Excursion 4 Tour II: Rejection Fallacies: Whose Exaggerating What?

If you want to eat nothing, eat nouvelle cuisine. Do you know what it means? No food. The smaller the portion the more impressed people are, so long as the food's got a fancy French name, haute cuisine. An empty plate with sauce!

SIR: The Severity Interpretation of a Rejection in test T+: *(small P-value)*

(i): [*Some* discrepancy is indicated]: $d(\boldsymbol{x}_0)$ is a good indication of $\mu > \mu_1 = \mu_0 + \gamma$ if there is a high probability of observing a *less* statistically significant difference than $d(\boldsymbol{x}_0)$ if $\mu = \mu_0 + \gamma$.

(ii): [I'm not *that* impressed]: $d(\boldsymbol{x}_0)$ is a poor indication of $\mu > \mu_1 = \mu_0 + \gamma$ if there is a high probability of an even more statistically significant difference than $d(\boldsymbol{x}_0)$ even if $\mu = \mu_0 + \gamma$.

Do P-Values Exaggerate the Evidence?

I. J. Berger and Sellke and Casella and R. Berger

II. Jeffreys-Lindley Paradox; Bayes/Fisher Disagreement

III. Redefine Statistical Significance

Common criticism: “Significance levels (or P-values) *exaggerate* the evidence against the null hypothesis”

What do you mean by exaggerating the evidence against H_0 ?

Answer: The P-value is too small, for ex.:

What I mean is that when I put a lump of prior weight π_0 of 1/2 on a point null H_0 (or a very small interval around it), the P-value is smaller than my Bayesian posterior probability on H_0.

(p.246)

"P-values exaggerate": if inference is appraised via one of the probabilisms–Bayesian posteriors, Bayes factors, or likelihood ratios–the evidence against the null isn't as big as 1 – P.

- On the other hand, the probability H_0 *would have survived is* 1 – P
- Difference in the role for probabilities

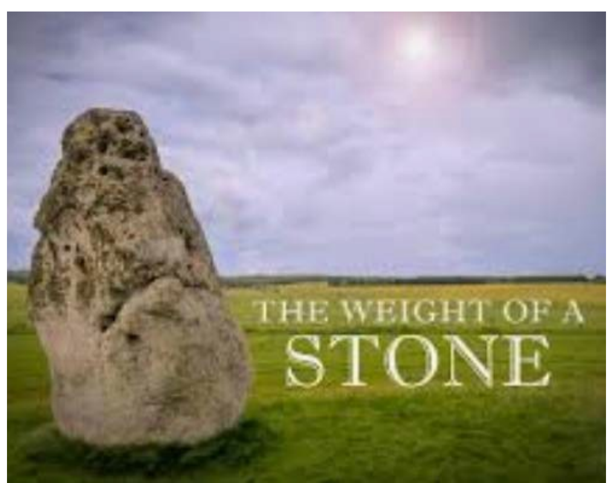
You might react by observing that:

- P-values are not intended as posteriors in H_0 (or Bayes factors, likelihood ratios)

- Why suppose a P-value should match numbers computed in very different accounts.

When the criticism is in the form of a posterior:

> …[S]ome Bayesians in criticizing P-values seem to think that it is appropriate to use a threshold for significance of 0.95 of the probability of the alternative hypothesis being true. This makes no more sense than, in moving from a minimum height standard (say) for recruiting police officers to a minimum weight standard, declaring that since it was previously 6 foot it must now be 6 stone (Senn 2001, p. 202).



Getting Beyond "I'm Rubber and You're Glue". P. 247

- The danger in critiquing statistical method X from the standpoint of a distinct school Y, is that of falling into begging the question.

- Whatever you say about me bounces off and sticks to you. This is a genuine worry, but it's not fatal.

- The minimal theses about “bad evidence no test (BENT)” enables scrutiny of any statistical inference account–at least on the meta-level.

- Why assume all schools of statistical inference embrace the minimum severity principle?

- I don’t, and they don’t.

- But by identifying when methods violate severity, we can pull back the veil on at least one source of disagreement behind the battles.

This is a “how to” book

- We do not depict critics as committing a gross blunder (confusing a P-value with a posterior probability in a null).
- Nor just deny we care about their measure of support: I say we should look at exactly what the critics are on about.

Bayes Factor (bold part)

$$\frac{\Pr(H_0|x)}{\Pr(H_1|x)} = \frac{\mathbf{Pr}(\boldsymbol{x}|\boldsymbol{H_0})\Pr(H_0)}{\mathbf{Pr}(\boldsymbol{x}|\boldsymbol{H_1})\Pr(H_1)}$$

- Likelihood ratio but not limited to point hypothesis
- The parameter is viewed as a random variable with a distribution

J. Berger and Sellke,
and Casella and R. Berger.

Berger and Sellke (1987) make out the conflict between P-values and Bayesian posteriors using the two-sided test of the Normal mean, H_0: $\mu = 0$ versus H_1: $\mu \neq 0$.

"Suppose that $\boldsymbol{X} = (X_1,\ldots,X_n)$, where the X_i are IID $N(\mu, 0^2)$, 0^2 known" (p. 112).

Then the test statistic $d(\boldsymbol{X}) = \sqrt{n}\, |\bar{X} - \mu_0|/0$, and the P-value will be twice the P-value of the corresponding one-sided test.

By titling their paper: “The irreconcilability of P-values and evidence,” Berger and Sellke imply that if P-values disagree with posterior assessments, they can’t be measures of *evidence at all*.

Casella and R. Berger (1987) retort that “reconciling” is at hand, if you move away from the lump prior.

First, Casella and Berger: Spike and Smear

Starting with a lump of prior, 0.5, on H_0, they find the posterior probability in H_0 is larger than the P-value for a variety of different priors assigned to the alternative.

The result depends entirely on how the remaining .5 is smeared over the alternative

- Using a Jeffreys-type prior, the .5 is spread out over the alternative parameter values as if the parameter is itself distributed $N(\mu_0,0)$.

- Actually Jeffreys recommends the lump prior only when a special value of a parameter is deemed plausible*

- The rationale is to enable it to receive a reasonable posterior probability, and avoid a 0 prior to H_0

“P-values are reasonable measures of evidence of evidence when there is no a priori concentration of belief about H_0 (Berger and Delampady)

Table 4.1 SIST p. 249
(From J. Berger and T. Sellke (1987))

Table 4.1 Pr(H_0|$\boldsymbol{x}$) for Jeffreys-type prior

		n (sample size)				
P one-sided	z_α	**10**	**20**	**50**	**100**	**1000**
0.05	1.645	0.47	0.56	0.65	0.72	0.89
0.025	1.960	0.37	0.42	0.52	0.60	0.82
0.005	2.576	0.14	0.16	0.22	0.27	0.53
0.0005	3.291	0.024	0.026	0.034	0.045	0.124

(From Table 1, J. Berger and T. Sellke (1987) p. 113 using the one-sided *P*-value)

- With n = 50, “one can classically ‘reject H_0 at significance level p = .05,’ although $\Pr(H_0|\boldsymbol{x}) = .52$ (which would actually indicate that the evidence *favors* H_0)” (Berger and Sellke, p. 113).

If n = 1000, a result statistically significant at the .05 level has the posterior probability to $\mu = 0$ go up from .5 (the lump prior) to .82!

From their Bayesian perspective, this seems to show P-values are exaggerating evidence against H_0.

From an error statistical perspective, this allows statistically significant results to be interpreted as no evidence against H_0–or even evidence *for* it!

(posterior H_0. is higher than the prior-B-boost)

- After all, 0 is excluded from the 2-sided confidence interval at level .95.
- The probability of declaring evidence for the null even if false is high.

- Why assign the lump of ½ as prior to the point null?

"The choice of $\pi_0 = 1/2$ has obvious intuitive appeal in scientific investigations as being 'objective'" Berger and Sellke (1987, p. 115).

- But is it?

- One starts by making H_0 and H_1 equally probable, then the .5 accorded to H_1 is spread out over all the values in H_1:

A Dialogue at the Water Plant Accident (p.251)

EPA Rep: The mean temperature of the water was found statistically significantly higher than 150 degrees at the 0.025 level.

Spiked Prior Rep: This even strengthens my belief the water temperature's no different from 150. If I update the prior of 0.5 that I give to the null hypothesis, my posterior for H_0 is still 0.6; it's not 0.025 or 0.05, that's for sure.

EPA Rep: Why do you assign such a high prior probability to H_0?

Spiked Prior Rep: If I gave H_0 a value lower than 0.5, then, if there's evidence to reject H_0, at most I would be claiming an improbable hypothesis has become more improbable.

[W]ho, after all, would be convinced by the statement 'I conducted a Bayesian test of H_0, assigning prior probability 0.1 to H_0, and my conclusion is that H_0 has posterior probability 0.05 and should be rejected?' (J. Berger and Sellke 1987, p. 115).

But it's scarcely an obvious justification for a lump of prior on the null H_0 that it ensures, if they *do* reject H_0, there will be a meaningful drop in its probability.

Casella and R. Berger (1987) charge that "concentrating mass on the point null hypothesis is biasing the prior in favor of H_0 as much as possible" (p. 111) whether in 1 or 2-sided tests.

According to them,

> The testing of a point null hypothesis is one of the most misused statistical procedures. In particular, in the location parameter problem, the point null hypothesis is more the mathematical convenience than the statistical method of choice (ibid. p. 106).

Most of the time "there is a direction of interest in many experiments, and saddling an experimenter with a two-sided test would not be appropriate"(ibid.).

Jeffreys-Lindley "Paradox" or Bayes/Fisher Disagreement (p. 250)

The disagreement (between the P-value and the posterior can be dramatic

With a lump given to the point null, and the rest appropriately spread over the alternative, an *n* can be found such an α significant result corresponds to

$\Pr(H_0|\boldsymbol{x}) = (1 - \alpha)!$

Contrasting Bayes Factors p. 254

They arise in prominent criticisms and/or reforms of significance tests.

1. *Jeffrey-type prior with the "spike and slab" in a two sided test.* Here, with large enough *n*, a statistically significant result becomes evidence *for* the null; the posterior to H_0 exceeds the lump prior.

2. *Likelihood ratio most generous to the alternative*. Second, there's a spike to a point null, to be compared to the point alternative that's maximally likely θ_{max}.

3. *Matching*. Instead of a spike prior on the null, it uses a smooth diffuse prior. Here, the P-value "is an approximation to the posterior probability that $\theta < 0$" (Pratt 1965, p. 182).

Why Blame Us Because You Can't Agree on Your Posterior?

Stephen Senn argues, "…the reason that Bayesians can regard P-values as overstating the evidence against the null is simply a reflection of the fact that Bayesians can disagree *sharply* with each other" (Senn 2002, p. 2442).

Senn riffs on the well-known joke of Jeffreys that we heard in 3.4 (1961, p. 385):

> It would require that a procedure is dismissed [by significance testers] because, when combined with information which it doesn't require and which may not exist, it disagrees with a [Bayesian] procedure that disagrees with itself. Senn (ibid. p. 195)

Exhibit (vii). *Jeffrey-Lindley 'paradox'*

A large number (n =527,135) of independent collisions either of type A or type B will test if the proportion of type A collisions is exactly .2, as opposed to any other value.

n Bernoulli trials, testing H_0: θ = .2 vs. H_1: θ ≠ .2.

The observed proportion of type A collisions is scarcely greater than the point null of .2:

$\bar{x}$ = k/n = *.20165233* where n=527,135; k = 106,298.

Example from Aris Spanos (2013) (from Stone 1997.)

The significance level against H_0 is small

- the result $\bar{x}$ is highly significant, even though it's scarcely different from the point null.

The Bayes Factor in favor of H_0 is high

- H_0 is given the spiked prior of .5, and the remaining .5 is spread equally among the values in H_1.

The Bayes factor $B_{01} = Pr(k|H_{\#}) / Pr(k|H_{\$})$ = .000015394/.000001897 = 8.115

While the likelihood of H_0 in the numerator is tiny, the likelihood of H_1 is even tinier.

There's no surprise once you consider the Bayesian question here: compare the likelihood of a result scarcely different from 0.2 being produced by a universe where θ = 0.2 – where this has been given a spiked prior of 0.5 under H_0 – with the likelihood of that result being produced by any θ in a small band of θ values, which have been given a very low prior under H_1. Clearly, $\theta = 0.2$ is more likely, and we have an example of the Jeffreys–Fisher disagreement.

Clearly, θ = .2 is more likely, and we have an example of the Jeffreys-Fisher disagreement.
SIST p. 255

Compare it with the second kind of prior:

Here Bayes factor $B_{01} = 0.01$; $B_{10} = Lik(\theta_{max})/Lik(.2) = 89$

Why should a result 89 times more likely under alternative θ_{max} than under $\theta = .2$ be taken as strong evidence *for* $\theta = .2$?

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Contrasting Bayes Factors p. 254

1. *Jeffrey-type prior with the "spike and slab" in a two sided test.* Here, with large enough *n*, a statistically significant result becomes evidence *for* the null; the posterior to H_0 exceeds the lump prior.

2. *Likelihood ratio most generous to the alternative*. Second, there's a spike to a point null, to be compared to the point alternative that's maximally likely θ_{max}.

3. *Matching*. Instead of a spike prior on the null, it uses a smooth diffuse prior. Here, the P-value "is an approximation to the posterior probability that $\theta < 0$" (Pratt 1965, p. 182).

Bayesian Family feuds

It shouldn't, according to some, including Lindley's own student, default Bayesian José Bernardo (2010). (SIST p. 256, Note 7)

Yet it's at the heart of recommended

reforms First, look at p. 256 on matching

priors

Greenland and Poole 2013, SIST, p. 256

matching result in # 3, Exhibit (vii). An uninformative prior, assigning equal probability to all values of the parameter, allows the *P*-value to approximate the posterior probability that $\theta < 0$ in one-sided testing ($\theta \leq 0$ vs. $\theta > 0$). In two-sided testing, the posterior probability that θ is on the opposite side of 0 than the observed is *P*/2. They proffer this as a way "to live with" *P*-values.

4.5 Reforms (Redefine Significance) Based on Bayes Factor Standards

“Redefine Significance” is recent, but, like other reforms, is based on old results:

> Imagine all the density under the alternative hypothesis concentrated at ***x***, the place most favored by the data. …Even the utmost generosity to the alternative hypothesis cannot make the evidence in favor of it as strong as classical significance levels might suggest (Edwards, Lindman, and Savage 1963, p. 228).

Normal testing case of Berger and Sellke, but as a one-tailed test of H_0: $\mu = 0$ vs. H_1: $\mu = \mu_1 = \theta_{max}$.

We abbreviate H_1 by H_{max}.

Here the likelihood ratio $Lik(\theta_{max})/Lik(\theta_0) = \exp[z^2/2]$);

the inverse is $Lik(\theta_0)/Lik(\theta_{max})$, is $\exp[-z^2/2]$.

What is θ_{max}?

It's the observed mean $\bar{x}$ (whatever it is), and we're to consider $\bar{x}$ = the result that is just statistically significant at the indicated P-value.

SIST p. 260 (see note #9)

Normal Distribution

$$y = \frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{(x-\mu)^2}{2\sigma^2}}$$

$$\begin{aligned}
\mu &= \text{ Mean} \\
\sigma &= \text{ Standard Deviation} \\
\pi &\approx 3.14159\cdots \\
e &\approx 2.71828\cdots
\end{aligned}$$

Table 4.2 Upper Bounds on the Comparative Likelihood

P-value: one-sided	z_α	$\mathrm{Lik}(\mu_{\max})/\mathrm{Lik}(\mu_0)$
0.05	1.65	3.87
0.025	1.96	6.84
0.01	2.33	15
0.005	2.58	28
0.0005	3.29	227

To ensure H_{max}: $\mu = \mu_{\max}$ is 28 times as likely as H_0: $\theta = \theta_0$, you'd need to use a P-value ~.005, z value of 2.58.

- **Valen Johnson** (2013a,b): a way to bring the likelihood ratio more into line with what counts as strong evidence, according to a Bayes factor.

- "The posterior odds between two hypotheses H_1 and H_0 can be expressed as"

$$\frac{\Pr(H_1|\boldsymbol{x})}{\Pr(H_0|\boldsymbol{x})} = \mathrm{BF}_{10}(\boldsymbol{x}) \times \frac{\Pr(H_1)}{\Pr(H_0)}\ .$$

"In a Bayesian test, the null hypothesis is rejected if the posterior probability of H_1 exceeds a certain threshold. …."(Johnson 2013b, p. 1721)

- and “the alternative hypothesis is accepted if $BF_{10} > k$”

- Johnson views his method as showing how to specify an alternative hypothesis–he calls it the “implicit alternative”

- It will be H_{max}

- Unlike N-P, the test does not exhaust the parameter space, it’s just two points.

Johnson offers an illuminating way to relate Bayes factors and standard cut-offs for rejection in UMP tests

- (SIST p. 262) Setting k as the Bayes factor you want, you get the corresponding cut-off for rejection by computing $\sqrt{(2\log k)}$: this matches the z_α corresponding to a N-P, UMP one-sided test.

- The UMP test (with $\mu > \mu_0$) is of the form:

Reject H_0 iff $\bar{X} \geq \bar{x}_\alpha$ where $\bar{x}_\alpha = \mu_0 + z_\alpha\, 0/\sqrt{n}$, which is $z_\alpha\, 0/\sqrt{n}$ for the case $\mu_0 = 0$.

Table 4.3 (SIST p. 262), computations note #10 p. 264

Table 4.3| V. Johnson's implicit alternative analysis for T+: H_0: $\mu \leq 0$ vs. H_1: $\mu > 0$

***P*-value one-sided**	z_α	$\mathrm{Lik}(\mu_{max})/\mathrm{Lik}(\mu_0)$	μ_{max}	$\Pr(H_0 \mid x)$	$\Pr(H_{max} \mid x)$
0.05	1.65	3.87	$1.65\sigma/\sqrt{n}$	0.2	0.8
0.025	1.96	6.84	$1.96\sigma/\sqrt{n}$	0.128	0.87
0.01	2.33	15	$2.33\sigma/\sqrt{n}$	0.06	0.94
0.005	2.58	28	$2.58\sigma/\sqrt{n}$	0.03	0.97
0.0005	3.29	227	$3.3\sigma/\sqrt{n}$	0.004	0.996
	$\sqrt{(2 \log k)}$	$exp\left(\frac{z_\alpha^2}{2}\right)$	$z_\alpha \sigma/\sqrt{n}$	$1/(1+k)$	$k/(1+k)$

$$Pr(H_0|x) = \frac{Pr(x|H_0)\,Pr(H_0)}{Pr(x|H_0)\,Pr(H_0) + P(x|H_{max})\,Pr(H_{max}}$$

erase priors - both are $1/2$

divide by $Pr(x|H_0)$

$$\frac{1}{1 + \dfrac{P(x|H_{max})}{P(x|H_0)}}$$

K

His approach is intended to “provide a new form of default, non subjective Bayesian tests” (2013b, p. 1719)

- It has the same rejection region as a UMP error statistical test, but to bring them into line with the BF you need a smaller α level.

Johnson recommends levels more like .01 or .005.

- True, if you reach a smaller significance level, say .01 rather than .025, you may infer a larger discrepancy.

- But more will fail to make it over the hurdle: the Type II error probability increases.

So, you get a Bayes Factor and a default posterior probability. What's not to like?

We perform our two-part criticism, based on the minimal severity requirement. SIST p. 263

(S-1) holds*, but (S-2) fails; the SEV is .5.

*H_{max} : μ = $\bar{x}_{\alpha}$ accords with $\bar{x}_{\alpha}$ --they're equal

Next slide: SIST p. 263

We perform our two-part criticism, based on the minimal severity requirement. The procedure under the looking glass is: having obtained a statistically significant result, say at the 0.005 level, reject H_0 in favor of H_{max}: $\mu = \mu_{max}$. Giving priors of 0.5 to both H_0 and H_{max} you can report the posteriors. Clearly, (S-1) holds: H_{max} accords with $\bar{x}$ – it's equal to it. Our worry is with (S-2). H_0 is being rejected in favor of H_{max}, but should we infer it? The severity associated with inferring μ is as large as μ_{max} is

$$\Pr(Z < z_\alpha; \mu = \mu_{max}) = 0.5.$$

This is our benchmark for poor evidence. So (S-2) doesn't check out. You don't have to use severity, just ask: what confidence level would permit the inference $\mu \geq \mu_{max}$ (answer 0.5). Yet Johnson assigns $\Pr(H_{max}|\boldsymbol{x}) = 0.97$. H_{max} is comparatively more likely than H_0 as $\bar{x}$ moves further from 0 – but that doesn't mean we'd want to infer there's evidence for H_{max}. If we add a column to Table 4.1 for SEV($\mu \geq \mu_{max}$) it would be 0.5 all the way down!

To conclude….

Exhibit (viii). *Whether P-values exaggerate depends on philosophy.*

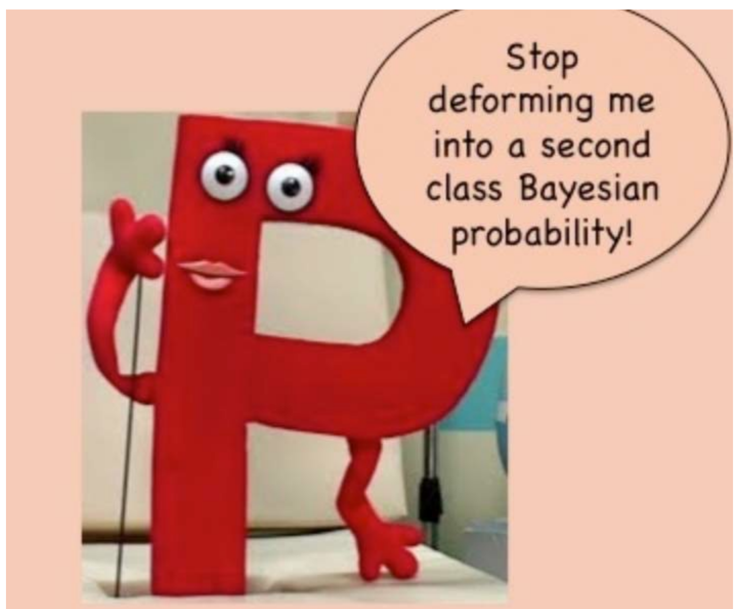
There are other interpretations of *P* values that are controversial, in that whether a categorical "No!" is warranted depends on one's philosophy of statistics and the precise meaning given to the terms involved. The disputed claims deserve recognition if one wishes to avoid such controversy. . . .

For example, it has been argued that *P* values overstate evidence against test hypotheses, based on directly comparing *P* values against certain quantities (likelihood ratios and Bayes factors) that play a central role as evidence measures in Bayesian analysis . . . Nonetheless, many other statisticians do not accept these quantities as gold standards, and instead point out that *P* values summarize crucial evidence needed to gauge the error rates of decisions based on statistical tests (even though they are far from sufficient for making those decisions). Thus, from this frequentist perspective, *P* values do not overstate evidence and may even be considered as measuring one aspect of evidence . . . with $1 - P$ measuring evidence against the model used to compute the *P* value. (p. 342)

Souvenir (R)

In Tour II you have visited the tribes who lament that P- values are sensitive to sample size (4.3), and they exaggerate the evidence against a null hypothesis (4.4, 4.5).

Stephen Senn says “reformers” should stop deforming P-values to turn them into second class Bayesian posterior probabilities (Senn 2015a). I agree.



There is an urgency here. Not only do the replacements run afoul of the minimal severity requirement, to suppose all is fixed by lowering P-values ignores the biasing selection effects at the bottom of nonreplicability.

["I]t is important to note that this high rate of nonreproducibility is not the result of scientific misconduct, publication bias, file drawer biases, or flawed statistical designs; it is simply the consequence of using evidence thresholds that do not represent sufficiently strong evidence in favor of hypothesized effects."
(Johnson 2013a, p.19316).

Let’s go back to where we left off in Excursion 6 Tour I…

What Ever Happened to Bayesian Foundations?

Excursion 6 Tour I

The Probabilities of Events (cont.)

Cox points out that even subjectivists must think their probabilities have a frequentist interpretation. Consider n events/hypotheses:

> ... all judged by You to have the same probability p and not to be strongly dependent ... It follows from the Weak Law of Large Numbers obeyed by personalistic probability that Your belief that about a proportion p of the events are true has probability close to 1.
>
> (Cox 2006a, p. 79)

[SIST, 408]

The Probabilities of Events (cont.)

To elicit Your probability for H you try to find events or hypotheses that you judge for good reason to have the same probability as H, and then find out what proportion of this set is true. This proportion would yield Your subjective probability for H.

- Here the (hypothetical or actual) urn contains hypotheses that you thus far judge to be as probable as the H of interest.
- If the proportion of hypotheses in this urn that turned out true was, say, 80%, then H would get probability 0.8.
- Rare to know truth rates Still, this would be a crazy way to actually go about evaluating evidence and hypotheses!

[SIST 408]

C.S. Peirce

[The present account] does not propose to look through all the possible universes, and say in what proportion of them a certain uniformity occurs; such a proceeding, were it possible, would be quite idle. The theory here presented only says how frequently, in this universe, the special form of induction or hypothesis would lead us right. The probability given by this theory is in every way different - in meaning, numerical value, and form - from that of those who would apply to ampliative inference the doctrine of inverse chances.

(C.S. Peirce 2.748)

6.3 Unification or Schizophrenia: Bayesian Family Feuds

Four Philosophical Positions

Berger (2006, p. 386) outlines "four philosophical positions":

1. A complete coherent objective Bayesian methodology for learning from data.
2. The best method for objectively synthesizing and communicating the uncertainties that arise in a specific scenario, but is not necessarily coherent.
3. A convention we should adopt in scenarios in which a subjective analysis is not tenable.
4. A collection of ad hoc but useful methodologies for learning from data.

Berger regards (1) as unattainable; (2) as often attainable and should be done if possible, but concedes that often the best we can hope for is (3), or maybe (4). Lindley

Questions

1. What are Ironic & Bad Faith Bayesians?

2. What are Grace & Amen Bayesians?

Ironic & Bad Faith Bayesians

One of the mysteries of modern Bayesianism is the lip service that is often paid to subjective Bayesian analysis as opposed to objective Bayesian analysis, but then the practical analysis actually uses a very adhoc version of objective Bayes, including use of constant priors, vague proper priors, choosing priors to 'span' the range of the likelihood, and choosing priors with tuning parameters that are adjusted until the answer 'looks nice.' I call such analyses pseudo-Bayes because, while they utilize Bayesian machinery, they do not carry with them any of the guarantees of good performance that come with either true subjective analysis (with a very extensive elicitation effort) or (well-studied) objective Bayesian analysis.

Grace & Amen Bayesians

Stephen Senn wrote a paper "You Might Believe You Are a Bayesian But You Are Probably Wrong."

- Researchers to have carried out a (subjective) Bayesian analysis when they have actually done something very different.
- They start and end with thanking the (subjective?) Bayesian account for housing all their uncertainties within prior probability distributions;
- in between, the analysis immediately turns to default priors, coupled with ordinary statistical modeling considerations that may well enter without being put in probabilistic form.
- "It is this sort of author who believes that he or

Grace & Amen Bayesians (cont.)

> I edit an applied statistics journal. Perhaps one quarter of the papers employs Bayes' theorem, and most of these do not begin with genuine prior information.
> (Efron 2013, p. 134)

In one example Senn cities Lambert et al. (2005, p. 2402):

[SIST, 413-414]

Grace & Amen Bayesians (cont.)

The authors "considered thirteen different Bayesian approaches to the estimation of the so-called random effects variance in meta-analysis ..." techniques fully available to the frequentist, "[n]one of the thirteen prior distributions considered can possibly reflect what the authors believe about the random effect " (pp. 62-3).

Senn says a person who takes into account the specifics of the case in their statistical modeling is "being more Bayesian in the de Finetti sense" (ibid) than the default/non-subjective Bayesian.

Focusing on how to dress the case into ill-fitting probabilistic clothing, Bayesians may miss context-dependent details because they were not framed probabilistically.

6.4 What Happened to Updating by Bayes' Rule?

If it is agreed that we have degrees of belief in any and all propositions, then it is argued that if your beliefs do not conform to the probability calculus you are being incoherent. We can grant that if we had degrees of belief, and were required to take any bets on them, that, given we prefer not to lose, we do not agree to a series of bets that ensures losing.

Howson declares it was absurd all along to consider it irrational to be induced to act irrationally. It's insisting on updating by Bayes' Rule that is irrational.

SIST 415

Counterexamples to Bayes' Rule often take the following form:

While an agent assigns probability 1 to event S at time t, i.e., $\mathrm{Pr(S)} = 1$, he also believes that at some time in the future, say t', he may assign a low probability, say 0.1, to S, i.e., $\mathrm{Pr'(S)} = 0.1$, where $\mathrm{P'}$ is the agent's belief function at later time t'.

> Let E be the assertion: $\mathrm{P'(S)} = 0.1$.
> So at time t, $\mathrm{Pr(E)} > 0$.
> But $\mathrm{Pr(S \mid E)} = 1$ since $\mathrm{P(S)} = 1$.

Now, Bayesian updating says:

> If $\mathrm{Pr(E)} > 0$, then $\mathrm{Pr'(.)} = \mathrm{Pr(. \mid E)}$.
> But at t' we have, $\mathrm{Pr'(S)} = 0.1$,

which contradicts $\mathrm{Pr'(S)} = \mathrm{Pr(S \mid Pr'(S) = 0.1)} = 1$ obtained by Bayesian updating. It is assumed, by the way, that learning E does not change any of the other degree of

Counterexample cont.

The kind of example at the heart of this version of the counterexample was given by William Talbott (1991, p. 139). In one of his examples: S is "Mayo ate spaghetti at 6 p.m., April 6, 2016". $\mathrm{Pr}(\mathrm{S}) = 1$, where Pr is my degree of belief in S now (time t), and E is " $\mathrm{Pr}'(\mathrm{S}) = r$ ", where r is the proportion of times Mayo eats spaghetti (over an appropriate time period); say $r = 0.1$. As vivid as eating spaghetti is today, April 6, 2016, as Talbott explains, I believe, rationally, that next year at this time I will have forgotten, and will (rationally) turn to the relative frequency with which I eat spaghetti to obtain Pr'. Variations on the counterexample involve current beliefs about impairment at t' through alcohol or drugs. This is temporal

Can You Change Your Bayesian Prior?

If you could really express your uncertainty as a prior distribution, then you could just as well observe data and directly write your subjective posterior distribution, and there would be no need for statistical analysis at all. (Gelman 2011, p. 77)

SIST, 417

The Bayesian Catchall

One is supposed to save some probability for a catchall hypothesis: "everything else," in case new hypotheses are introduced, which they certainly will be.

SIST 420

Normative Epistemology

Someone is bound to ask: Can a severity assessment be made to obey the probability axioms?

SIST, 423

Assignment 3 (from excursion 4 Tour I, objectivity)

(there will also be a question on power)

1. What is the argument against objectivity based on “dirty hands”? (explain as fully as you can). Should we reject or accept it? Or retain it in part? (222-5)
2. Compare: “how well have you probed” and “how strongly do/should you believe it? In explaining these, bring out some central linguistic ambiguities. 226)
3. How might you respond to the argument to “embrace your subjectivity”? Explain the argument. Do you agree with the position in this section of SIST? (228)
4. What are “objective” (default, non-subjective) Bayesians (230-1 and elsewhere in SIST)? Why are there no “uninformative” priors? Why does J. Berger argue for O-Bayesianism? Why does Kadane argue against it? (230-231)

Excursion 4 Tour I Objectivity:

6. Evaluate the argument

(i) that prior probabilities let us be explicit about bias (232-3)

(ii) that prior probabilities allow combining background information

(See also “grace and amen Bayesians” (413-415)

7. Objectivity in epistemology; 235-6. Evaluate the links between objectivity and

(i) Externalism

(ii) Diversity of knowers

8. In the Farewell Keepsake (436-) points 1-8 are often taken as central criticisms of statistical significance tests: First explain, and then critically appraise, two of them.