***Learning Statistics w/ R - University of Adelaide***

***Part VI – Endings, alternatives and prospects***

**Chapter 17 - Bayesian statistics**

* *In our reasoning concerning “matter of fact”, there are all imaginable degrees of assurance, from the highest certainty to the lowest species of moral evidence. A wise man, therefore, proportions his belief to the evidence*. – David Hume
* The ideas presented prior describe **inferential statistics** from the **frequentist perspective**.
* Almost every text given to undergrad psych students presents the opinions of the frequentist statistician as *THE* theory of inferential statistics, the one true way to do things
* Frequentist statistics dominated the academic field of statistics for most of the 20th century, + this dominance is even more extreme among applied scientists.
* It was + is current practice among psychologists to use frequentist methods.
* B/c frequentist methods are ubiquitous in scientific papers, every student of statistics needs to understand those methods, otherwise they will be unable to make sense of what those papers are saying
* Unfortunately, the current practice in psychology is often misguided + the reliance on frequentist methods is partly to blame.

**17.1 Probabilistic reasoning by rational agents**

* From a **Bayesian perspective**, statistical inference is all about **belief revision**.
* Start out w/ a set of **candidate hypotheses** **h** about the world + don’t know which are *true*, but do have some **beliefs** about *which* are *plausible* + which are not.
* When I observe the data **d**, I have to **revise** those beliefs.
* If data are consistent w/ a hypothesis, my belief in hypothesis is strengthened + inconsistent w/ the hypothesis, my belief in that hypothesis is weakened.
* Key Ideas Ex: I’m carrying an umbrella. Do you think it will rain?
* Single piece of data **d = carrying the umbrella**
* Asking about your beliefs about whether it’s raining.
* 2 possible hypotheses, **h** 🡺 either it rains today or it does not.
* **Priors** = *what you believed before*
* 1st thing to do = ignore what I told you about the umbrella, + write down your pre-existing beliefs about rain
* This is important 🡪 **to be honest about how beliefs have been revised in the light of new evidence, you must say something about what you believed before those data appeared**
* You know I live in Australia + that much of Australia is hot + dry (Adelaide has a Mediterranean climate, very similar to southern California, southern Europe or northern Africa).
* Assume it’s the middle of summer + look on Wikipedia to discover Adelaide gets an average of 4.4 days of rain across the 31 days of January.
* W/out knowing anything else, you might conclude probability of January rain in Adelaide is about 15%, + probability of a dry day is 85%.
* If this is really what you believe about Adelaide rainfall, this is your **prior distribution**, written **P(h):**



* **Likelihoods** = *theories about the data*
* To solve the reasoning problem, you need a **theory** about behavior 🡪 When do I carry an umbrella?
* May assume I will try to carry umbrellas only on rainy days, but we also may know I have young kids + wouldn’t be surprised to know that I’m pretty forgetful about this sort of thing.
* Suppose on rainy days I remember my umbrella ~30% of the time + say on dry days I’m only about ~5% likely to be carrying an umbrella.



* It’s important to remember each cell in this table describes **your beliefs about what data d will be observed, given the truth of a particular hypothesis h.**
* This **conditional probability** is written **P(d | h)** = probability of d given h.
* In **Bayesian statistics**, this = **likelihood of data d given hypothesis h**.
* At this point, all elements are in place 🡪 w/ priors + the likelihood, you have all the info needed for Bayesian reasoning.
* How do we use this information? 🡪 there’s a very simple equation we can use
* Let’s start out w/ 1 of the rules of **Probability Theory** that talks about probability 2 things are true
* Might want to calculate probability today is rainy (hypothesis h = true) + I’m carrying an umbrella (data d is observed).
* The **joint probability** of the hypothesis + the data is written **(P(d, h)** + is calculated by multiplying the **prior** P(h) by the **likelihood** P( d| h) 🡪 Mathematically, say that:



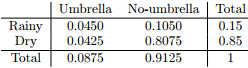
* Probability today = rainy + I remember to carry an umbrella 🡪 The **prior** says P(rain) = 15%, + the **likelihood** says probability of remembering my umbrella *on a rainy day* = 30%.
* Probability both of these things are true = calculated by multiplying the two



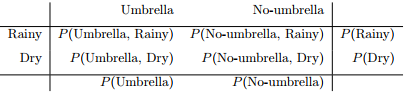
* In other words, before *being told anything* about what *actually happened*, you think there is a 4.5% probability today will be rainy + I will remember an umbrella.
* However, there are 4 four possible things that could happen 🡺 Repeat the exercise for all four.



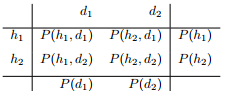
* This captures all the info about which of the 4 possibilities are likely.
* To get the full picture, it helps to add the row totals + column totals.



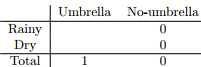
* Some statisticians would object to using the word **likelihood** here
* Problem = the word “likelihood” has a very specific meaning in frequentist statistics that is not quite the same in Bayesian statistics.
* Bayesians didn’t originally have any agreed upon name for “likelihood”, + so it became common practice for people to use frequentist terminology.
* This wouldn’t have been a problem, except the way Bayesians use the word turns out to be quite different to the way frequentists do.
* To put it crudely: when a *Bayesian* says a **likelihood function**, they’re usually referring 1 of the rows of the above table.
* When a *frequentist* says the same thing, they’re referring to the same table, but to them a likelihood function almost always refers to one of the columns.
* This distinction matters in some contexts, but it’s not important for our purposes.
* This is a very useful table, so it’s worth taking a moment to think about what these #’s are telling us.
* 1st, the row sums aren’t telling us anything new at all.
* 1st row = if we ignore all this umbrella business, the chance today will be rainy = 15% = **our prior**
* The important thing isn’t the number itself, but that it gives us some confidence that our calculations are sensible
* The column sums tell us something we haven’t explicitly stated yet.
* In the same way row sums tell us probability of rain, column sums tell us probability of me carrying an umbrella.
* Specifically, the 1st column tells us that, on average (ignoring whether it’s rainy or not), probability of me carrying an umbrella = 8.75%.
* Finally, when we sum across all 4 logically-possible events, everything adds up to 1.
* In other words, what we have written down is a proper **probability distribution** *defined over all possible combinations of data + hypothesis.*



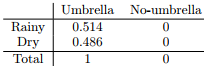
* Finally, use proper statistical notation 🡪 data corresponds to the observation that I do or do not have an umbrella 🡪 d1 = possibility you observe me carrying an umbrella + d2 = you observing me not carrying one.
* Similarly, h1 = your hypothesis that today is rainy + h2 = hypothesis that it is not.



* This table is a very powerful tool for solving the rainy day problem b/c it considers all 4 logical possibilities + states exactly how confident you are in each of them *before being given any data*.
* But, beliefs can change when actually given the data.
* In the rainy day problem, you are told I *really am* carrying an umbrella, which is surprising b/c according to our table, P(u) = 8.75%.
* No matter how unlikely you thought it was, you must now **adjust your beliefs** to accommodate the fact that *you now know that I have an umbrella*.
* If being a bit more sophisticated, could extend the example to accommodate the possibility I’m lying about the umbrella. But let’s keep things simple
* To reflect this new knowledge, our *revised* table must have the following numbers:



* In other words, the **facts** have eliminated *any possibility of no umbrella*, so we have to put 0’s into any cell that implies I’m not carrying an umbrella.
* Also, you know for a FACT I am carrying an umbrella, so the column sum on the left must = 1 to correctly describe the fact that P(u) = 1.
* *What 2 #’s should we put in the empty cells?*
* Don’t worry about the math + instead think about intuitions.
* In the 1st table, it turned out those 2 cells had almost identical numbers
* We worked out the joint probability of rain + umbrella = 4.5%, + the joint probability of dry + umbrella = 4.25%
* In other words, before we knew I am in fact carrying an umbrella, you’d have said these 2 events were *almost identical in probability*
* But notice *both of these possibilities are consistent w/ the fact I actually am carrying an umbrella*.
* *From the perspective of these 2 possibilities, very little has changed*.
* So, it’s still true that these 2 possibilities are equally plausible.
* What we expect to see in our final table is some #’s that preserve the fact that rain + umbrella is slightly more plausible than dry + umbrella, while still ensuring that #’s in the table add up.



* This says that, after being told I’m carrying an umbrella, we believe there’s a 51.4% chance today will be rainy + a 48.6% chance it won’t.
* The **posterior probability** of rain given I am carrying an umbrella **P(h | d)** = 51.4%
* To work out there was a 0.514 probability of rain, take P(rain \* u) = 0.045 + divide it by P(u) = 0.0875
* This produces a table that satisfies our need to have everything sum to 1 + our need not to interfere w/ the relative plausibility of the 2 events that are actually consistent w/ the data.
* To say the same thing using statistical jargon, what I’ve done here is **divide the joint probability of the hypothesis + the data P(d, h) by the marginal probability of the data P(d) to get the posterior probability of the hypothesis given we know the data have been observed.**



* Might notice this equation is actually a restatement of the same basic rule listed at the start of the last section.
* If you multiply both sides of the equation by P(d), you get **P(d)\*P(h | d) = P(d, h)**, which is the rule for how joint probabilities are calculated.
* So I’m not actually introducing any new rules here, it’s the same rule in a different way
* However, remember a joint probability P(d, h) = multiplying the prior P(h) by the likelihood P(d | h)
* This gives us the following formula for the posterior probability:



* This says Probability of hypothesis given observed data = result of the probability of observed data given the hypotheses multiplied by the probability of the hypothesis divided by the probability of the observed data
* This is as **Bayes’ rule =** describes how a learner starts out w/ prior beliefs about the plausibility of different hypotheses + tells you how those beliefs should be revised in the face of data.
* In the Bayesian paradigm, all statistical inference flows from this 1 simple rule.

**17.2 Bayesian hypothesis tests**

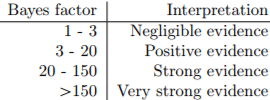
* Null hypothesis testing is a very elaborate contraption people find very hard to make sense of.
* In contrast, the Bayesian approach to hypothesis testing is incredibly simple.
* Pick a setting closely analogous to an orthodox scenario 🡪 2 hypotheses to compare: h0 + h1.
* Prior to running the experiment, we have some **beliefs** **P(h)** about *which hypotheses are true*.
* We run an experiment + obtain data **d**.
* Unlike *frequentist* statistics Bayesian statistics DOES allow one to talk about the probability the null is true.
* Better yet, it allows us to calculate **posterior probability** of the null, using Bayes’ rule
* Similarly, we can work out how much belief to place in the alternative using the same equation.

* This tells us exactly *how much belief we should have in the null after having observed the data d.*
* This is highly simplified 🡪all the complexity of real-life Bayesian hypothesis testing comes down to how you calculate the **likelihood P(d|h)** *when the hypothesis h is a complex + vague thing.*
* In practice, most Bayesian data analysts tend not to talk in terms of the *raw* posterior probabilities P(h0|d) + P(h1|d).
* Instead, they tend to talk in terms of the posterior **odds ratio**.
* Think of it like betting.
* Suppose posterior probability of the null = 25% + posterior probability of the alternative = 75%.
* The alternative is 3X as probable as the null, so we say the odds are 3:1 in favor of the alternative.
* To calculate **posterior odds** = divide 1 posterior probability by the other: P(h1|d) / P(h0|d)



* There are 3 different terms here to know.
* On the LHS = **posterior odds 🡪** tells you what you believe about the relative *plausibility* of the null + the alternative *after seeing the data*
* On the RHS: **prior odds =** what you thought before seeing the data.
* In the middle: the **Bayes factor =** describes the amount of evidence provided by the data:
* **Bayes factor** (**BF**) has a special place in Bayesian hypothesis testing b/c it serves a similar role to the p-value in orthodox hypothesis testing = **quantifies the strength of evidence provided by the data**
* BF is what’s usually reported when running a Bayesian hypothesis test.
* Reason for reporting BF’s rather than posterior odds = different researchers have different priors
* Some might have a strong bias to believe the null is true, others might have a strong bias to believe it is false.
* B/c of this, the polite thing for an applied researcher to do is report the BF
* That way, anyone reading a paper can multiply the BF by their own personal prior odds + can work out for themselves what the posterior odds would be.
* In any case, by convention we like to pretend we give equal consideration to both the null + alternative, in which case the prior odds = 1, + the posterior odds become the same as the BF.
* 1 of the really nice things about BF = *the numbers are inherently meaningful.*
* If in an experiment you compute a BF = 4, it means the evidence provided by your data corresponds to betting odds of 4:1 in favor of the alternative.
* However, there have been some attempts to quantify the standards of evidence that would be considered meaningful in a scientific context.
* The 2 most widely used are from Jeffreys (1961) + Kass + Raftery (1995).
* The Kass + Raftery (1995) table is a bit more conservative.



* Some even think Kass + Raftery standards are a bit charitable + call the “positive evidence” category “weak evidence” + think anything in the range 3:1 to 20:1 is weak/modest evidence at best.
* But there are no hard + fast rules
* What counts as strong or weak evidence depends entirely on how conservative you are + upon the standards your community insists upon before it is willing to label a finding as true.
* In any case, note that all numbers listed above make sense if BF > 1 (i.e., the evidence favors the alternative hypothesis).
* However, 1 big practical advantage of the Bayesian approach relative to the orthodox approach is it also allows you to *quantify evidence for the null*.
* When that happens, BF < 1.
* You can choose to report a BF < 1, but it can be confusing.
* For example, suppose the likelihood of the data under the null P(d|h0) = 0.2, + the corresponding likelihood under the alternative P(d|h0) = 0.1.
* Using the equations above, BF here:



* Read literally, this result tells us the **evidence in favor of the alternative is 0.5:1**
* This can be hard to understand + it can make a lot more sense to turn the equation upside down + report the amount of evidence in favor of the null.



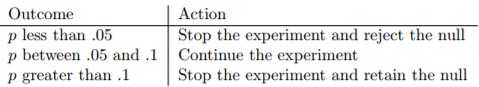
* We report is a BF of 2:1 in favor of the null 🡪 much easier to understand + can be interpreted using the table above

**17.3 Why be a Bayesian?**

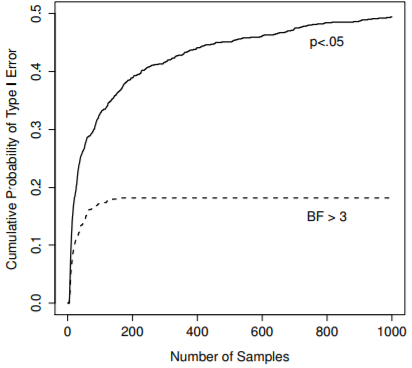
* Up to this point I’ve focused exclusively on logic underpinning Bayesian statistics + talked about the idea of probability as a degree of belief + it implies about how a **rational agent** should reason about the world.
* The question that you have to answer for yourself is this: *how do you want to do your statistics*?
* Do you want to be an orthodox statistician relying on sampling distributions + p-values to guide decisions?
* Or do you want to be a Bayesian, relying on BFs + the rules for rational belief revision?
* Ultimately it depends on what you think is right. It’s your call, + your call alone
* But if one would lean towards Bayesian, statistics mean what you think they mean
* 1 of the biggest advantages to the Bayesian approach is it answers the *right questions*.
* W/in the Bayesian framework, it is perfectly sensible + allowable to refer to the probability a hypothesis is true + can even try to calculate this probability.
* Ultimately, isn’t that what you want your statistical tests to tell you?
* To an actual human being, this would seem to be the whole point of doing statistics: to determine what is true + what isn’t.
* Any time you aren’t exactly sure about what the truth is, you should use the language of **probability theory** to say things like “there is an 80% chance Theory A is true, but a 20% chance Theory B is true instead”
* This seems so obvious to a human, *yet is explicitly forbidden w/in the orthodox framework.*
* To a frequentist, such statements are a nonsense b/c the theory is *true is not a repeatable event*.
* A theory is TRUE or it is NOT, + NO probabilistic statements are allowed, no matter how much you might want to make them.
* There’s a reason why we do not interpret the p-value as probability of the null is true.
* There’s a reason why almost every textbook on statistics is forced to repeat that warning 🡪 b/c people desperately want that to be the correct interpretation.
* Frequentist dogma not w/standing, most actual humans think that the probability a hypothesis is true is not only meaningful, but is the thing we care most about.
* It’s such an appealing idea that even trained statisticians fall prey to the mistake of trying to interpret a p-value this way.
* For example, here is a quote from an official Newspoll report in 2013, explaining how to interpret their (frequentist) data analysis:
* “Throughout the report, where relevant, statistically significant changes have been noted. All significance tests have been based on the 95% level of confidence. This means that if a change is noted as being statistically significant, there is a 95% probability that a real change has occurred, + is not simply due to chance variation.
* *No, that’s not what p < .05 means + is NOT what 95% confidence means to a frequentist statistician*
* It is just plain wrong: *Orthodox methods cannot tell you there is a 95% chance a real change has occurred, b/c this is not the kind of event to which frequentist probabilities may be assigned.*
* To an ideological frequentist, this sentence should be meaningless.
* Even if you’re a more pragmatic frequentist, it’s still the wrong definition of a p-value.
* It is simply not an allowed or correct thing to say if you want to rely on orthodox statistical tools.
* On the other hand, suppose you’re a Bayesian.
* Although the passage is still the wrong definition of a p-value, it’s pretty much exactly what a Bayesian means when they say the **posterior probability** of the alternative is > 95%.
* If the Bayesian posterior is the *actual thing* you want to report, why even try to use orthodox methods?
* If you want to make Bayesian claims, all you have to do is be a Bayesian + use Bayesian tools (maybe the most liberating thing about switching to the Bayesian view)
* Once you’ve made the jump, you no longer have to wrap your head around counterintuitive definitions of p-values + don’t have to bother remembering why you can’t say you’re 95% confident the true mean lies w/in some interval.
* All you have to do is be honest about what you believed before you ran the study + then report what you learned from doing it
* This is the big promise of the Bayesian approach 🡪 do the analysis you really want to do, + express what you really believe the data are telling you
* “If [p] is below .02 it is strongly indicated that the [null] hypothesis fails to account for the whole of the facts. We shall not often be astray if we draw a conventional line at .05 + consider that [smaller values of p] indicate a real discrepancy.” – Sir Ronald Fisher (1925)
* Consider the quote above by Sir Ronald Fisher, 1 of the founders of orthodox statistics.
* If anyone has ever been entitled to express an opinion about the intended function of p-values, it’s Fisher.
* He’s pretty clear about what it means to reject a null at p < .05.
* In his opinion, if we take p < .05 to mean there is a *real effect*, we shall not often be astray.
* This is hardly unusual 🡪 most practitioners express views very similar to Fisher’s.
* In essence, the p < .05 convention is assumed to represent a fairly stringent **evidentiary standard**.
* *Well, how true is that?*
* 1 way to approach this question is to try to *convert p-values to BFs* + see how the 2 compare.
* It’s not easy to do b/c *a p-value is a fundamentally different kind of calculation to a BF* + *they don’t measure the same thing*.
* However, there have been some attempts to work out the relationship between the 2, + it’s somewhat surprising
* For example, Johnson (2013) presents a pretty compelling case that (for t-tests at least) the p < .05 threshold corresponds roughly to a BF of somewhere between 3:1 + 5:1 in favor of the alternative.
* If that’s right, Fisher’s claim is a bit of a stretch.
* Suppose the null is true about ½ the time (i.e., prior probability of H0 = 0.5) + we use those numbers to work out that the posterior probability of the null given it has been rejected at p < .05.
* Using the data from Johnson (2013), we see that if you reject the null at p < .05, you’ll be correct about 80% of the time.
* An evidentiary standard that ensures you’ll be wrong on 20% of your decisions isn’t good enough.
* The fact remains that, quite contrary to Fisher’s claim, if you reject at p < .05 you shall quite often go astray
* It’s not a very stringent evidentiary threshold at all.
* At this point you might be thinking the real problem is not w/ orthodox statistics, just the p < .05 standard.
* In 1 sense, that’s true: The recommendation Johnson gives is not that everyone must be a Bayesian
* Instead, the suggestion is it would be wiser to shift the conventional standard to something like a p < .01 level.
* That’s not an unreasonable view to take, but the problem is a little more severe than that.
* There’s a fairly big problem built into the way most (not all) orthodox hypothesis tests are constructed
* They’re grossly naive about how humans actually do research + b/c of this, most p-values are wrong
* Consider the following scenario: You’ve come up w/ a really exciting research hypothesis + design a study to test it.
* You’re very diligent + run a power analysis to work out what your sample size should be + run the study.
* You run your hypothesis test + out pops a p-value = 0.072. Really annoying
* What should you do? Here are some possibilities:
* 1. Conclude there is no effect + try to publish it as a null result
* If you try to publish it as a null result, the paper will struggle to be published.
* Some reviewers will think that p = .072 is *not* really a null result + will argue it’s *borderline* significant.
* Others will agree it’s a null result, but will claim that even though some null results are publishable, *yours* isn’t.
* 1-2 reviewers might even be on your side, but you’ll be fighting an uphill battle to get it through
* 2. Guess that there *might* be an effect + try to publish it as a *borderline significant* result
* Some reviewers will claim it’s a null result + should not be published.
* Others will claim the evidence is ambiguous + that you should collect more data until you get a *clear* significant result.
* Again, the publication process does not favor you
* 3. Give up + try a new study
* Given the difficulties in publishing an ambiguous result like p = .072, this option might seem tempting: give up + do something else.
* But that’s a recipe for career suicide.
* If you give up + try a new project else every time you find yourself faced w/ ambiguity, your work will never be published
* If you’re in academia w/out a publication record you can lose your job.
* 4. Collect some more data to see if the p value goes up or (preferably) drops below the magic criterion of p < .05
* You don’t GET conclusive results + decide to collect more data + re-run the analysis
* Seems sensible, but unfortunately, if you do this, ALL your p-values are now *incorrect*.
* Not *just* the p-values you calculated for this study. All of them. All p-values you calculated in the past + all p-values you will calculate in the future.
* No one will notice, you’ll get published, + *you’ll have lied*.
* 4) sounds like a perfectly reasonable strategy 🡪 You collected some data, results weren’t conclusive, so now you collect more data until the results ARE conclusive.
* There’s nothing “wrong” w/ it 🡪 It’s a reasonable, sensible + rational thing to do.
* In real life, this is exactly what every researcher does.
* Unfortunately, the theory of null hypothesis testing as (Chapter 11) *forbids* you from doing this
* This b/c the theory assumes the experiment is finished + all the data are in
* B/c it assumes the experiment is over, it only considers 2 possible decisions.
* If using the conventional p < .05 threshold, those decisions are:



* What *you’re* doing is adding a 3rd possible action to the decision-making problem 🡪 using the p-value itself as a reason to justify continuing the experiment + as a consequence transformed the decision-making procedure into one that looks more like this:



* \*\*\*Not all orthodox statistical tests rely on this silly assumption.
* There’re a # of sequential analysis tools sometimes used in clinical trials + the like.
* These methods are built on the assumption that data are analyzed as they arrive + these tests aren’t horribly broken in the way I’m complaining about here.
* However, sequential analysis methods are constructed in a *very different* fashion to the standard version of null hypothesis testing.
* They don’t make it into any intro texts + are not very widely used in psychological literature
* The concern I’m raising here is valid for every single orthodox test I’ve presented so far, + for almost every test reported in the papers
* The “basic” theory of null hypothesis testing isn’t built to handle this sort of thing, not in this form
* If you’re the kind of person who would choose to “collect more data” in real life, it implies you are not making decisions in accordance w/ the rules of null hypothesis testing.
* Even if you happen to arrive at the same decision as the hypothesis test, you aren’t following the decision process it implies, + it’s this failure to follow the process that is causing the problem
* Your p-values are a lie in a dangerous way b/c *they’re all too small*.
* Consider the following (worst-case) scenario: You’re an enthusiastic researcher on a tight budget who didn’t pay any attention to my warnings above + design a study comparing 2 groups
* You desperately want to see a significant result at the p < .05 level, but really don’t want to collect any more data than you have to (it’s expensive).
* In order to cut costs, you start collecting data, but every time a new observation arrives you run a t-test on your data.
* If the t-tests says p < .05, you stop the experiment and report a significant result.
* If not, you keep collecting data.
* You keep doing this until you reach your pre-defined spending limit for this experiment which kicks in at N = 1000 observations.
* As it turns out, the truth of the matter is that *there is no real effect to be found*: the null is true
* So, what’s the chance you’ll make it to the end of the experiment + (correctly) conclude there is no effect?
* In an ideal world, the answer here should be 95%.
* After all, the whole point of the p < .05 criterion is to control the Type I error rate at 5%, so we’d hope is there’s only a 5% chance of falsely rejecting the null in this situation.
* However, there’s no guarantee that will be true.
* *You’re breaking the rules 🡪* you’re running tests repeatedly, “peeking” at your data to see if you’ve gotten a significant result, and all bets are off

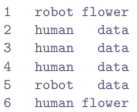


* Look at the solid black line 🡪 If you peek at your data *after every single observation*, there’s a 49% chance you will make a Type I error, quite a bit bigger than the 5% it’s supposed to be
* By comparison, imagine you used the following strategy: Start collecting data 🡺 Every single time an observation arrives, run a Bayesian t-test (Section 17.5) + look at the BF
* Assume Johnson (2013) is right + treat a BF of 3:1 as roughly equivalent to a p-value of .05
* Some readers might wonder why I picked 3:1 rather than 5:1, given Johnson suggests that p < .05 lies somewhere in that range.
* I did so in order to be charitable to the p-value.
* W/ a 5:1 BF instead, the results would look even better for the Bayesian approach
* This time around, our trigger-happy researcher uses the following procedure:
* If the BF is 3:1 or more in favor of the null, stop the experiment + retain the null.
* If it is 3:1 or more in favor of the alternative, stop the experiment + reject the null
* Otherwise continue testing.
* Now, just like last time, assume the null hypothesis is true 🡪 results = dashed line
* It turns out the Type I error rate is much, much lower than the 49% rate resulted from using the orthodox t-test.
* In some ways, this is remarkable.
* *The entire point of orthodox null hypothesis testing is to control the Type I error rate.*
* Bayesian methods aren’t *actually designed to do this at all*.
* Yet, as it turns out, when faced w/ a “trigger-happy” researcher who keeps running hypothesis tests as the data come in, the Bayesian approach is much more effective.
* Even the 3:1 standard, which most Bayesians consider unacceptably lax, is much safer than the p < .05 rule
* This is a pretty extreme situation + IRL people don’t run hypothesis tests every time a new observation arrives.
* So, it’s not fair to say the p < .05 threshold “really” corresponds to a 49% Type I error rate (i.e., p = .49)
* But the fact remains: If you want your p-values to be *honest*, you either have to switch to a completely different way of doing hypothesis tests, or you enforce a strict rule: no peeking
* *You’re not allowed to use the data to decide when to terminate the experiment + are not allowed to look at a “borderline” p-value + decide to collect more data.*
* You aren’t even allowed to change your data analysis strategy *after looking at data*.
* You’re *strictly* required to follow these rules, otherwise p-values calculated will be nonsense.
* And *yes*, these rules are *surprisingly* strict.
* Suppose you started running a study w/ the intention of collecting N = 80 people.
* When the study starts out, you follow the rules, refusing to look at the data or run any tests.
* But when you reach N = 50, your willpower gives in + you take a peek + you’ve got a significant result
* You said you’d keep running the study out to a sample size of N = 80, but it seems sort of pointless now as the result is significant w/ N = 50
* Wouldn’t it be wasteful + inefficient to keep collecting data? Aren’t you tempted to stop?
* Keep in mind that if you do, your Type I error rate at p < .05 just ballooned out to 8%.
* When you report p < .05 in your paper, you’re really saying p = .08.
* That’s how bad the consequences of “just one peek” can be.
* Now consider this: Scientific literature is filled w/ t-tests, ANOVAs, regressions + chi-square tests
* These 4 tools appear in most intro stats texts is b/c *these are the bread + butter tools of science.*
* NONE of these tools include a correction to deal w/ data peeking + they all assume you’re not doing it
* But how realistic is that assumption? IRL, how many people do you think have “peeked” at data before an experiment was finished + adapted subsequent behaviors after seeing what the data looked like?
* Except when a sampling procedure is fixed by an external constraint, most people have done it.
* If that has happened, you can infer that the reported p-values are wrong.
* Worse yet, b/c we don’t know *what decision process they actually followed*, we have no way to know what the p-values *should have been*.
* **You can’t compute a p-value when you don’t know the decision-making procedure a researcher used**
* And so, the reported p-value remains a lie.
* Given all the above the take home message is NOT that Bayesian methods are foolproof
* If a researcher is determined to cheat, they can always do so.
* Bayes’ rule cannot stop people from lying, nor from rigging an experiment.
* Key point 🡪 The reason we run statistical tests is to protect us from ourselves.
* The reason **data peeking** is such a concern is it’s so tempting, even for honest researchers.
* A theory for statistical inference HAS to acknowledge this.
* Yes, you might try to defend p-values by saying it’s the fault of the researcher for not using them properly, but that misses the point.
* A theory of statistical inference that is so completely naive about humans that it doesn’t even consider the possibility a researcher might look at their own data isn’t a theory worth having.
* In essence, my point is this: **Good laws have their origins in bad morals** – Ambrosius Macrobius
* **Good rules for statistical testing have to acknowledge human frailty**.
* None of us are without sin or are beyond temptation.
* A good system for statistical inference should still work even when it is used by actual humans.
* **Orthodox null hypothesis testing does not**
* Some knowledgeable frequentists will dispute this
* If you adopt a sequential analysis perspective, you *can* avoid these errors w/in the orthodox framework
* You *can* explicitly design studies w/ interim analyses in mind.
* So yes, in 1 sense this is attacking a straw man version of orthodox methods.
* However, the straw man is the 1 used by almost every single practitioner.
* If it ever reaches the point where sequential methods become the norm among experimental psychologists, default Bayes factor methods are much more robust in the face of data analysis practices as they exist in the real world.

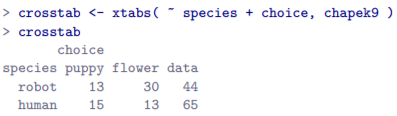
**17.4 Bayesian analysis of contingency tables**

* Statistical inference problem 🡪 categorical data analysis = analysis of **contingency tables**.
* In this kind of data analysis, we have a cross-tabulation of 1 variable against another + the goal = to find out if there is some association between these variables.

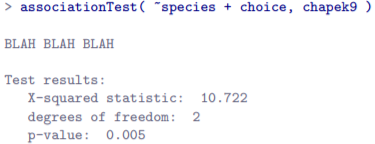
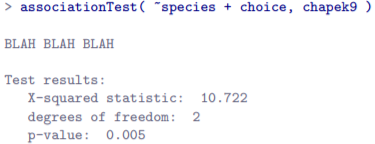




* In this data set, we sampled 180 beings + measured 2 things 🡪 checked if a human or robot (**species**) + then asked them if they most preferred flowers, puppies, or data (**choice**)



* Surprisingly, humans seemed to show a much stronger preference for “data” than robots did.
* The 1st time, we speculated this might have been b/c the questioner was a large robot carrying a gun, + the humans might have been scared
* In Ch. 12, b/c we wanted to determine if there’s some association between species + choice, we used **associationTest**() in the **lsr** package to run a **chi-square test of association**.

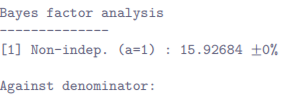


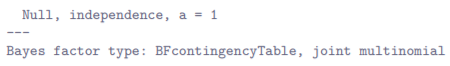
* B/c we found a small p value, we concluded the data are *inconsistent w/ the null of no association*, + we rejected it
* To run an equivalent test as a Bayesian, like every other thing in stats, there’s a lot of different ways you could do it.
* Can use the **BayesFactor** package written by Jeff Rouder + Rich Morey
* For the analysis of contingency tables, BayesFactor contains a **contingencyTableBF**().
* Need to give to this function the contingency table itself (the crosstab variable)



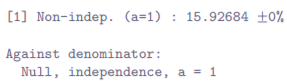
* You’ll get an error message b/c **contingencyTestBF**() needs to know what sampling plan you used to run your experiment, specified w/ **sampleType**, which has 4 experiment design options
* Fixed sample size 🡪 deliberately set out to test 180 people, but we *didn’t try to control the # of humans or robots, nor did try to control the choices they made*.
* In this design, the total # of observations N is fixed, but *everything else is random*.
* This = **joint multinomial sampling** 🡪 specify **sampleType = jointMulti**. In
* Fixed row (or column) totals 🡪 decide ahead of time we want 180 people, but try to be a little more systematic about it.
* Specifically, constrain experiment to get a predetermined # of humans + robots (90 of each)
* In this design, either the row totals or the column totals are fixed, *but not both*.
* This = **independent multinomial sampling** 🡪 specify **sampleType = indepMulti**
* Both row + column totals fixed. 🡪 designed experiment so that both row totals + column totals are fixed.
* Doesn’t make any sense for chapek9, but there are other designs that can work this way.
* Suppose we have a collection of 20 toys + give them 10 blue “boy” + 10 pink “girl” stickers.
* We put the stickers on the 20 toys such that every toy has a color + every toy has a gender.
* No matter how you assign the stickers, the total # of pink + blue toys = 10, as will the # of boys + girls.
* In this design both the rows + columns of the contingency table are fixed = **hypergeometric sampling** 🡪 specify **sampleType = hypergeom**
* Nothing is fixed. 🡪 Nothing is fixed, not row nor column totals, + not the total sample size
* In chapek9, suppose we’d run the study for a fixed length of time + by chance, it turned out we got 180 people to turn up, but it could easily have been something else.
* This = **Poisson sampling** 🡪 specify **sampleType=poisson**.
* Now we have enough knowledge to actually run a test + for chapek9, I implied we designed the study such that the total sample size N was fixed, so we set **sampleType = jointMulti**.



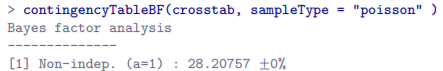




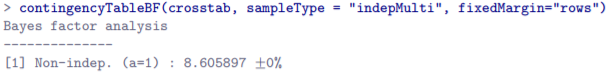
* The output initially looks suspiciously similar to utter gibberish but it’s simple
* Note the stuff at the top + bottom are irrelevant fluff + you already know you’re: doing a BF analysis, analyzing a contingency table, + specified a joint multinomial sampling plan.
* Strip that out + take a look at what’s left over:



* Ignore those 2 **a = 1** bits, since they’re technical details that you don’t need to know about at this stage
* If desperate to know, find all gory details in Gunel + Dickey (1974).
* The help documentation to **contingencyTableBF**() gives this explanation: “the argument **priorConcentration** indexes the expected deviation from the null hypothesis under the alternative, + corresponds to Gunel + Dickey’s (1974) a parameter.”
* Setting a = 1 is a pretty sensible default choice, since it corresponds to an assumption that you have very little **a priori knowledge** about the contingency table.
* The output defines the null for you 🡪*there is no relationship between species + choice* =these 2 variables are independent.
* Look at the line above it + see that **Non-indep**. refers to the alternative 🡪 there IS a relationship between species + choice 🡪 they are NOT independent.
* The only thing left in the output is **15.92684 +/- 0%**
* **15.9** part = the BF 🡪 tells you the odds for the alternative against the null are about 16:1.
* The **+/- 0%** is not very interesting
* Essentially, all it tells you is R has calculated an *exact* BF, so the uncertainty about the BF is 0%
* In any case, the data are telling us we have moderate evidence for the alternative hypothesis
* This number is not always 0% b/c the **BayesFactor** package often has to run some simulations to compute *approximate* Bayes factors.
* The answers you get won’t always be identical when you run the command a 2nd time.
* That’s why the output of these functions tells you what the margin for error is.
* When writing up the results, there aren’t quite so many rules for how to report Bayesian hypothesis tests.
* That might change in the future if Bayesian methods become standard, but use common sense
* Ex: Avoid writing: “A Bayesian test of association found a significant result (BF = 15.92)”
* This write up is unclear.
* Even assuming you’ve already reported relevant descriptive statistics, there are a # of unsatisfactory things here.
* 1st, the concept of statistical significance is pretty closely tied w/ p-values, so it reads slightly strangely
* 2nd, **BF = 15.92** part will only make sense to people who already understand Bayesian methods, + not everyone does.
* 3rd, it is somewhat unclear exactly *which* test was run + *what software* was used to do so.
* On the other hand, unless precision is extremely important, the following is taking things a step too far:
* *We ran a Bayesian test of association (Gunel & Dickey, 1974) using version 0.9.10-1 of the BayesFactor package (Morey & Rouder, 2015) using default priors + a joint multinomial sampling plan. The resulting BF of 15.92 to 1 in favor of the alternative hypothesis indicates that there is moderately strong evidence for the non-independence of species + choice.*
* Everything about that passage is correct, of course.
* Morey + Rouder built their Bayesian tests of association using the paper by Gunel + Dickey, the specific test we used assumes the experiment relied on a joint multinomial sampling plan, + indeed the BF of 15.92 is moderately strong evidence.
* It’s just far too wordy. In most situations you just don’t need that much info.
* Usually, go for something a little briefer.
* 1st, if reporting multiple BF analyses in a write up, then you only need to cite the software *once*, *somewhere*, at the *beginning of the results section*.
* So, you might have 1 sentence like this:
* *“All analyses were conducted using the BayesFactor package in R (Morey & Rouder, 2015), + unless otherwise stated default parameter values were used”*
* Notice I don’t bother including the version # b/c the citation itself includes that info
* There’s no need to clutter up your results w/ redundant info almost no-one will actually need.
* When you get to the *actual test*, you can get away w/ this:
* **“A test of association produced a Bayes Factor of 16:1 in favor of a relationship between species + choice”**
* We’ve rounded 15.92 to 16, b/c there’s no important difference between 15.92:1 + 16:1.
* I spelled out “Bayes Factor” rather than truncating it to BF b/c not everyone knows the abbreviation + I indicated exactly *what* the effect is (i.e., a relationship between species + choice) + *how strong* the evidence was.
* I didn’t bother indicating whether this was moderate or strong evidence, b/c *the odds themselves tell you!*
* There’s nothing stopping you from including that info, + but you don’t strictly need it.
* Similarly, I didn’t bother to indicate I ran the joint multinomial sampling plan, b/c I’m assuming the method section of the write up would make clear how the experiment was designed.
* Neither did I bother indicating this was a Bayesian test of association: if your reader can’t work that out from the fact you’re reporting a BF + you’re citing the BayesFactor package for all your analyses, there’s no chance they’ll understand anything you’ve written.
* For the **Poisson sampling** plan (nothing fixed):



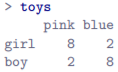
* Notice BF = 28:1 here is not identical to BF = 16:1 obtained from the last test.
* **The sampling plan actually does matter.**
* What about the design in which the row columns (or column totals) are fixed?
* Need to specify whether you fixed the rows or the columns
* Suppose we deliberately sampled 87 humans + 93 robots, so we’d need to indicate the **fixedMargin** of the contingency table is the rows.



* Again, the BF is different, w/ the evidence for the alternative dropping to a mere 9:1.
* As expected, answers would be different again if the columns of the contingency table were fixed
* Finally, if we turn to **hypergeometric sampling** in which *everything is fixed*, we get:



* Some quick reading through the help files hints support for larger contingency tables is coming, but it’s not been implemented yet.
* In the meantime, imagine we have data from the toy labelling experiment described earlier:

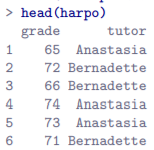
 



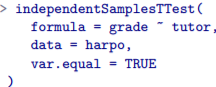
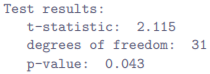
* The BF = 8:1 provides *modest* evidence that the labels were being assigned in a way that correlates gender w/ color, but it’s not conclusive.

**17.5 Bayesian t-tests**

* The 2nd type of statistical inference problem = the comparison between 2 means, t-tests (Ch. 13).
* Recall there are several versions of the t-test + BayesFactor package contains a function **ttestBF**() that is flexible enough to run several different versions of the t-test.
* Independent samples t-test
* Most common type of t-test = **independent samples t-test** which arises when you have data that look something like this:

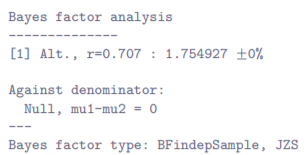


* In this data set, we have 2 groups of students = received lessons from A or from B
* The question we want to answer is whether *there’s any difference in the grades received by these 2 groups of students*.
* Back in Ch. 13, we analyzed this kind of data using **independentSamplesTTest**() in **lsr** package.
* For example, to run a **Student’s t-test:**

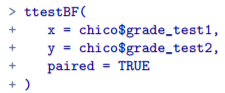
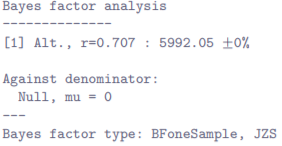
 

* Again, we obtain a p-value < 0.05, so we reject the null.
* Using **ttestBF**(), we can obtain a *Bayesian analog* of Student’s independent samples t-test:





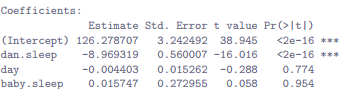
* Notice there’s no analog of the **var.equal** argument b/c the BayesFactor package does NOT include an analog of the **Welch test**
* Apparently this omission is deliberate b/c the author was of the opinion that when homoscedasticity is violated, results of a t-test are uninterpretable.
* In any case, when you run this command you get this as the output:
* **BFindepSample** tells says we ran an independent samples t-test, + **JZS** is technical info beyond our scope
* relates to how the Bayesian test expresses the **prior uncertainty** about the variance σ2
* it’s short for the names of 3 people: Jeffreys Zellner Siow
* See Rouder, Speckman, Sun, Morey, + Iverson (2009) for details.
* The text “Null, mu1-mu2 = 0” tells you the null is that there are no differences between means.
* The only part that really matters is: **[1] Alt., r=0.707 : 1.754927 +/- 0%**
* Ignore **r = 0.707 part**, which refers to a technical detail we won’t worry about in this chapter
* null here specifies an effect size of 0, since the 2 means are identical.
* The alternative states there IS an effect, but doesn’t specify exactly *how big* it will be.
* **r** here relates to *how big the effect is expected to be according to the alternative*
* Check ?ttestBF to get more details.
* Instead, focus on **1.754927**, the BF 🡺 the evidence provided by these data are about 1.8:1 in favor of the alternative.
* Before moving on, it’s worth highlighting the difference between the orthodox + Bayesian test results
* According to the orthodox test, we obtained a significant result, though only barely.
* Nevertheless, many people would happily accept p = .043 as reasonably strong evidence for an effect
* In contrast, notice the Bayesian test doesn’t even reach 2:1 odds in favor of an effect, + would be considered *very weak evidence* at best.
* In my experience that’s a pretty typical outcome, as Bayesian methods usually require more evidence before rejecting the null.
* Paired samples t-test
* Section 13.5 w/ the chico data (students grades measured on 2 tests), we were interested in finding out whether grades went up from test 1 to test 2.
* B/c every student did both tests, we used a **paired samples t-test** to analyze the data using **pairedSamplesTTest**() in the **lsr** package
* This time we’ll use **ttestBF**() from **BayesFactor** to do the same thing.
* The easiest way to do it w/ this data set is to use the x argument to specify 1 variable + y argument to specify the other.
* Then, all we need to do is specify **paired=TRUE** to tell R this is a paired samples test.

* The data provide evidence of about 6000:1 in favor of the alternative, so we reject the null w/ great confidence

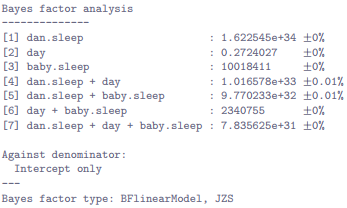
**17.6 Bayesian regression**

* Okay, so now we’ve seen Bayesian equivalents to orthodox chi-square tests + t-tests.
* Ch. 15 🡪 used parenthood data to illustrate basic ideas behind regression + proposed a theory in which grumpiness (dan.grump) on any given day = related to amount of sleep the night before (dan.sleep), + possibly amount of sleep baby got (baby.sleep), though probably not to the day on which we took the measurement.
* Tested this w/ a regression model
* Hypothesis tests for each of term in the regression model were extracted using **summary**()



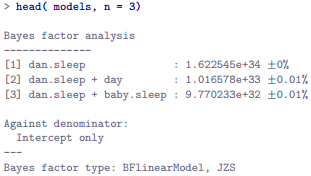
* When interpreting the results, each row = a possible predictor.
* The Intercept term isn’t usually interesting, though it is highly significant.
* Important thing for our purposes = dan.sleep IS significant at p < .001 + other variables aren’t
* Do same thing using the **BayesFactor** package 🡪 **regressionBF**() instead of **lm**().



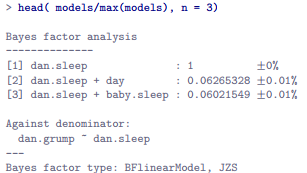


* At the bottom = technical rubbish, + at the top = info about the BF’s.
* What’s new = we seem to have lots of BFs here
* To understanding this output = recognize that if we’re interested in working out *which* of the 3 predictor variables are related to **dan.grump,** there are actually *8* possible regression models that could be considered.
* 1 possibility = the intercept-only model, in which none of 3 variables have an effect.
* At the other end of the spectrum = the full model in which all 3 variables matter.
* regressionBF() treats the intercept-only model as the null, + prints out the BFs for all other models *when compared against that null.*
* Line 4 = the evidence is about 1033:1 in favor of the claim that a model that includes both dan.sleep + day is better than the intercept-only model.
* Line 1 = odds are about 1.6ˆ1034:1 that a model containing dan.sleep (but no others) is better than the intercept-only model
* In practice, this isn’t super helpful + in most situations, that you don’t really care about the intercept-only model
* Helpful 🡪 start out by working out which model is the best + *then seeing how well all alternatives compare to it*.
* In this case, it’s easy to see the best model = *contains dan.sleep only* b/c it has the largest BF.
* However, for a lot of possible models in the output, it’s handy to know you can use **head**() to pick out the best few models.
* 1st, go back + save the BF info to a variable + check top 3 models



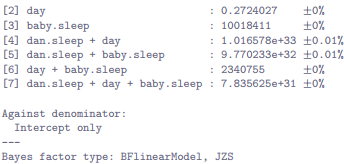


* This says the model in line 1 (dan.grump ~ dan.sleep) is the best one.
* That’s *almost* what I’m looking for, but it’s still comparing all models against the intercept-only model
* What I’d *like* to know is how big the difference is between the *best model* + the other good models.
* For that, there’s a trick:

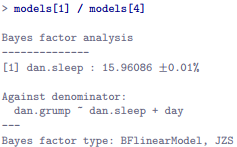


* Notice the bit at the bottom showing the denominator changed 🡪 *this means the BFs are now comparing each of those 3 models listed against the dan.grump ~ dan.sleep model.*
* Obviously, the BF in the 1st line is *exactly* 1, since it’s comparing the best model to itself.
* More to the point, the other 2 BFs are both < 1, indicating they’re all worse than that model.
* BFs of 0.06:1 imply the odds for the best model over the 2nd best model are about 16:1 (0.06/1 = 16)
* Instead of using simple math, we can directly compare the models

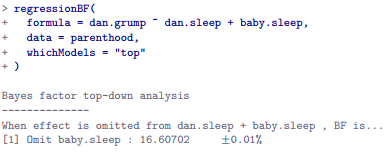


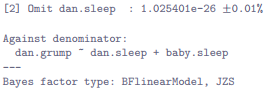


* The best model corresponds to row 1 in this table, + the 2nd best model corresponds to row 4.
* To compare these 2 models:



* Now we’ve found the regression model w/ the highest BF (dan.grump ~ dan.sleep) + we know the evidence for that model over the next best alternative (dan.grump ~ dan.sleep + day) is about 16:1.
* Say you’ve settled on a specific regression model. *What BFs should you report*?
* In this example, pretend we’ve decided **dan.grump ~ dan.sleep + baby.sleep** is the model you think is best.
* Sometimes it’s sensible to do this, even when it’s not the one w/ the highest BF b/c we have a substantive theoretical reason to prefer 1 model over the other.
* Having figured out which model you prefer, it can be really useful to call **regressionBF**() + specifying **whichModels = top**.
* You use your preferred model as the formula argument, + the output will show the BFs that result when you try to drop predictors from this model:

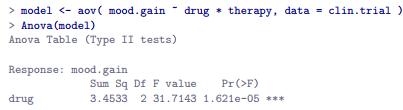




* The BF when we try to drop the dan.sleep predictor is about 1^-26, which is *very strong evidence you shouldn’t drop it*.
* On the other hand, the BF actually goes UP to 17 if you drop baby.sleep, so you’d usually say that’s pretty strong evidence for dropping that one.

**17.7 Bayesian ANOVA**

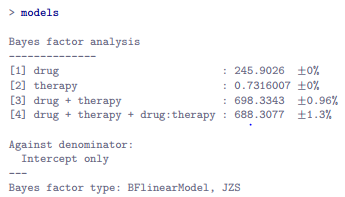
* BayesFactor pckg is pretty flexible + can do Bayesian versions of pretty much everything in this book
* ANOVA reminder 🡪 clin.trial data
* To run our orthodox analysis, we used **aov**() + used **Anova**() from the **car** package to produce the ANOVA table (it uses Type II tests by default) (Ch 16.10)
* Here’s what our analysis looked like:



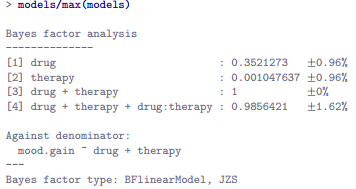


* This is pretty clearly showing evidence for a main effect of drug at p < .001, an effect of therapy at p < .05 + *no interaction* (p > .05)
* BayesFactor contains **anovaBF**() to do a Bayesian ANOVA w/ a pretty standard formula + data structure
* Useful to save the output to a variable:





* Output is different to the traditional ANOVA but similar to the output obtained from **regressionBF**()
* Remember that (Ch. 16.6) under the hood, ANOVA is no different to regression, + both are just different examples of a linear model.
* B/c of this, **anovaBF**() reports the output in much the same way.
* To ID the best model, we could use the same commands used in the last section.
* 1 quite useful variant is this:



* By dividing the models output by the best model (**max(models)**), R uses the best model (drugs + therapy in this case) as the denominator, which gives a pretty good sense of how close the competitors are.
* For instance, the model that contains the interaction term is *almost* as good as the model w/out the interaction (BF = 0.98 vs BF = 1)
* . In other words, the data do not clearly indicate whether there is or is not an interaction.
* Constructing Bayesian Type II tests
* What do we report as the alternative to the p-value?
* In a classical ANOVA table, you get a single p-value for every predictor in the model, so you can talk about the significance of each effect.
* The Bayesian analog of this is tricky b/c ANOVA is complicated.
* Even in classical ANOVA there are several different things ANOVA might correspond to.
* You get different p-values depending on whether you use Type I, Type II or Type III tests.
* To work out which BF is analogous to the p-value in a classical ANOVA, you need to work out *which version of ANOVA you want an analog for.*
* For our purposes of this section, assume we want Type II tests, b/c those are usually most sensible
* (Ch. 16.10) Type II tests for a 2-way ANOVA are reasonably straightforward + we cab pull them from the BF table.
* Suppose we want to test the main effect of **drug**.
* The null for this test corresponds to a model that includes an effect of therapy, but no effect of drug.
* The alternative = the model that includes both.
* In other words, we want the BF corresponding to this comparison:



* As it happens, we can read the answer to *this* straight off the table 🡺 b/c it corresponds to a comparison between the model in line 2 of the table + the model in line 3:



* The BF in this case represents evidence for the null of 0.001 to 1 🡪 the odds are about 1000:1 *against* the null.
* The main effect of **therapy** can be calculated in much the same way.
* In this case, the null model contains only an effect of drug, + the alternative = model that contains both.
* So the relevant comparison is between lines 3 + 2 in the table.



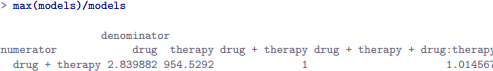
* The odds in favor of the null here are only 0.35:1.
* Again, it’s useful to frame things the other way around, so we’d refer to this as evidence of about 3:1 in favor of an effect of therapy (the alternative)
* Finally, in order to test an **interaction effect**, the null model contains both main effects but no interaction + the alternative model adds the interaction.



* If we look those 2 models in the table, we see this comparison is between models on lines 3 + 4



* The odds of 0.98 to 1 imply these 2 models are fairly evenly matched.
* This is all pretty laborious
* A trick to get a Bayesian Type II ANOVA table from the output of the **anovaBF**() is:



* The output isn’t quite so pretty as the last one, but the nice thing is you can read off everything you need
* The best model = drug + therapy, so all other models are being compared to that.
* BF for main effect of drug 🡪 The relevant null = model that contains only therapy = 954:1
* BF for main effect of therapy 🡪 relevant null = model that contains only drug = 2.83:1 = weaker
* Finally, evidence against an interaction is very weak, at 1.01:1.
* Reading the results off this table is sort of counterintuitive, b/c you have to read off the answers from the wrong part of the table
* evidence for effect of drug = read from the column labeled therapy,
* In any case, if you know what you’re looking for, you can look at this table + report the results of the Bayesian analysis in a way that is pretty analogous to how you’d report a regular Type II ANOVA.
* There’s still no convention on how to do that, but I usually go for something like this:
* “*A Bayesian Type II ANOVA found evidence for main effects of drug (BF: 954:1) + therapy (BF: 3:1), but no clear evidence for or against an interaction (BF: 1:1)*”

**17.8 Summary**

* 1st half of this chapter focused on theoretical underpinnings of Bayesian statistics 🡪 introduced mathematics for how Bayesian inference works (17.1), + a very basic overview of how Bayesian hypothesis testing is typically done (17.2), + why Bayesian methods are worth using (17.3).
* The 2nd half was a lot more practical + focused on tools provided by BayesFactor package
* Used **contingencyTableBF**() to do Bayesian analogs of chi-square tests (17.4), **ttestBF**() to do Bayesian t-tests, (17.5), **regressionBF**() to do Bayesian regressions, + **anovaBF**() for Bayesian ANOVA.
* If interested in learning more about the Bayesian approach, there are many good books you could look into.
* John Kruschke’s Doing Bayesian Data Analysis + is a nice mix of theory + practice.