Probative Foundations for Bayesian Statistics

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- The Bayesian paradigm provides a good place to implement this model.
- In science, we need some measures of statistical evidence given the data to guide this process.
- Classical statistics has for some time been the main way to do this. See Fletcher, Samuel C. and Mayo-Wilson, Conor (forthcoming). "Evidence in Classical Statistics". In *Routledge Handbook of the Philosophy of Evidence* (edited by Maria Lasonen-Aarnio and Clayton Littlejohn). Routledge.

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- Compare with Roberto Trotta (2008) "Bayes in the sky: Bayesian Inference and Model Selection in Cosmology", Contemporary Physics, 49:2, 71-104

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Suppose $\frac{\pi(M_0)}{\pi(M_1)}$ measures our **prior odds** for two models M_0 and M_1. Then we expect the **posterior odds** $\frac{p(M_0|X)}{p(M_1|X)}$ to be given by:

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- Bayes Factor B_{01} is the updating factor U that quantifies the **relative predictive accuracy** of our models (Rouder and Morey 2019).

$$B_{01} = \frac{f(X|M_0)}{f(X|M_1)}$$

Background

- Challenges to this proposal

  Either your methodology picks up on influences on error probing capacities of methods or it does not. If it does, then you are in sync with the minimal severity requirement. We may compare our different ways of satisfying it. If it does not, then we've hit a crucial nerve. If you care, but your method fails to reflect that concern, then a supplement is in order. Opposition in methodology of statistics is fighting over trifles if it papers over this crucial point. If there is to be a meaningful "reconciliation," it will have to be here. Mayo (2018, 270)

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- What does "practically guaranteed" mean?
- What is a "capability of finding flaws"?

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- Morey, R. (2022) "Bayes factors, p-values, and the replication crisis" Workshop on 22nd of September 2022 of *Statistics Wars and Their Casualities* organized by Deborah Mayo. https://cardiffunipsychstats.co.uk/statswars2022/

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- Grant Mayo the point that Bayes Factors can't help with model validation but argue that Bayes Factors can meet the minimum requirement of evidence for model comparison.
- The key to my proposal is to argue that Mayo is wrong to criticize Bayes Factors because her criticism assumes that Bayes Factors are used in isolation. A subjective Bayesian will **consider** error by modeling the variability due to chance in their priors. Call this the **Full Package View of Bayes Factors** (Full Package View, in short).

Thesis

I will argue that the Full Package View is a Small Sample Optimal Test of Hypotheses.

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- Call the problem raised by question 2 the **Model Comparison** problem.

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  - **Marginal p-values** (Box, 1980)
  - **Predictive p-values** (Rubin, 1984; Gelman et al. 2004)

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  - Clarifies the issues. The challenge (raised by Mayo) is for Bayesian statistics is to develop an analogous small sample optimal test theory using Bayes Factors.

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- Suppose that we only observe one value $\mathrm{Y} = \mathrm{y}$, where $y \in \{1, 2, 3, 4, 5, 6\}$ and $f(Y; \theta)$ is the probability mass function for $Y = y$. Now consider the following sampling models of our data Y and the likelihood ratio (LR) in each case.

y	1	2	3	4	5	6
$f(Y = y\,;\,\theta \in \Theta_0)$	0.89	0.07	0.01	0.01	0.01	0.01
$f(Y = y\,;\,\theta \in \Theta_1)$	0.01	0.01	0.01	0.05	0.28	0.64
$\mathrm{LR} = \frac{f(Y=y\,;\,\theta\in\Theta_1)}{f(Y=y\,;\,\theta\in\Theta_0)}$	0.0112	0.143	1	5	28	64

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- Let y_0 denote the observed value of y. Given a sampling model, define the p-**value** as:

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- Consider our sampling models. What are the associated p-values of these sampling models for $y_0 = 1$, $y_0 = 3$, $y_0 = 6$?

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- It has nothing to do with "extremeness" or "weirdness" of our data. We can ask for the p-value of $Y = y_0 = 1$, after all in the $f(Y = y\,;\,\theta \in \Theta_0)$ case. Although a **low** p-value may invalidate our model.

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- *p*-values are not conditional probabilities.
- *p*-values are the right tools for **Model Validation**. More of this later.

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- What did they propose instead?

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- On the Choice View, a statistical test is a decision problem (something which Wald later realized).

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$f(Y = y\,;\, \theta \in \Theta_1)$	0.01	0.01	0.01	0.05	0.28	0.64
$\text{LR} = \frac{f(Y=y\,;\,\theta\in\Theta_1)}{f(Y=y\,;\,\theta\in\Theta_0)}$	0.0112	0.143	1	5	28	64

- Here I assumed that $\theta \in \Theta_0$ and $\theta \in \Theta_1$ are simple, say $\theta = \theta_0$ and $\theta = \theta_1$ to simplify computation of LR and in order to appeal to the Neyman-Pearson Lemma.

More Historically (and Mathematically) Accurate

- What is the optimal rejection regions/tests on the Choice View? Suppose I observe $y_0 = 3$? What probability, under $\theta \in \Theta_0$, would I consider so low that it invalidates my model?

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- The point is that the optimal choice is underdetermined unless I specify an additional constraint. This was Gosset's (aka "Student") idea that led Neyman and Pearson to their theory of hypothesis testing. The view I am calling Small Sample Optimal Test Theory

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- "That the original hypothesis is not true" is a distraction. We might just substitute "the alternative" here without loss of meaning.

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- In Neyman and Pearson (1928) and Neyman and Pearson (1933) we get what are called today **Neyman-Pearson Likelihood Ratio Tests**. The general (decision theoretic) theory was developed by Wald (1945).

Uniformly Most Powerful Tests

- The additional constraint is defined in terms of what is called the power function. Let $\eta : \Omega \times \Theta \to \mathbb{R}$. Where $R \subset \Omega$, which is the sample space for Y, define the **power function** as:

$$\eta(R, \theta) = P_{\theta \in \Theta}(Y \in R)$$

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Compare Cohen (1988, 4).

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- Define the **size** of the test $\alpha = \sup \eta \restriction \Omega \times \Theta_0$

$$\alpha = \sup_{\theta \in \Theta_0} P(Y \in R)$$

  Why “supremum”? We want a unique upper bound.

Uniformly Most Powerful Tests

- What is α and β for $R = \{3, 4\}$, $R = \{3, 5\}$, $R = \{4, 5\}$, $R = \{5, 6\}$, $R = \{4, 5, 6\}$?

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- The UMP Test is also where LR is highest. See next slide.

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- Her account is **not** about long-run error control, which is what many people have assumed her account is about.
- Mayo insists that it is about what can be **learned** from an error-statistical approach to experimental inquiry. The error-probabilities on her account and the severity function measure of evidence allow us to **learn** that certain errors (e.g., mistaking an artifact from a real effect) are absent with certain probabilities. See Mayo (1996, 94 - 95).

Severe Testing: So, what can be learned?

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- Let $\theta \in \Theta_0$ denote the null model, $\theta \in \Theta_1$ denote the alternative model and Ω denote the sample space for Y. For a given test or rejection region $R \subset \Omega$, Mayo's **Severity Function** is the function $\mathrm{SEV} : \Omega \times \Theta \to \mathbb{R}$ is given by:

$$SEV(\theta, D) = \begin{cases} P_{\theta \in \Theta_0}(Y \leq D) & \text{if } D \in R \\ P_{\theta \in \Theta_1}(Y \geq D) & \text{if } D \notin R \end{cases}$$

  where D is a function of the *observed* y_0.

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  where D is a function of the *observed* y_0.

- Compare with the definition of the power function. Where $R \subset \Omega$, we defined the **power of a test** as the function $\beta = \eta \upharpoonright \Omega \times \Theta_1$ given by:

$$P_{\theta \in \Theta_1}(Y \in R)$$

Severe Testing: So, what do we learn with SEV?

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- What is the severity of the Test $R = \{5, 6\}$ and Test $R = \{3, 4\}$ with $D = 1$. Now compare these tests with $D = 3$, $D = 5$ and $D = 6$. See next slide.

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Interpretation: If $D \in R$ and $SEV(\theta, D)$ is **high**, then we learn that $\theta \in \Theta_1$ passed a highly severe test. If $D \notin R$ and $SEV(\theta, D)$ is **high**, then we learn that $\theta \in \Theta_0$ passed a highly severe test.

	$R = \{5, 6\}$	
D = 1	SEV(θ, 1) = 1	$\theta \in \Theta_0$ passed
D = 3	SEV(θ, 3) = 0.98	$\theta \in \Theta_0$ passed
D = 5	SEV(θ, 5) = 0.99	$\theta \in \Theta_1$ passed
D = 6	SEV(θ, 6) = 1	$\theta \in \Theta_1$ passed

	$R = \{3, 4\}$	
D = 1	SEV(θ, 1) = 1	$\theta \in \Theta_0$ passed
D = 2	SEV(θ, 2) = 0.99	$\theta \in \Theta_0$ passed
D = 3	SEV(θ, 3) = 0.97	$\theta \in \Theta_1$ passed
D = 4	SEV(θ, 4) = 0.98	$\theta \in \Theta_1$ passed

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- For some D and Test R, we can either pass $\theta \in \Theta_0$ or $\theta \in \Theta_1$.
- A likelihood ratio for $D = 3$ is neutral between the two models. But with SEV and a given choice of R, we can say whether or not either $\theta \in \Theta_0$ or $\theta \in \Theta_1$ passed a highly severe test.

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- A likelihood ratio for $D = 3$ is neutral between the two models. But with SEV and a given choice of R, we can say whether or not either $\theta \in \Theta_0$ or $\theta \in \Theta_1$ passed a highly severe test.

  It's the idea of viewing statistical inference as severe testing that invites a non-trivial difference with probabilism. Mayo (2018, 346)

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- The error is due to **sampling variability**. A random variable has intrinsic variability quantified by its distribution function. (Call this the variability view of error.)
- On the variability view of error, we are not **controlling** errors, we are **considering** errors.

Truth and Error Rates

Consider the famous passage from Pearson and Neyman (1930, 106) "On the Problem of Two Samples."

> *But if we accept the criterion suggested by the method of likelihood it is still necessary to determine its sampling distribution in order to control the error involved in rejecting a true hypothesis, because a knowledge of λ alone is not adequate to insure control of this error.*

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- Most people stop here and say, "Aha! See! It's about controlling error."
- Not so fast!

Truth and Error Rates

In the same paragraph they go on to say:

> *We cannot for example say in general that if $\lambda \leq \lambda_0 = 0.01$, we should be justified in rejecting the hypothesis. In order to fix a limit between "small" and "large" values of λ we must know how often such values appear when we deal with a true hypothesis. That is to say we must have knowledge of P_{λ_0}, the chance of obtaining $\lambda \leq \lambda_0$ in the case where the hypothesis tested is true. The frequency distribution of λ differs according to the size of samples and the nature of the hypothesis tested, and it may well happen that the modal value of λ is in the neighbourhood of zero.*

Here they are clearly talking about **considering error** due to **sampling variability**.

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Concerning the notion of "truth" and "Type I and Type II errors", they had said this in the same paper.

> *We have discussed elsewhere (Neyman and Pearson (1928a, b); Neyman (1929a)) certain principles regarding the testing of hypotheses, which we believe to be intuitively sound. They are not, strictly speaking, mathematical results and may be rejected by those who do not believe in them.*

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- So we may drop talk of "truth concerning statistical hypotheses", even the talk of "Type I and Type II errors".
- But we can preserve the "intuitively sound" choice of α. While this choice is seemingly "arbitrary", it can be justified from a decision-theoretic point of view as the risk given a 0-1 loss function.

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- HKH are right in the problems they raise: specify your subjective priors ("break the Bayesian egg"), calibrate by providing subjective priors on effect sizes.
- HKH are wrong to "elaborate how frequency calculations can be used to provide an interpretation of the size of the Bayes factor" because they focus on **controlling error**.

Truth and Error Rates

- Subjective priors based on effect sizes is a good idea if you're **considering errors** because by definition, you are considering the variability. Compare Wetzels, R., Matzke, D., Lee, M. D., Rouder, J. N., Iver- son, G. J., & (2011). "Statistical evidence in experimental psychology: An empirical comparison using 855 t tests" Perspectives on Psychological Science, 6, 291–298

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- Is this enough calibration? This is what HKH were arguing is not enough.

Towards a Small Sample Optimal Test Theory for Bayesian Statistics

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  We note that, from a Bayesian perspective, the effect size can naturally be conceived as (a summary statistic of) the posterior distribution of a parameter representing the effect, under an uninformative prior distribution. In this sense, a standard Bayesian combination of parameter estimation and model selection could encompass all of the useful measures of evidence we observed. Wetzels et. al (2011, 296)

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- Why “uninformative?” Break the Bayesian egg.

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- Assuming sensitivity and specificity is a good thing (which I think it is), the challenge, again, is **to formulate a small sample optimal test theory for Bayesian statistics**.

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- So my proposal is that we can adjust these within a unified framework of both Bayes Factors **and** priors by taking into account $P(\text{have disease})$ and $P(\text{don't have disease})$ to get the right posterior odds, $P(\text{have disease}\,|+)$ and $P(\text{don't have the disease}\,|-)$

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- $P(+|\,\text{have disease})$ (Sensitivity) and $P(-|\,\text{don't have the disease})$ (Specificity)
- But the Bayes Factor $\frac{f(X|M_0)}{f(X|M_1)}$ has this natural interpretation in terms of sensitivity and specificity.
- So my proposal is that we can adjust these within a unified framework of both Bayes Factors **and** priors by taking into account $P(\text{have disease})$ and $P(\text{don't have disease})$ to get the right posterior odds, $P(\text{have disease}\,|+)$ and $P(\text{don't have the disease}\,|-)$
- Just as one uses the sampling distribution to consider variability one can *use the prior* to consider error.

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- This is our sample small optimal test theory. The details will be added.

THANK YOU