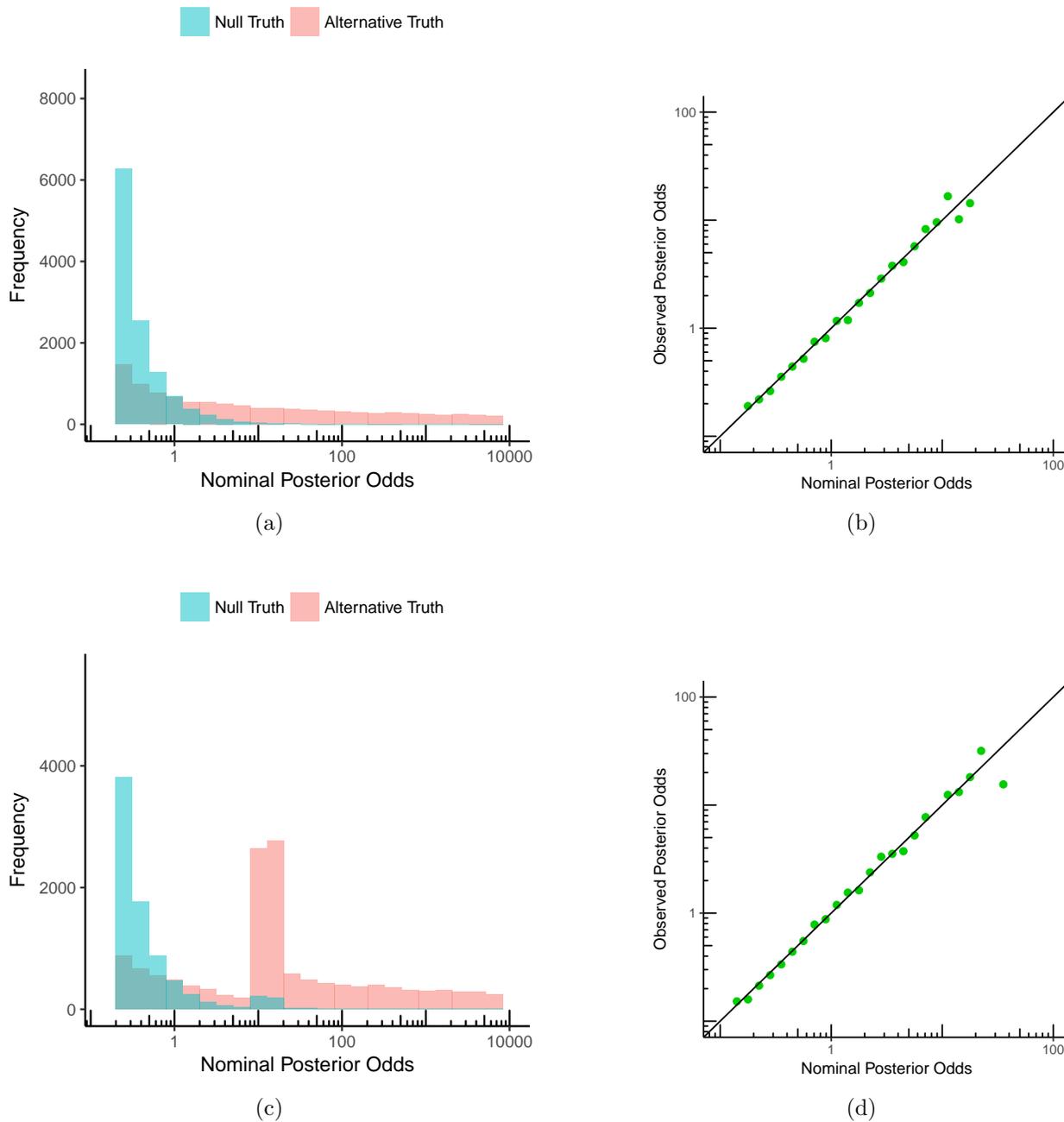


Figure 3: Calibration in the t -test experiment, Section 4.1, from 20,000 replicate experiments. (a) The distribution of posterior odds as a histogram under \mathcal{H}_0 and \mathcal{H}_1 in one figure. (b) The observed posterior odds as a function of the nominal posterior odds. (c) Distribution of the posterior odds with optional stopping. (d) The observed posterior odds as a function of the nominal posterior odds with optional stopping.

Since σ^2 is a nuisance parameter equipped with its Type 0 prior, it does not matter what value we take when sampling data. We may ask ourselves what happens if, similarly, we fix particular values of the mean within a region of nonnegligible prior density and sample from them, rather than from the prior; for sampling from \mathcal{H}_0 , this does not change anything since the prior is concentrated on the single point $\mu_0 = 1$; in \mathcal{H}_1 , this means we can basically pick any μ that is not too far from μ_0 and sample from it. In other words, we will check whether we have strong calibration rather than prior-calibration not just for σ^2 , but also for the mean μ . We now first describe such an experiment, and will explain its importance further below.

We generate 10 observations under both models. The mean length of the wheat is again set to be 1 meter with the old fertilizer, and now we pick a particular value for the mean length of the wheat with the new fertilizer: 130 centimeters. For the variance, we again pick $\sigma^2 = 1$. We continue to follow Rouder’s experiment and set our prior odds on \mathcal{H}_0 and \mathcal{H}_1 , before observing the data, to 1-to-1. We sample 20,000 replicate experiments with 10 + 10 experiments each, 10 from one of the hypotheses (normal with mean 1 for \mathcal{H}_0) and 10 from the other (normal with mean $\mu = 1.3$ for \mathcal{H}_1), and we calculate the Bayes factors. In Figure 4a we see that calibration is, to some extent, violated: the points follow a line that is still approximately, but now not precisely, a straight line. Now what happens in this experiment under optional stopping? We repeated the experiment with the difference that we sampled more data points until the posterior odds were at least 10-to-1, or the maximum number of 25 samples was reached. In Figure 4b we see the results: calibration is now violated significantly — when we stop early the nominal posterior odds (on which our stopping rule was based) are on average significantly higher than the actual, observed posterior odds. We repeated the experiment with various choices of μ ’s within \mathcal{H}_1 , invariably getting similar results.⁵ We conclude that strong calibration for the parameter of interest μ is violated somewhat for fixed sample sizes, but much more strongly under optional stopping. We did similar experiments for a different model with discrete data (see the appendix), once again getting the same result. We also did experiments in which the means of \mathcal{H}_1 were sampled from a different prior than the Cauchy: this also yielded plots which showed violation of (prior-)calibration. Our experiments are all based on a one-sample t -test; experiments with a two-sample t -test and ANOVA (also with the same overall mean for both \mathcal{H}_0 and \mathcal{H}_1) yielded severe violation of strong calibration under optional stopping as well.

The Issue Why is this important? When checking Rouder’s prior-based calibration, we sampled the effect size from a Cauchy distribution, and then we sampled data from the realized effect size. But do we really believe that in this way, we get typical, realistic data? The Cauchy prior was advocated by Jeffreys for the effect size corresponding to a location parameter μ because it has some desirable properties in hypothesis testing, i.e. when comparing two models (Ly et al., 2016). For estimating a one-dimensional location parameter directly, Jeffreys (like most objective Bayesians) would advocate an improper uniform prior on μ . Thus, objective Bayesians may *change their prior depending on the inference task of interest*, even when they are dealing with data representing the same underlying phenomenon. It does then not seem very realistic to study what happens if data are sampled from the prior; *the prior is used as a tool in inferring likely parameters or hypotheses, and not to be thought of as something that prescribes how actual data will tend to look like*. This is the first reason why it is interesting to study not just prior calibration, but also strong calibration

⁵Invariably, strong calibration is violated both with and without optional stopping. In the experiments without optional stopping, the points still lie on an increasing and (approximately) straight line; the extent to which strong calibration is violated — the slope of the straight line — depends on the effect size. In the experiments with optional stopping, strong calibration is violated more strongly in the sense that the points do not follow a straight line anymore.

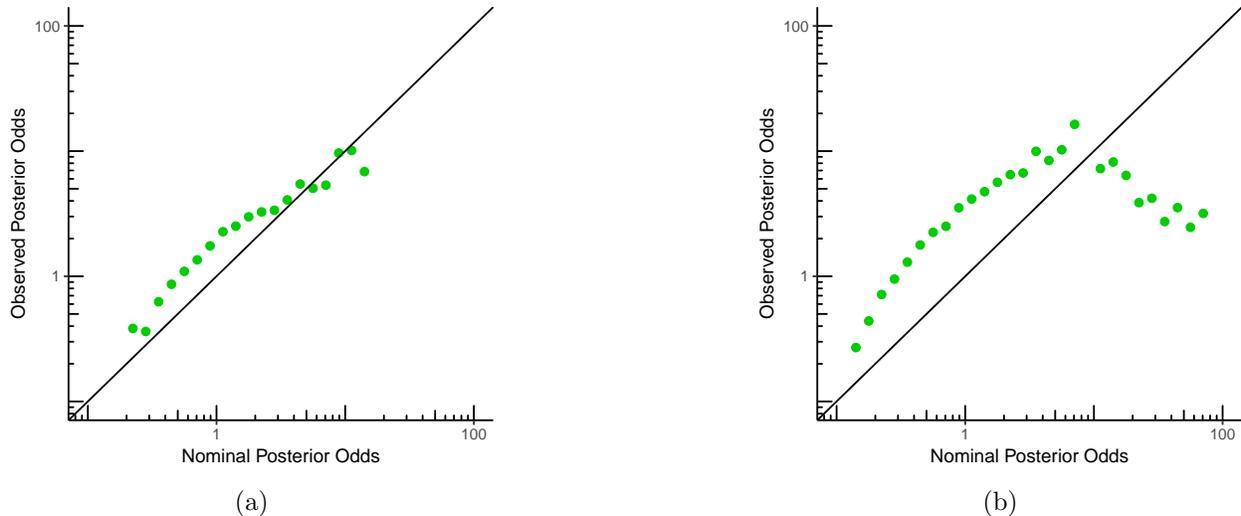


Figure 4: Calibration in the t -test experiment with fixed values for the means of \mathcal{H}_0 and \mathcal{H}_1 (Section 4.1, from 40,000 replicate experiments). (a) The observed posterior odds as a function of the nominal posterior odds. (b) The observed posterior odds as a function of the nominal posterior odds with optional stopping.

for the parameter of interest.

Now, it would make sense to sample from a Cauchy prior if it really described our prior beliefs about the data in the subjective Bayesian sense⁶. But in this particular setup, the Cauchy distribution is highly unrealistic: it is a heavy tailed distribution, which means that the probability of getting very large values is not negligible, and it is very much higher than with, say, a Gaussian distribution. To make the intuition behind this concrete, say that we are interested in measuring the height of a type of corn that with the old fertilizer reaches on average 2 meters. The probability that a new fertilizer would have a mean effect of 6 meters or more under a standard Cauchy distribution would be somewhat larger than one in twenty. For comparison: under a standard Gaussian, this is as small as $9.87 \cdot 10^{-10}$. Do we really believe that it is quite probable (more than one in twenty) that the fertilizer will enable the corn to grow to 8 meters *on average*? Of course we could use a Cauchy with a different spread, but which one? Default Bayesians have emphasized that such choices should be made subjectively (i.e. based on informed prior guesses), but whatever value one chooses, the chosen functional form of the prior (a Cauchy has, e.g., no variance) severely restricts the options, making any actual choice to some extent arbitrary. While growing crops (although a standard example in this context) may be particularly ill-suited to be modeled by heavy-tailed distributions, the same issue will arise with many other possible applications for the default Bayesian t -test: one will be practically sure that the effect size will not exceed certain values (not too large, not too small, certainly not negative), but it may be very hard to specify exactly which values. As a purely objective Bayesian, this need not be such a big problem - one resorts to the default prior and uses it anyway; but one has to be aware that in that case, sampling from the prior — as done by Rouder — is not meaningful anymore, since the data one may get may be quite atypical for the underlying process one is modeling.

⁶A subjective Bayesian would not think of this in terms of sampling from a prior distribution, but as taking an expectation under the prior — see Hendriksen et al. (2018) for details. Mathematically this amounts to the same thing, and it allow us to avoid unnecessary technical language.

In practice, most Bayesians are pragmatic, striking a balance between ‘flat’, ‘uninformative’ priors, prior knowledge and ease of computation. In the present example, they might put a Gaussian prior with mean μ on the effect size instead, truncated at 0 to avoid negative means. But then there is the question what variance this Gaussian should have — as a pragmatic Bayesian, one has to acknowledge that there will always be arbitrary or ‘convenience’ aspects about one’s priors. This is the second reason why it is interesting to study not just prior calibration, but also strong calibration for the parameter of interest.

Thus, both from a purely objective and from a pragmatic Bayesian point of view, strong calibration is important. Except for nuisance parameters with Type 0 priors, we cannot expect it to hold precisely (see [Gu et al. \(2016\)](#) for a related point) — but this is fine; like with any sensitivity or robustness test, we acknowledge that our prior is imperfect and we merely ask that our procedure remains reasonable, not perfect. And we see that by and large this is the case if we use a fixed sample size, but not if we perform optional stopping. In our view this indicates that for pragmatic Bayesians using default priors, there is a real problem with optional stopping after all. However, within the taxonomy defined above, we implicitly used Type I priors (Cauchy) here. Default priors are often of Type II and III, and then, as we will see, the problems get significantly worse.

As a final note, we note that in our strong calibration experiment, we chose parameter values here which we deemed ‘reasonable’, by this we mean values which reside in a region of large prior density. We repeated the experiment for many other such values and always obtained similar results. Whether our choices are truly reasonable is of course up to debate, but we feel that the burden of proof that our values are ‘unreasonable’ lies with those who want to show that Bayesian methods can deal with optional stopping even with default priors.

4.2 Example 3: Bayesian linear regression and Type II Priors

We further extend the previous example to a setting of linear regression with fixed design. We employ the default Bayes factor for regression from the R package `Bayesfactor` ([Morey and Rouder, 2015](#)), based on [Liang et al. \(2008\)](#) and [Zellner and Siow \(1980\)](#), see also [Rouder and Morey \(2012\)](#). This function uses as default prior Jeffreys’ prior for the intercept μ and the variance ($\mathbb{P}_J(\mu, \sigma) \sim 1/\sigma$), and a mixture of a normal and an inverse-gamma distribution for the regression coefficients, henceforth *g-prior*:

$$\begin{aligned} y &\sim N(\mu + X\beta, \sigma^2), \\ \beta &\sim N(0, g\sigma^2n(X'X)^{-1}), \\ g &\sim \text{IG}\left(\frac{1}{2}, \frac{\sqrt{2}}{8}\right). \end{aligned} \tag{6}$$

Since the publication of [Liang et al. \(2008\)](#), this prior has become very popular as a default prior in Bayesian linear regression. Again we provide an example concerning the growth of wheat. Suppose a researcher wants to investigate the relationship between the level of a fertilizer, and the growth of the crop. We can model this experiment by linear regression with fixed design. We add different levels of the fertilizer to pots with seeds: the first pot gets a dose of 0.1, the second 0.2, and so on up to the level 2. These are the x -values (covariates) of our simulation experiment. If we would like to repeat the examples of the previous sections and construct the calibration plots, we can generate the y -values — the increase or decrease in length of the wheat from the intercept μ —

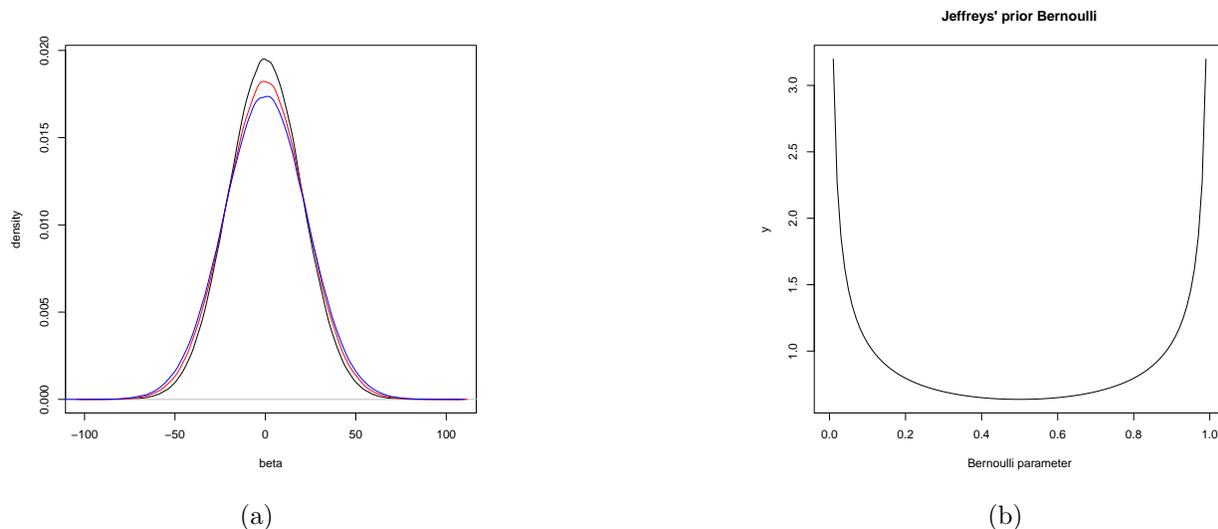


Figure 5: Default priors that depend on aspects of the experimental setup: (a) G-priors for the regression example of Section 4.2 with different sample sizes: $n = 20$ (black), $n = 23$ (red) and $n = 34$ (blue). (b) Jeffreys’ prior for the Bernoulli model for the specific case that n is fixed in advance (no optional stopping): a Beta $(1/2, 1/2)$ distribution.

according to the proposed priors in Eq. (6). First we draw a g from an inverse gamma distribution, then we draw a β from the normal prior that we construct with the knowledge of the x -values, and we compute each y_i as the product of β and x_i plus Gaussian noise.

As we can see in Equation 6, the prior on β contains a scaling factor that depends on a part of the data — it depends on the x -values, but not on the y -values. If there is no optional stopping, then for a pragmatic Bayesian, the dependency on the x -values of the data is convenient to achieve appropriate scaling; it poses no real problems, since the whole model is conditional on X : the levels of fertilizer we administered to the plants. But under optional stopping, the dependency on X does become problematic, *for it is unclear which prior she should use!* If initially a design with 40 pots was planned (after each dose from 0.1 up to 2, another row of pots, one for each dose is added), but after adding three pots to the original twenty (so now we have two pots with the doses 0.1, 0.2 and 0.3, and one with each other dose), the researcher decides to check whether the results already are interesting enough to stop, should she base her decision on the posterior reached with prior based the initially planned design with 40 pots, or the design at the moment of optional stopping with 23 pots? This is not clear, and it does make a difference, since the g-prior changes as more x -values become available. In Figure 5a we see three g-priors on the regression coefficient β for the same fixed value of g , the same x -values as described in the fertilizer experiment above, but increasing sample size. First, each dose is administered to one plant, yielding the black prior distribution for β . Next, 3 plants are added to the experiment, with doses 0.1, 0.2 and 0.3, yielding the red distribution: wider and less peaked, and lastly, another 11 plants are added to the experiment, yielding the blue distribution which puts even less prior mass close to zero.

This problem may perhaps be pragmatically ‘solved’ in practice in two ways: either one could, as a rule, base the decision to stop at sample size n always using the prior for the given design at sample size n ; or one could, as a rule, always use the design for the maximum sample size available.

It is very unclear though whether there is any sense in which any of these two (or other) solutions ‘handle optional stopping’ convincingly. In the first case, Rouder’s prior-based calibration experiment is undefined (one does not know what prior to sample from until after one has stopped); in the second, one can perform it (by sampling β from the prior based on the design at the maximum sample size), but it seems rather meaningless, for if, for some reason or other, even more data were to become available later on, this would imply that the earlier sampled data were somehow ‘wrong’ and would have to be replaced. The g -prior, while quite convenient, performing well in practice and satisfying several desiderata as outlined by Liang et al. (2008), can simply not be interpreted as a prior expressing beliefs about ‘how the data will look like’.

What, then, about strong calibration? Sampling from particular, ‘reasonable’ values of β does seem meaningful in this regression example. However (figures omitted), when we pick reasonable values for β instead of sampling β from the prior, we obtain again the conclusion that strong calibration is, on one hand, violated significantly under optional stopping (where the prior used in the decision to stop can be defined in either of the two ways defined above); but on the other hand, only violated mildly for fixed sample size settings. Using the taxonomy above, we conclude that optional stopping is a significant problem for Bayesians with Type-II priors.

4.3 Bernoulli Parameters and Problems with Type-II and Type-III Priors

Now let us turn to discrete data: we test whether a coin is fair or not. The data D consist of a sequence of n_1 ones and n_0 zeros. Under \mathcal{H}_0 , the data are i.i.d. Bernoulli(1/2); under \mathcal{H}_1 they can be Bernoulli(θ) for any $0 \leq \theta \leq 1$ except 1/2, θ representing the bias of the coin. One standard objective and default Bayes method (in this case coinciding with an *MDL (Minimum Description Length) method*, (Grünwald, 2007)) is to use Jeffreys’ prior for the Bernoulli model within \mathcal{H}_1 . For fixed sample sizes, this prior is proper, and is given by

$$\mathbb{P}_J(\theta) = \frac{1}{\sqrt{\theta(1-\theta)}} \cdot \frac{1}{\pi}, \tag{7}$$

where the factor $1/\pi$ is for normalization; see Figure 5b. If we repeat Rouder’s experiment, and sample from this prior, then the probability that we would pick an extreme θ , within 0.01 of either 1 or 0, would be about 10 times as large as the probability that we would pick a θ within the equally wide interval [0.49, 0.51]. But, lacking real prior knowledge, do we really believe that such extreme values are much more probable than values around the middle? Most people would say we do not (under the subjective interpretation, i.e. if one really believes one’s prior⁷, then such a prior would imply a willingness to gamble at certain stakes — see Section 5. Jeffreys’ prior is chosen in this case because it has desirable properties such as invariance under reparameterization and good frequentist properties, but not because it expresses any ‘real’ prior belief about some parameter values being more likely than others. This is reflected in the fact that in general, it depends on the stopping rule. Using the general definition of Jeffreys’ prior (see e.g. Berger (1985)), we see, for example, that in the Bernoulli model, if the sample size is not fixed in advance but depends on the data (for example, we stop sampling as soon as three consecutive 1s are observed), then, as a simple calculation shows, Jeffreys’ prior becomes improper. While sampling from an improper prior is impossible, we note that ‘prior calibration’ for a fixed stopping rule in the mathematical sense will still hold for improper priors (see Hendriksen et al. (2018) for details); the issue is rather that such a prior behaves qualitatively differently from Type 0, I and II priors, hence we decided

⁷With some abuse of language we mean with *real belief in a prior* that an agent has some true beliefs about the world, that are accurately expressed as a probability function: the prior.

to designate it ‘Type III’; yet, as a referee remarked, the difference between Type II and Type III priors is not that important, and one may think of them as a single type as well.

In the appendix we give another example of a common discrete setting, namely the 2x2 contingency table. Here the null hypothesis is a Bernoulli model and its parameter θ is intuitively a nuisance parameter, and thus strong calibration relative to this parameter would be especially desirable. However, the Bernoulli model does not admit a group structure, and hence neither Jeffreys’ nor any other prior can serve as a Type 0 prior, and strong calibration can presumably not be attained — the experiments show that it is certainly not attained if the default Gunel and Dickey Bayes factors (Jamil et al., 2016) are used (these are Type-II priors, so we need to be careful about what prior to use in the strong calibration experiment; see the appendix for details).

5 Other Conceptualizations of Optional Stopping

We have seen several problems with optional stopping under default and pragmatic priors. Yet it is known from the literature that, in some senses, optional stopping is indeed no problem for Bayesians (Lindley, 1957; Savage, 1972; Edwards et al., 1963; Good, 1991). What then, is shown in those papers? Interestingly, different authors show different things; we consider them in turn.

5.1 Subjective Bayes optional stopping

The Bayesian pioneers Lindley (1957) and Savage (1972) consider a purely subjective Bayesian setting, appropriate if one truly believes one’s prior. But what does this mean? According to De Finetti, one of the two main founding fathers of modern, subjective Bayesian statistics, this implies a willingness to bet at small stakes, at the odds given by the prior.⁸ For example, a subjective Bayesian who would adopt Jeffreys’ prior \mathbb{P}_J for the Bernoulli model as given by (7) would be willing to accept a gamble that pays off when the actual parameter lies close to the boundary, since the corresponding region has substantially higher probability, cf. the discussion underneath Eq. (7). For example, a gamble where one wins 11 cents if the actual Bernoulli parameter is in the set $[0, 0.01] \cup [0.99, 1]$ and pays 100 cents if it is in the set $[0.49, 0.51]$ and neither pays nor gains otherwise would be considered acceptable⁹ because this gamble has positive expected gain under \mathbb{P}_J . We asked several Bayesians who are willing to use Jeffreys’ prior for testing whether they would also be willing to accept such a gamble; most said no, indicating that they do not interpret Jeffreys prior the way a subjective Bayesian would.¹⁰

Now, if one adopts priors one really believes in in the above gambling sense, then it is easy to show that Bayesian updating from prior to posterior is not affected by the employed stopping rule; one ends up with the same posterior if one had decided the sample size n in advance or if it had been

⁸Savage, the other father, employs a slightly different conceptualization in terms of preference orderings over outcomes, but that need not concern us here.

⁹One might object that actual Bernoulli parameters are never revealed and arguably do not exist; but one could replace the gamble by the following essentially equivalent gamble: a possibly biased coin is tossed 10,000 times, but rather than the full data only the average number of 1s will be revealed. If it is in the set $[0, 0.01] \cup [0.99, 1]$ one gains 11 cents and if it is in the set $[0.49, 0.51]$ one pays 100 cents. If one really believes Jeffreys’ prior, this gamble would be considered acceptable.

¹⁰Another example is the Cauchy prior with scale one on the standardized effect size (Rouder et al., 2012), as most would agree that this is not realistic in psychological research. Thanks to an anonymous reviewer for pointing this out.

determined, for example, because one was satisfied with the results at this n . In this sense a subjective Bayesian procedure does not depend on the stopping rule (as we have seen, this is certainly not the case in general for default Bayes procedures). This is the main point concerning optional stopping of Lindley (1957), also made by e.g. Savage (1972); Bernardo and Smith (1994), among many others. A second point made by Lindley (1957, p. 192) is that the decisions a Bayesian makes will “not, *on average*, be in error, when ignoring the stopping rule”. Here the “average” is really an expectation obtained by drawing θ from the prior, and then data from the prior, making this claim very similar to what is shown empirically by Rouder’s (2014) experiments — once again, the claim is correct, but works only if one believes that sampling (or taking averages over) the prior gives rise to data of the type one would really expect; and if one would not be willing to gamble based on the prior in the above sense, it indicates that perhaps one doesn’t really expect that data after all.

We cannot resist to add here that, while for a subjective Bayesian, such prior sampling (and hence prior-based calibration) is sensible, even the founding fathers of subjective Bayes gave a warning against taking such a prior too seriously:¹¹

“ Subjectivists should feel obligated to recognize that any opinion (so much more the initial one) is only vaguely acceptable... So it is important not only to know the exact answer for an exactly specified initial problem, *but what happens changing in a reasonable neighborhood the assumed initial opinion*” De Finetti, as quoted by Dempster (1975).

“ ...in practice the theory of personal probability is supposed to be an idealization of one’s own standard of behaviour; the idealization is often imperfect in such a way that an aura of vagueness is attached to many judgments of personal probability...” (Savage, 1972).

Hence, one would expect that even a subjectivist would be interested in seeing what happens under a sensitivity analysis, for example checking for strong rather than prior-based calibration of the posterior. And even a subjectivist cannot escape the conclusion from our experiments that optional stopping leads to more brittle (more sensitive to the prior choice) inference than stopping at a fixed n .

5.2 Frequentist optional stopping under \mathcal{H}_0

Interestingly, some other well-known Bayesian arguments claiming that ‘optional stopping is no problem for Bayesians’ really show that some Bayesian procedures can deal, in some cases, with optional stopping in a different, frequentist sense. These include Edwards et al. (1963); Good (1991) and many others (the difference between this justification and the above one by Lindley (1957) roughly corresponds to Example 1 vs. Example 2 in the appendix to (Wagenmakers, 2007)). We now explain this frequentist notion of optional stopping, emphasizing that some (but — contrary to what is claimed — by no means all!) tests advocated by Bayesians *do* handle optional stopping in this frequentist sense.

The (or at least, ‘a common’) frequentist interpretation of handling optional stopping is about controlling the Type I error of an experiment. A Type I error occurs when we reject the null hypothesis when it is true, also called a *false positive*. The probability of a Type I error for a certain test is called the *significance level*, usually denoted by α , and in psychology the value of α is usually

¹¹Many thanks to Chris Holmes for bringing these quotations to our attention.

set to 0.05. A typical classical hypothesis test computes a test statistic from the data and uses it to calculate a p-value. It rejects the null hypothesis if the p-value is below the desired Type I error level α . For other types of hypothesis tests, it is also a crucial property to control the Type I error, by which we mean that we can make sure that the probability of making a Type I error remains below our chosen significance level α . The frequentist interpretation of handling optional stopping is that the Type I error guarantee holds if we do not determine the sampling plan — and thus the stopping rule — in advance, but we may stop when we see a significant result. As we know, see e.g. [Wagenmakers \(2007\)](#), maintaining this guarantee under optional stopping is not possible with most classical p-value based hypothesis tests.

We know from probability theory that if a test is a so-called *nonnegative martingale test* under all distributions in \mathcal{H}_0 ([Vovk et al., 2011](#)), then it can handle optional stopping in the frequentist sense — if we reject H_0 iff the posterior odds in favor of \mathcal{H}_0 are smaller than some fixed α , then, even under optional stopping, we are guaranteed a Type I error of at most α . Some (but not all, very few in fact) classical hypothesis tests are such martingale tests, for example Wald’s sequential likelihood ratio test ([Wald, 1949](#)). And some Bayes factors (but not all) are martingale tests as well. To be precise, all Bayes factors of the form (2) where the null hypothesis \mathcal{H}_0 is *simple*, are martingales. Hence, frequentist optional stopping is no problem for Bayesians-with-a-simple-null-hypothesis. This is what was noted by [Edwards et al. \(1963\)](#) (using a different terminology) and [Good \(1991\)](#), based on what [Sanborn and Hills \(2014\)](#) call the *universal bound*, and what in probability theory is known as *Doob’s maximal inequality*. More information on martingale tests can be found in [Doob \(1971\)](#), [Vovk et al. \(2011\)](#) and [Van der Pas and Grünwald \(2018\)](#).

But what happens if \mathcal{H}_0 is composite? Now not only the alternative distribution involves marginalization over an unknown parameter (with help of a prior distribution), but so does the null distribution. For composite null hypotheses, this condition would be met if we have the special case of a Bayes factor that is a nonnegative martingale under all distributions in \mathcal{H}_0 , which is a very stringent requirement. As follows by arguments of [Hendriksen et al. \(2018\)](#), this does happen if *all* free parameters in \mathcal{H}_0 are nuisance parameters observing a group structure and equipped with the corresponding Type 0 prior and are shared with \mathcal{H}_1 , an example being Jeffreys’ Bayesian *t*-test of Section 4.1. Thus, for Type 0-only priors on \mathcal{H}_0 , Bayes factor hypothesis tests can handle optional stopping in the frequentist sense under \mathcal{H}_0 . We conjecture (though have no proof) that these are the only such cases.

An Empirical Frequentist Study of Bayesian Optional Stopping [Schönbrodt et al. \(2017\)](#) performed a thorough simulation study to analyze frequentist performance of optional stopping with Bayes factors both under \mathcal{H}_0 and under \mathcal{H}_1 . They confined their analysis to the Bayesian *t*-test, i.e. our Example 2, and found excellent results for the Bayesian optional stopping procedure *under a certain frequentist interpretation* of the Bayes factors (posterior odds). As to optional stopping under \mathcal{H}_0 (concerning Type-I error), this should not surprise us: in the Bayesian *t*-test, all free parameters in \mathcal{H}_0 are equipped with Type 0 priors, which, as we just stated, can handle optional stopping. We thus feel that one should be careful in extrapolating their results to other models such as those for contingency tables, which do not admit such priors. As to optional stopping under \mathcal{H}_1 , the authors provide a table showing how, for any given effect size δ and desired level of Type-II error β , a threshold B can be determined such that the standard Bayesian *t*-test with (essentially) the following optional stopping and decision rule, has Type-II error β :

Take at least 20 sample points. After that stop as soon as posterior odds are larger than

B or smaller than $1/B$; accept \mathcal{H}_0 if they are smaller than $1/B$, and reject \mathcal{H}_0 if larger than B .

For example, if $\delta \geq 0.3$ and one takes $B = 7$ then the Type-II error will be smaller than 4% (see their Table 1). They also determined the average sample size needed before this procedure stops, and noted that this is considerably smaller than with the standard t -test optimized for the given desired levels of Type-I and Type-II error and a priori expected effect size. Thus, if one determines the optional stopping threshold B in the Bayesian t -test based on their table, one can use this Bayesian procedure as a frequentist testing method that significantly improves on the standard t -test in terms of sample size. Under *this* frequentist interpretation (which relies on the specifics of a table), optional stopping with the t -test is indeed unproblematic. Note that this does not contradict our findings in any way: our simulations show that if, when sampling, we fix an effect size in \mathcal{H}_1 , then the posterior is biased under optional stopping, which means that we cannot interpret the posterior in a *Bayesian* way.

6 Discussion and Conclusion

When a researcher using Bayes factors for hypothesis testing truly believes in her prior, she can deal with optional stopping in the Bayesian senses just explained. However, these senses become problematic for every test that makes use of default priors, including all default Bayes factor tests advocated within the Bayesian Psychology community. Such ‘default’ or ‘objective’ priors cannot be interpreted in terms of willingness to bet, and sometimes (Type II and Type III priors) depend on aspects of the problem at hand such as the stopping rule or the inference task of interest. To make sense of such priors generally, it thus seems necessary to *restrict* their use to their appropriate domain of reference — for example, Jeffreys’ prior for the Bernoulli model as given by (7) is o.k. for Bayes factor hypothesis testing with fixed sample size, but not for more complicated stopping rules. This idea, which is unfortunately almost totally lacking from the modern Bayesian literature, is the basis of a novel theory of the very concept of probability called *Safe Probability* which is being developed by one of us (Grünwald, 2013; Grünwald, 2018).

Rouder (2014) argues in response to Sanborn and Hills (2014) that the latter ‘evaluate and interpret Bayesian statistics as if they were frequentist statistics’, and that ‘the more germane question is whether Bayesian statistics are interpretable as *Bayesian statistics*’. Given the betting interpretation above, the essence here is that we need to make a distinction between the purely subjective and the non-subjective approach: we can certainly not evaluate and interpret *all* Bayesian statistics as *purely subjective* Bayesian statistics, what Rouder (2014) seems to imply. He advises Bayesians to use optional stopping — without any remark or restriction to purely subjective Bayesians, and for a readership of experimental psychologists who are in general not familiar with the different flavors of Bayesianism — as he writes further on: ‘Bayesians should consider optional stopping in practice. [...] Such an approach strikes me as justifiable and reasonable, perhaps with the caveat that such protocols be made explicit before data collection’. The crucial point here is that this can indeed be done when one works with a purely subjective Bayesian method, but not with the *default Bayes factors* developed for practical use in social science. In Table 1 we provide researchers with a simplified overview of four common default Bayes factors indicating which forms of optional stopping they can handle.

While some find the purely subjective Bayesian framework unsuitable for scientific research (see e.g. Berger (2006)), others deem it the only coherent approach to learning from data per se. We do

	Prior Cal.	Strong Calibration	Freq. OS
Default Bayes Factors			
T-test (Rouder et al., 2009)	✓ but... (I)	✓ for σ (0) ✗ for δ (effect size) (I)	✓
ANOVA (Rouder et al., 2012)	✓ but... (I)	✓ for μ, σ (0) ✗ for δ (effect size) (I)	✓
Regression (Rouder and Morey, 2012)	✗ (II)	✓ for μ, σ (0) ✗ for β (effects) (II)	✓
Contingency Tables (Jamil et al., 2016)	✗ (II)	✗	✗
Bayes Factors with proper, fully subjective priors (Rouder, 2014)	✓	N/A	N/A

Table 1: Overview of several common default Bayes Factors (from the R-package `BayesFactor` (Morey and Rouder, 2015)), and their robustness against different kinds of optional stopping. Between parentheses is the type of prior used, in the taxonomy introduced in this paper. The **but..** indicates that, formally, prior calibration works for the priors, yet, because we are in the default setting, the Bayes factor is not fully subjective, so prior calibration is not too meaningful — which is just the main point of this paper. A second caveat is that sampling from the prior on μ, σ is not possible in these situations since the prior is improper; yet the results of Hendriksen et al. (2018) show that prior calibration can still be mathematically defined, and does hold.

not want to enter this discussion, and we do not have to, since in practice, nowadays most Bayesian statisticians tend to use priors which have both ‘default’ and ‘subjective’ aspects. Basically, one uses mathematically convenient priors (which one does not really believe, so they are not purely subjective — and hence, sampling from the prior should be regarded with suspicion), but they are also chosen to be not overly unrealistic or to match, to some extent, prior knowledge one might have about a problem. This position is almost inevitable in Bayesian practice (especially since we would not like to burden practitioners with all the subtleties regarding objective and subjective Bayes), and we have no objections to it — but it does imply that, just like frequentists, Bayesians should be careful with optional stopping.

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A Example 4: An independence test in a 2x2 contingency table

Suppose that a researcher considers two hypotheses: a null hypothesis \mathcal{H}_0 that states that there is no difference in voting preference (Democrat or Republican) between men and women, and an alternative hypothesis \mathcal{H}_1 stating that men’s voting preferences differ from the women’s preferences. Both hypotheses are composite — we may think of a Bernoulli model for \mathcal{H}_0 : the data are i.i.d. with a fixed probability of 1 (voting Democrat). We are however not interested in the percentage of the persons voting for the Democrats. We are, instead, only interested to learn if this percentage is *equal* for men and women or not. Thus our null hypothesis \mathcal{H}_0 consists of all Bernoulli distributions (all possible biases of the coin, infinitely many between 0 and 1) where the model for the men is the same as for the women. Our alternative hypothesis is composite as well: all the sets of two Bernoulli distributions — one for the men and one for the women — that are not equal. Thus, the Bernoulli parameter in \mathcal{H}_0 is not a parameter of interest; instead, at least intuitively, it is a nuisance parameter similar to the variance in Example 1; however, it does not observe a group structure and a Type 0-prior for this parameter does not exist.

Once again we follow Rouder’s experiments closely. We now use the *Default* *Gunel and Dickey Bayes Factors for Contingency Tables* (Jamil et al., 2016), which employs specific default choices for the priors within \mathcal{H}_0 and \mathcal{H}_1 , depending on four different sampling schemes (see Section A for the details). We immediately run into a problem similar to the problems described with the g -prior and Jeffreys’ prior for Bernoulli: which prior we should choose depends on the sampling plan itself. Based on earlier work by Gunel and Dickey (1974) (GD from now on), Jamil et al. (2016) provide different default priors depending on whether the sample size n and/or some of the four counts (number of men/women voting democratic/republican) are fixed in advance. For the case that none of these are fixed in advance, they provide a prior which assumes that the four counts are all Poisson distributed; see the next section for details. Intuitively, none of these priors seem to be compatible with the very idea of ‘optional stopping’ and prior-based calibration under optional stopping cannot be tested (since it is not clear what prior to sample from — a Type II-problem in our earlier terminology). Still, to check the claim that ‘optional stopping is no problem for Bayesians’ we will again check whether *strong* calibration holds with and without optional stopping. We display here the results of an experiment with the prior advocated for the case in which neither n nor any of the counts are assumed to be fixed in advance, since this seems the choice least incompatible with optional stopping. To avoid discussion on this issue though, we also performed the experiments with the priors advocated for other sampling schemes and combinations of different sampling schemes, which led to very similar results.

We will again fix some ‘reasonable’ parameter values in each model: when sampling from \mathcal{H}_0 , we really sample from $\theta = 1/2$, i.e. we suppose that 50% of either gender prefers the Democrats. When we sample from \mathcal{H}_1 , we suppose that 45% of the men prefers the Democrats, but for the women it is as much as 55%. If there are equally many men as women, under both hypotheses the average percentage is equal. Like Rouder, we set our prior odds to 1-to-1.

We simulate 20.000 replicate experiments of 100 + 100 samples each, from both \mathcal{H}_0 and \mathcal{H}_1 , and we calculate the Bayes Factors. We construct the histograms and the plots with the odds as before. We can check the calibration in Figure 6b: we can see that the nominal posterior odds agree roughly with the observed posterior odds. In Figure 6d however, we see the same plot where we did the same experiment with optional stopping. We can clearly see that even the rough linear relationship from Figure 6b is completely gone. For this example, we can conclude as well that

strong calibration is violated.

We now revisit the example, but we change the proportions under both hypotheses and survey only 25 men and 25 women, and we use a joint multinomial sampling scheme (the grand total, n , is fixed). Under \mathcal{H}_0 , 70% of both men and women vote for the Democrats, and under \mathcal{H}_1 , 65% of the men and 75% of the women do. We repeat exactly the same experiment (without optional stopping), and we see the resulting plot in Figure 7a. We see that the relationship between the observed and nominal posterior odds looks linear, but the slope is off. If we repeat the same experiment with optional stopping, we see in Figure 7b that additionally the linear association is missing.

We do note that the objective priors used in the default Bayes Factor test for contingency tables are proper, so we are able to sample from them. In Figure 7c we see what happens if we do exactly the same experiment as in Figure 7a, but sampled from the prior: we see the observed posterior odds plotted against the nominal posterior odds, and the points lie approximately on the identity line, in contrast with Figure 7a. Furthermore, we performed the same experiment as in Figure 7b in this subjective Bayesian way, and we see that (in Rouder’s terminology) the interpretation of the posterior odds holds with optional stopping in Figure 7d. As said, we do not think this kind of sampling is very meaningful in default prior context; we just add the experiment to show that invariably, *if* one can and wants to sample from priors, then Rouder’s conclusions do hold.

Subjective vs. Objective Interpretation In their original paper, [Gunel and Dickey \(1974\)](#) (GD) give a *subjective* interpretation to their priors. These priors depend on the sampling scheme, i.e. on whether the grand total, and/or one or both of the marginals are known or set by the experimenter in advance. At first sight, this seems to be at odds with the fact that, with subjective priors, Bayesian procedures do not depend on the stopping rule used, as we pointed out in Section 5. However, closer inspection reveals that if one follows the method under their subjective interpretation, then the posterior indeed would not depend on the sampling scheme. How is this possible? To see this, note that GD do not model their data as coming in sequentially, but rather they consider a fixed, single datum $D = (N_1, \dots, N_4)$ consisting of the four entries in the contingency table (see e.g. Table 2 below). The different versions of their model and prior are then arrived at by calculating, for example, $\mathbb{P}(D \mid \mathcal{H}_0)$ for the case that no information about the design is given, and $\mathbb{P}(D \mid \mathcal{H}_0, n)$ (where $n = N_1 + N_2 + N_3 + N_4$) for the case that the grand total (sample size) n is determined in the experiment design. In every case, the posterior odds $\text{post-odds} \mid D$ will remain the same; for they require the *prior* to be used when n is given, $\mathbb{P}(\mathcal{H}_0 \mid n)$, to be arrived at by conditioning the original prior $\mathbb{P}(\mathcal{H}_0)$ on the grand total n . In particular, this means that a truly subjective Bayesian who follows the GD model would have $\mathbb{P}(\mathcal{H}_0 \mid n) \neq \mathbb{P}(\mathcal{H}_0)$, and could thus not use a $(1/2, 1/2)$ ‘uninformative’ prior on $(\mathcal{H}_0, \mathcal{H}_1)$ both when the grand total is known in advance and when it is not. In other words, the posterior is *not* affected by the sampling scheme, but the *prior* is.

Details of the experiments For Example 4 above, we used the function `contingencyTableBF`. This function gives the user the option to choose between four different so called sampling schemes, implementing the *Default Gunel and Dickey Bayes Factors for Contingency Tables* of [Jamil et al. \(2016\)](#). Which of the four options to use, depends on which covariates in the contingency table are to be treated as fixed or as random, depending on the design of the experiment.

In the first sampling scheme, none of the cell counts in the contingency table are considered fixed, and the assumption is made that each cell count is Poisson distributed. The default prior for this scheme is a conjugate gamma prior on the Poisson rate parameter, with hyperparameters suggested by Gunel and Dickey. We use this sampling scheme for our first experiment in Section A,

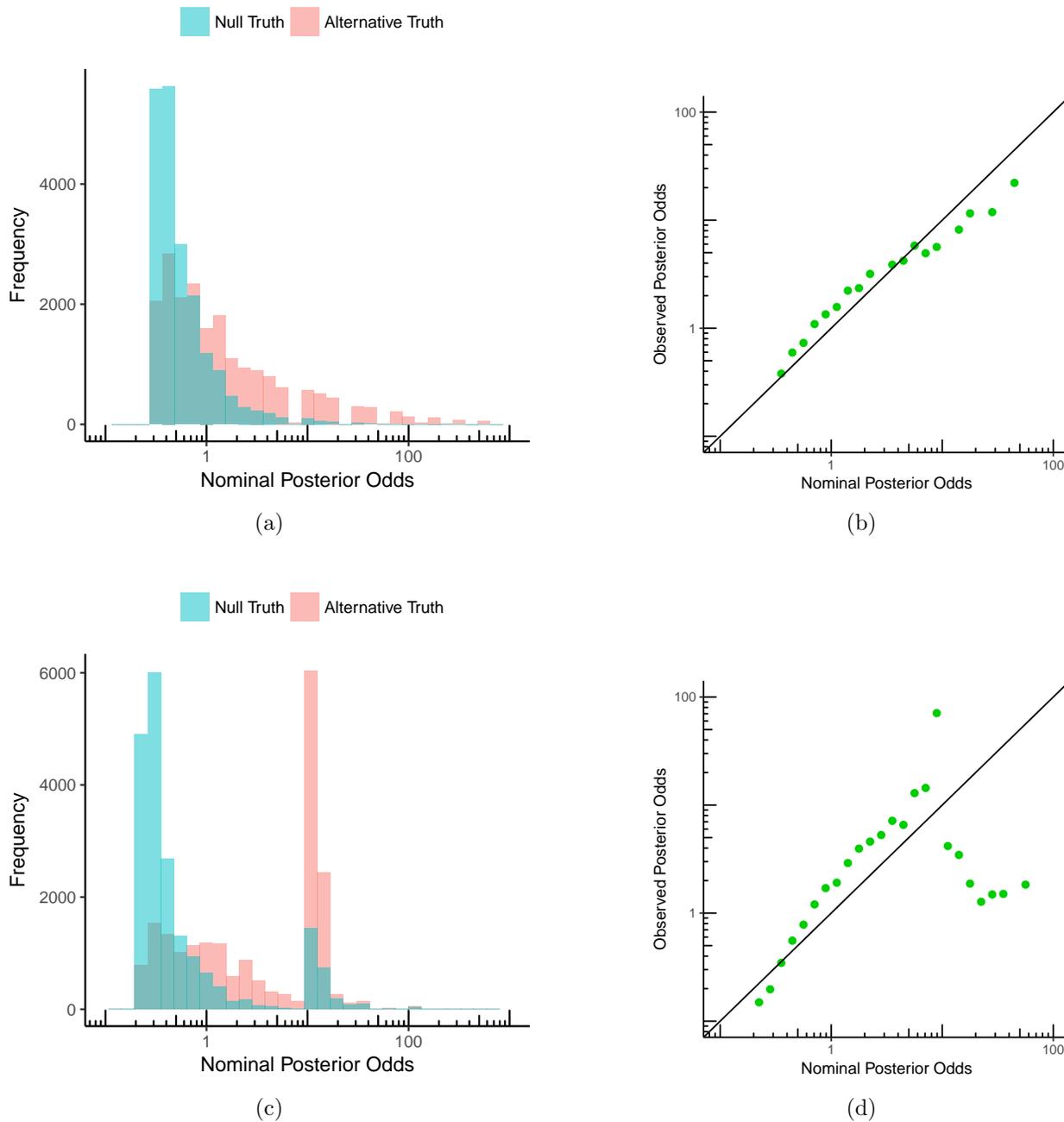
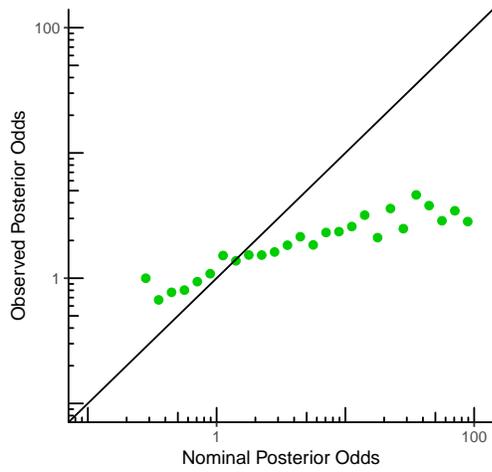
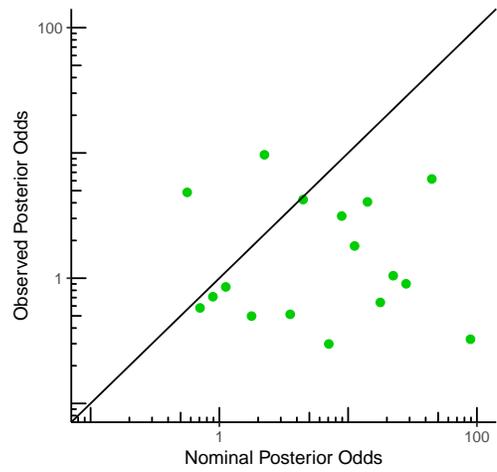


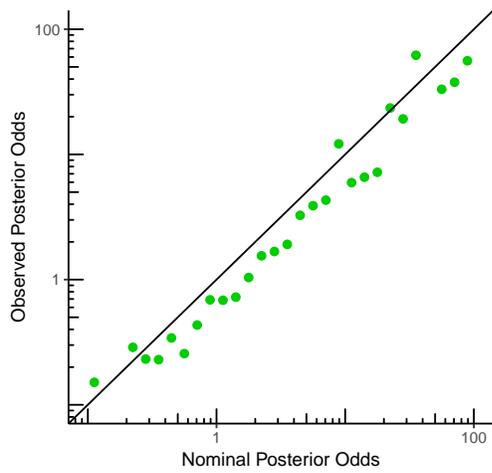
Figure 6: Calibration of the contingency table experiment, Section A, from 20,000 replicate experiments. (a) The distribution of posterior odds as a histogram under \mathcal{H}_0 and \mathcal{H}_1 . (b) The observed posterior odds as a function of the nominal posterior odds. (c) Distribution of the posterior odds with optional stopping. (d) The observed posterior odds as a function of the nominal posterior odds with optional stopping.



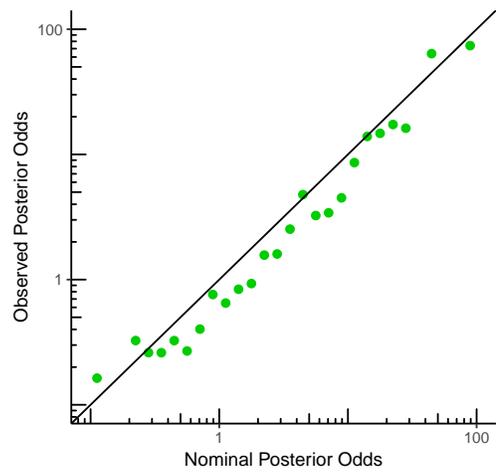
(a)



(b)



(c)



(d)

Figure 7: The observed posterior odds as a function of the nominal posterior odds, from 20,000 replicate experiments. (a) Contingency table experiment, without optional stopping. (b) Contingency table experiment, with optional stopping. (c) Subjective Bayesian version of the experiment in a. (d) Subjective Bayesian version of the experiment in b.

	0	1	sum
0	$n_1 - k_1$	$n_2 - k_2$	$n - k$
1	k_1	k_2	k
sum	n_1	n_2	n

Table 2: 2x2 contingency table; the four entries correspond to the numbers N_1, N_2, \dots, N_4 above.

but as we noted in our discussion in the same section, the question of ‘what is the actual sampling scheme’ and hence ‘what is the right default prior’ for the type of experiment we do — the same experiment with and without optional stopping — is really impossible to answer. Thus, we repeated the experiment with other (combinations of) sampling schemes, in all cases obtaining similar results. Indeed, when we perform the experiment without optional stopping, we sample a fixed number of men and women, whereupon one margin (n_1, n_2) and the grand total (n) is fixed. For our second example (Figure 7a and 7b) we used the prior advocated for the sampling scheme in which the grand total $(n$ in Table 2) is fixed. Under this sampling scheme, the cell counts are assumed to be jointly multinomial distributed, and a Dirichlet conjugate distribution with the suggested parameters (Jamil et al., 2016) is used as prior, which in our case amounts to a uniform prior on the Bernoulli parameter θ ; see Jamil et al. (2016) for details. Again, using instead one of the priors advocated for one of the other sampling schemes leads to very similar results.