"Weight of Evidence, Evidential Completeness and Accuracy"

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1 Balance vs. weight

Suppose we want to represent our uncertainty about a proposition in terms of a single probability that we assign to it. It is not too difficult to inspire the intuition that this representation does not capture an important dimension of how our uncertainty connects with the evidence we have or have not obtained. In a 1872 manuscript of *The Fixation of Belief* (W3 295) C. S. Peirce gives an example meant to do exactly that.

> When we have drawn a thousand times, if about half have been white, we have great confidence in this result. We now feel pretty sure that, if we were to make a large number of bets upon the color of single beans drawn from the bag, we could approximately insure ourselves in the long run by betting each time upon the white, a confidence which would be entirely wanting if, instead of sampling the bag by 1000 drawings, we had done so by only two.

The objection is not too complicated. Your best estimate of the probability of $W =$'the next bean will be white' is .5 if half of the beans you have drawn randomly so far have been white, no matter whether you have drawn a thousand or only two of them. But this means that expressing your uncertainty about W by locutions such as "my confidence in W is .5' does not capture this intuitively important distinction.

Similar remarks can be found in Peirce's 1878 *Probability of Induction*. There, he also proposes to represent uncertainty by at least two numbers, the first depending on the inferred probability, and the second measuring the amount of knowledge obtained; as the latter, Peirce proposed to use some dispersion-related measure of error (but then suggested that an error of that estimate should also be estimated and so, so that ideally more numbers representing errors would be needed).

Peirce himself did not call this the weight of evidence (and in fact, used the phrase rather to refer to the balance of evidence, W3 294) [CITE KASSER 2015]. However, his criticism of such an oversimplified representation of uncertainty anticipated what came to be called weight of evidence by Keynes in his 1921 *A Treatise on Probability*:

> As the relevant evidence at our disposal increases, the magnitude of the probability of the argument may either increase or decrease, according as the new knowledge strengthens the unfavourable or the favourable evidence; but something seems to have increased in either case,—we have a more substantial basis upon which to rest our conclusion. I express this by saying that an accession of new evidence increases the weight of an argument. New evidence will sometimes decrease the probability of an argument but it will always increase its 'weight.' (p. 71)

The key point is the same [CITE LEVI 2001]: the balance of probability alone cannot characterize all important aspects of evidential appraisal. Keynes also considered measuring weight of evidence in terms of the variance of the posterior distribution of a certain parameter, but was quite attached to the idea that weight should increase with new information, even if the dispersion increase with new evidence [TP 80-82], and so he proposed only a very rough sketch of a positive sketch. Moreover, as he was uncertain how a measure of weight should be incorporated in further decision-making, the was skeptical about the practical significance of the notion. [TP 83]

But what is this positive sketch? On one hand, Keynes [TP 58-59] connects the notion of weight with

relevance. Call evidence E relevant to X given K just in case $\mathsf{Pr}(X|K \wedge E) \neq \mathsf{Pr}(X|K)$.[1] One postulate than can be found in the *Treatise* [TP 84] is:[2]

(Monotonicity) If E is relevant to X given K, where K is background knowledge, $V(X|K \wedge E) > V(X|K)$, where V is the weight of evidence.

[RUNDE 1990, 280] suggests that Keynes at some point calls weight the completeness of information. This however, is a bit hasty, as Keynes only says that *the degree of completeness of the information on which a probability is based does seem to be relevant, as well as the actual magnitude of the probabilitiy, in making practical decisions*. As later on we will argue that it is actually useful to distinguish evidential weight (how much evidence do we have?) and evidential completeness (do we have all the evidence that we would expect in a given case?), we rather prefer to extract a more modest postulate:

(Completeness) If E_1 and E_2 are relevant items of evidence, and E_2 is (in a sense to be discussed) more complete than E_1, $V(X|K \wedge E_2) > V(X|K \wedge E_1)$.

If we conceptualize E_2 being complete and E_1 being incomplete as E_2 being a maximal relevant conjunction of relevant claims one of which is E_1, (Completeness) follows from (Monotonicity).

Similar requirements seem to be inspired by the urn example. We put them in two forms, a weaker and a stronger one.

(Weak increase) In cases analogous to the urn example, the weight obtained by a larger sample is higher, if the frequencies in the samples remain the same.

(Strong increase) In cases analogous to the urn example, the weight obtained by a larger sample is higher.

Now, some requirements on how weight of evidence is related to the balance of probability. For one thing, Keynes insists that new (relevant) evidence might decrease probability but will always increase weight [TP 77]. Since (Monotonicity) already captures the idea that weight will always increase, here we extract the other part of the claim:

(Possible decrease) It is possible that $V(X|K \wedge E) > V(X|K)$ while $\mathsf{P}(X|K \wedge E) < \mathsf{P}(X|K)$.

Clearly, Keynes also endorsed the following two requirements of a very similar form:

(Possible increase) It is possible that $V(X|K \wedge E) > V(X|K)$ while $\mathsf{P}(X|K \wedge E) > \mathsf{P}(X|K)$.

(Possibly no change) It is possible that $V(X|K \wedge E) > V(X|K)$ while $\mathsf{P}(X|K \wedge E) = \mathsf{P}(X|K)$.

Now, back to the urn example. You might think the actually frequency observed should contribute more to the balance, not to the weight, at least in the sense that with the same number of observations, more extreme frequencies should not have lower weight:

(Frequency monotonicity) For a fixed number of observations in a binomial experiment, for two observed frequencies f_1 and f_2, if f_2 is closer to .5 than f_1, the weight of observing f_1 is not less than that of observing f_2.

Interestingly, Keynes for quite a few years did not attempt to provide anything close to a formal explication of the notion, and did not spend too much time studying the issue. Various reasons for this has been proposed the literature, a prominent one [CITE FEDUZI 2010] being that from the decision-theoretic perspective no clear stopping rule emerged as to whether the evidence is weighty enough to make a decision. Later on we will see a sort of revival—some ideas later developed by Keynes has been used to explicate the notion of weight formally, and we will take a closer look at this proposal.

Should I talk about other theories here, or should we leave it as is without getting into interpretative details.

REF section

2 Examples and informal desiderata

- Go over Nance in particular, some other sources?
- first check for completeness, then evaluate
- what do you mean: are there items of relevant evidence that you could reasonably obtain

[1] Keynes also uses a slightly more convoluted notion of relevance to avoid equally strong items of opposite evidence turning out to be irrelevant (this objection has also been brought up by [COHEN 1986 TWELVE]). The more complex version is that a proposition E_1 is relevant to X given K just in case it entails a proposition E_2 such that $\mathsf{P}(X|K \wedge E_2) \neq \mathsf{Pr}(X|K)$. [COHEN 1986 TWELVE] complaints that this still runs into difficulties. Ignore K, take an irrelevant proposition Z. It entails $Z \vee X$ and $\mathsf{P}(Z \vee X|X \wedge E) = 1$. Now, by Bayes' theorem we have $\mathsf{P}(X|E \wedge (Z \vee X)) = \frac{\mathsf{P}(X|E) \times \mathsf{P}(Z \vee X|X \wedge E)}{\mathsf{P}(Z \vee X|E)} = \frac{\mathsf{P}(X|E)}{\mathsf{P}(Z \vee X|E)}$. If the denominator differs from 1, the result differs from the numerator. We will ignore such difficulties, as they are not of key importance for the development of this chapter.

[2] RUNDE 1990 283 suggests Keynes allows for weight of evidence to decrease when new evidence increases the range of alternatives, but this is based on Keynes' claim that weight is increased when the number of alternatives is reduced, and Keynes does not directly say anything about the possibility of an increase of the number of alternatives.

- destroyed?

2.1 Monotonicity of weight

Before we move on, let us ponder whether (Monotonicity) is actually desirable. Is it always the case, as some formulations from Keynes would suggest, that any item of relevant of evidence, when obtained, leads to a higher weight of evidence?

Here are two examples from [WEATHERSON 2002], one qualitative, one quantitative. First. you are playing poker and wonder if one of the other players, Lydia, has a straight flush (five cards of sequential rank in the same suite). There are 40 possible straight flush hands out of 2,958,960 possible hands, so you estimate the probability of this event to be $40/2{,}958{,}960$. But then you look at her facial expressions, listen to her tone of voice, past bluffing behavior, and this makes you more confused about the issue. It seems, obtaining this additional information diluted your original calculated first stab at the problem. Second. You are drawing from an urn with 10 blue and 90 black lottery tickets. Your initial assessment of the probability of drawing a blue ticket is .1. Then, you learn that the proportion of the tickets at the top is somewhere between .2 and 1. You acquired new evidence, but your evidence became imprecise. In both cases, it seems intuitive that the weight of evidence should decrease, as the evidence becomes less telling.

CITE B. Weatherson. Keynes, uncertainty and interest rates. Cambridge Journal of Economics, 26 (1): 47-62, 2002.]

3 Hamer's weight of evidence

4 Good's weigh of evidence and the information value

One notion in the vicinity also called *weight of evidence* has been introduced by Good [CITE PROBABILITY AND THE WEIGHING OF EVIDENCE 1950]. Let $W(H : E)$ be the Good's weigh of evidence in favor of H provided by E (if we want to explicitly conditionalize on some background knowledge K, we write $W(H : E|K)$). One assumption about W taken by Good is as follows:

(Function) "It is natural to assume that $W(H : E)$ is some function of $\mathsf{P}(E|H)$ and of $\mathsf{P}(E|\neg H)$, say $f[\mathsf{P}(E|H), \mathsf{P}(E|\neg H)]$. I cannot see how anything can be relevant to the weight of evidence other than the probability of the evidence given guilt and the probability given innocence." [cite Good 1985 p 250]

pays attention to different values of different items of evidence, which is better than just counting or supersets

The other two are:

(Independence) $\mathsf{P}(H|E)$ should depend only on the weight of evidence and on the prior: $\mathsf{P}(H|E) = g[W(H : e), \mathsf{P}(H)]$.

(Additivity) $W(H : E_1 \wedge E_2) = W(H : E_1) + W(H : E_2|E_1)$

The three conditions can be simultaneously satisfied by only one function (up to a constant factor), which leads to Good's definition of weight of evidence:[3]

$$W(H : E) = \log \frac{\mathsf{P}(E|H)}{\mathsf{P}(E|\neg H)}$$

The natural question that arises is the extent to which Good's weight satisfies the desiderata related to Keynes' notion of weight. First, let us think about weight increase with sample size. If in an experiment the observations $E_1, \ldots, E_K$ are independent given H and independent given $\neg H$, the resulting joint likelihood is the result of the multiplication of the individual likelihoods, and so the resulting joint weight is the result of adding the individual weights.

For example, suppose a die is selected at random from a hat containing nine fair dice and one loaded die with the chance $1/3$ of obtaining a six. The initial uniform distribution gives you weight of evidence for the die being loaded of $log_{10}(.1)$, that is -1 (Good and Turing would say, it is -10 db). Now, every time you toss it and obtain a six, you gain $log_{10}(\frac{1/3}{1/6}) = log_{10}(2)$, that is 0.30103, and every time you toss it and obtain something else, the weight changes by $log_{10}(\frac{2/3}{5/6}) = log_{10}(.8)$, that is -0.09691. Let us inspect the weights in db (that is, multiplied by 10) for all possible outcomes of up to 20 tosses (Figure 1).

[3] To be fair, logarithms of the ratio of posterior odds to prior odds have been used Jeffrey in 1936, [CITE] and the use of logarithm to ensure additivity has been suggested by Turing [CITE 1950 o 63]. Good's measure differs from Jeffrey's by taking the ratio

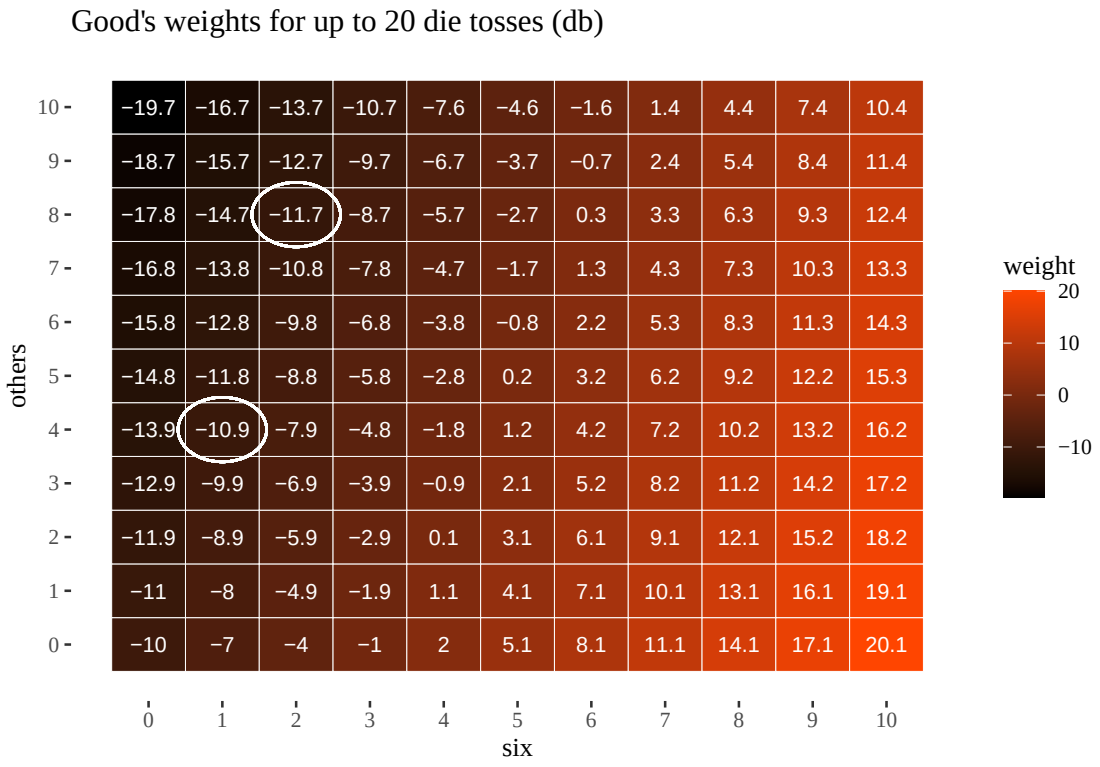

Figure 1: Good's weights in dbs, rounded, for all possible outcomes of up to 20 tosses of a die randomly selected from 10 dice nine of which were fair, and one is 1/3 loaded towards six. H ='the die is loaded'.

Two facts are notable. (1) Weight can drop with sample size: for instance the weight for 4 others and 5 sixes is 1.2db, and it is .2db for 5 others and 5 sixes. (2) Weight can drop while the sample size increases even if the proportion of sixes remains the same. For instance, if none of the observations are sixes, the weights go from -10 to -19.7 as the sample size goes from 0 to 10. Less trivially, the observation of one six in five leads to weight of -10.9, while the observation of two sixes in ten tosses leads to weight -11.7. That is, (Monotonicity), (Completeness), (Weak increase) and (Strong increase) all fail for Good's measure.

Moreover, there is a conceptual difficulty in the neighborhood. Suppose you are trying to ascertain the bias θ of a coin, but you do not restrict yourself to two hypotheses as in the dice example, but rather initially take any bias to be equally likely. For each particular hypothesis $\theta = x$ and any set of observations E you can use the binomial distribution to calculate $\mathrm{P}(E|\theta = x)$. But to deploy Good's definition, you also need $\mathrm{P}(E|\theta \neq x)$, which is less trivial, as now you have to integrate to calculate the expected probability of the evidence given an infinite array of possible values of y. Suppose you have no problem calculating such items. Now imagine you observe 10 heads in 20 tosses. The question 'how weighty is the evidence' makes no sense here, as Good's weight needs a hypothesis (and its negation) to be plugged in. For this reason, in such a situation, we can at best talk about a continuum of Good's weights, one for each particular value of θ.

- compare to pointwise mutual information
- evaluate in light of the desiderata

of likelihoods rather than odds. In fact, the former ratio is identical to $O(H|E)/O(H)$, the ratio of conditional odds of H to the prior odds of H.

5 Weight and completeness

A question similar to "how weighty is the evidence" is "how complete is it"? These are conceptually different: the former asks about how much information pertinent to a given hypotheses the evidence provides, or, about the amount of evidence relevant to that hypothesis, the latter seems to suggest a comparison to some ideal list of what such items would be needed for the evidence to be complete. While we think that these notions, albeit related, should be clearly distinguished, the distinction has not always been made clearly in the literature, starting with Keynes himself, who suggested that in their evaluation of the evidence an agent should consider "the degree of completeness of the information upon which a probability is based." [TP p. 345]

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This picture of ideal-list-of-evidence-relative notion of weight has been explored by [CITE FEDUZI 2010]. Let us first present the view following [CITE FEDUZI 343], Ω stands for the set of all items of possible evidence relevant for estimating the probability of the hypothesis H. Let K be the agent's knowledge, the set of items of evidence already obtained by the agent, $K \subseteq \Omega$. Then her relevant ignorance is $I = \Omega \setminus K$.

CITE NANCE HERE?

Then, Feduzi, following [CITE RUNDE 1990], proposes to define the weight of information E provides about H, $V(H/E)$ as follows:

$$V(H/E) = \frac{K}{K+I}. \tag{Vdiv}$$

While literally it does not make sense to divide sets by sets, we might charitably interpreting Feduzi as using Ω, K and I the symbols ambiguously, standing for both the sets of items of evidence, and the amount of relevant information that the sets contain. The obvious difficulty is that it is not a successful explication (at least not yet), as we are not told how to get K and $K+I$ as numerical values to be used in the division. But however we get them, let us see whether (Vdiv) can result in any insights.

First, one advantage of the completeness approach is that the resolution of the stopping problem is more or less automatic: the agent should make the decision if the evidence is complete, and should collect more evidence if it is not. Later on, when discussing Nance's approach to the notion, we will see a complication: obtaining further evidence might be practically unfeasible, and so it makes sense to distinguish ideal completeness from reasonable completeness and base the practical stopping rule on the former. For now, we put this complication aside.

Second, it might be the case that obtaining further evidence while providing more information results in the decrease of weight. Here is an example illustrating this due to Feduzi [CITE 345]. Joan in her research tries to establish who is the most quoted author in the literature on decision theory under ambiguity. Ω is the set of all n papers (though of as items of evidence $E_1, \ldots, E_n$). K_0 contains the m papers that Joan inspected so far ($E_1, \ldots, E_m$, $m < n$. I_0 is the set of papers she did not look at yet, $\Omega = K_0 \cup I_0$. However, Joan is aware only of a part of Ω, the papers in the field she believes exist, S. Thus, her objective ignorance, I_0, and her subjective ignorance, $I_S = S - K$, diverge, as she underestimates the amount of papers that she has not yet encountered. Joan's assessment of weight is going to be $K/K+I_s$. Say Joan formulates a hypothesis, H: "Ellsberg is the most highly cited author in the ambiguity literature" and that she is quite confident that the papers she had not looked at yet would not significantly affect the probability of H. She thinks she has read enough, say $\mathsf{P}(H|K) = .7$ and $V(H/K) = .8$. Then, she looks at another paper, somewhat increasing K, but that paper contains reference to many papers she has not heard of in journals that she has not heard of, thus increasing her estimation of S quite a lot— the ultimate impact of the new evidence is a drop in weight as the denominator in (Vdiv) will grow much more than the numerator. Thus, (Monotonicity) can fail on this approach.

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However, even putting the conceptual difference between weight and completeness that we have already mentioned aside, there are concerns about using degrees of completeness as our explication of the notion of weight of evidence.

To start with, we have not really explicated the notion of the amount of evidence employed in (Vdiv). Sure, we could simply count propositions. One simple strategy, to be used if we do not want to use (Vdiv) would be to simply count the relevant propositions included in the evidence—this would validate (Monotonicity) . Another strategy along these lines would be to assign sizes to sets of propositions and use these as numbers in (Vdiv), invalidating (Monotonicity) in the process. Either way, the strategy is not viable, as it is too syntax-sensitive. Different propositions, intuitively, can contain hugely different amount of nevertheless relevant information , and the individuation of propositions is too arbitrary a

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is this trivial or do we need an example?

matter to take such an approach seriously. On one hand, without some measure of assigning numerical values to sets of evidence, we have no way to deploy (Vdiv). On the other hand, if we could meaningfully assign numbers expressing the "amount of evidence" prior to any application of (Vdiv), there are no clear reasons why we should take these number to express weights of evidence, especially given the second concern with the completeness approach.

So the second difficulty is that on this approach the weight of evidence becomes very sensitive not only to what the actual evidence is, but also to what an ideal evidence in a given case should be. unless as clear and epistemologically principled guidance as to how to formulate such ideal lists is available, this seems to open a gate to arbitrariness. Change of awareness of one's own ignorance, without any major chance to the actual evidence obtained, might lead to overconfidence or under-confidence in one's judgment. Moreover, it is not clear how disagreement about weight arising between agents not due to evidential differences, but rather due to differences in their list of ideal items of evidence should be adjudicated.

6 Skyrms and resilience?

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- relation to law Davidson Pargetter 1986, perhaps Nance, who else?

7 Evidential probability and weight

[PEDDEN 2018] follows a suggestion from [KYBURG 1961] He proposed using the degree of imprecision of the intervals in his probability system called Evidential Probability (EP). The key idea in EP is that evidential probabilities should be imprecise, and so accordingly an evidential probability function EP is of the form $\mathsf{EP}(H|E \wedge K) = [x, y]$, where the right-hand is the closed interval expressing the objective degree of support that $E \wedge K$ provide for H.

REF

REF Kyburg. Probability and the Logic of Rational Belief. Wesleyan University Press, Mid- dletown Connecticut, 1961 and H. E. Kyburg and C. M. Teng. Uncertain Inference. Cambridge University Press, Cambridge, 2001.

How is this interval to be determined, though? Kyburg's proposal is the following, if the hypothesis is about a single object o and a predicate P. For the reference classes to which o is known to belong and for which K contains frequency information (possibly imprecise, in the interval form) for objects with P, enumerate the corresponding frequency statements, $r_1, \ldots, r_n$. Now, you are facing a reference class problem. Apply sequentially the following rules:

(Sharpening by richness) If r_j conflicts with r_i and r_i has been obtained from a marginal distribution while r_j from a full joint distribution, ignore r_i.

As the formulation might be somewhat cryptic, let us illustrate the recommendation with an example.

make sure these examples are better made sense of the HOP way!

(Richness example) suppose you are drawing a card from one of two decks of cards, $H :=$ 'you will draw the Ace of Spades'. You know that Deck 1 is a regular deck, and Deck 2 is a regular Deck with the Ace of Spades removed. First you toss a fair die and use Deck 1 if the die lands on 1 or 2, and use Deck 2 otherwise. You have at least two frequencies to consider:

- The frequency of Aces of Spades in the total number o cards ($1/103$), which is your marginal-distribution-based-probability.
- The one obtained by using the information about the die, and about the frequencies in the decks. There is probability $1/3$ of using Deck 1 which is the only deck containing the card, in which the probability of drawing it is $1/52$, so the probability to be used is $1/3 1/52 = 1/156$.

One can easily observe that $1/103$ simply is not the probability of drawing the Ace of Spades in the setup. After all, we are not drawing a random card from the joint decks, but have to factor in the uneven probabilities of the decks being chosen, and once we do so, the correct probability is $1/156$. The second strategy is this:

(Sharpening by specificity) If among the remaining intervals r_j conflicts with r_i and r_j is a proper subset of r_i, choose r_j over r_i.

This mirrors the idea that one should use more specific information. The third rule is:

(Sharpening by precision) If there is a single interval that is a proper sub-interval of every other interval, this is the evidential probability. Otherwise, the evidential probability is the shortest possible cover of these intervals.

With this system in the background, [PEDDEN 2018 681] proposes the following definition of the weight of the argument for H given E and K, where $\mathsf{EP}(H|E \wedge K) = [x, y]$:

$$\mathsf{WK}(\mathsf{H}|\mathsf{E} \wedge \mathsf{K}) = 1 - (y - x) \tag{WK}$$

That is, the weight of the evidence is the spread of the evidential probability, transformed to scale between 0 and 1, reaching 1 when the spread is 0 and 0 when the spread is 1.

How is this approach to be applied to examples such as the one by C. S. Peirce (recall: drawing balls with replacement from an urn, with observed frequency of white balls .5, in one scenario the sample size is 1000 in another it is 2)?

[PEDDEN 2018 686] proposes the following analysis, of an example analogous to that by Peirce. You are drawing from an urn full of black and red beans (the proportion is unknown). First, abbreviations:

H $49.5 - 50.5\%$ of the beans are red.
E_1 2 sampled beans are red.
E_2 3000 sample beans are red.

Further, imagine you have enough information to calculate that between 2% and 100% of two-fold samples of any large finite population will be matching samples, that is they will match the population with a margin of error of 1%. Then $\mathsf{EP}(H|E_1 \wedge K) = [.02, 1]$. Similarly, Pedden invites us to suppose that we can calculate the relative frequency of 3000-fold samples that match any large finite population within a margin of error of 1% to somewhere between 72.665% and 100%, so accordingly $\mathsf{EP}(H|E_2 \wedge K) = [.72665, 1]$. Then, the corresponding values of WK are .02 and .72665 (this is because in both cases $y = 1$ and $1 - (1 - x) = x$).

What are we to make of this? Are imprecise probabilities promising when it comes to the explication of weight of evidence? Some progress has been made, but note the following limitations.

- The edges of the intervals are what contributes to WK. They are highly sensitive to the choice of the margin of error, but what margin of error to choose and why remains a mystery, and what margin of error has been chosen does not function anywhere in the EP representation of uncertainty.
- Relatedly, it may easily happen that for two different distributions the 1% intervals will be identical while the 78%$ intervals will not. Such differences will obviously not be captured by the 1% margin of error intervals.
- The calculations of such intervals might be easy for simple combinatorial cases, but it is far from obvious how similar intervals are to be obtained for more complicated real-life cases. Emphatically, classical statistical confidence intervals are not ranges within which the true parameter lies with a certain probability (and if you interpret confidence intervals this way, you behave as if you were running a Bayesian reasoning with a uniform prior, which is often unjustified and prone to over-fitting).

Before we abandon the idea, though, let us know that over the last 30 years we have observed a revival of imprecise probabilities, and so it is only fair that we should take its most recent versions for a ride. Hence our next interest: imprecise probabilism, its motivations, the difficulties it runs into.

8 Imprecise probabilities and weight

The point of departure for imprecise probabilism (IP) is the precise probabilism (PP), which we are already familiar with. On the latter view, a rational agent's uncertainty is to be represented as a single probability measure. **Imprecise probabilism**, in contrast with PP, holds that an agent's credal stance towards a hypothesis H is to be represented by means of a set of probability measures, called a representor, $\mathbb{P}$, rather than a single measure P. The idea is that the representor should include all and only those probability measures which are compatible with this evidence. For instance, if an agent knows that the coin is fair, their credal stance would be captured by $\{\mathsf{P}\}$, where P is simply a probability measure which assigns .5 to H. If, on the other hand, the agent knows nothing about the coin's bias, their stance would rather be represented by means of the set of all probabilistic measures, as none of them is excluded by the available evidence. Note that on IP it is not the case that the set represents

admissible options and the agent can legitimately pick any precise measure from the set. Rather, the agent's credal stance is essentially imprecise and has to be represented by means of the whole set.[4] The literature contains an array of arguments for IP. Let us take a look at the main ones.

- PP does not seem appropriately evidence-responsive, especially when evidence is limited. Following PP, in Peirce's example, the agent's uncertainty about W :="the next drawn ball is going to be white' is .5 no matter whether you have drawn two balls one of which was white, or a thousand balls, five hundred of which were white.
- Indifference is not sensitive to sweetening (improving the chances of H only slightly), while PP predicts such sensitivity. For instance, if you do not know what the bias of a coin is, learning that it now has been slightly modified to increase the probability of heads by .001 will still leave you unwilling to bet on heads in a bet that would've been fair if the actual chance of H was .5 and not .001.
- PP has problems representing complete lack of knowledge. Suppose you start tossing the coin starting with knowing only that the coin bias is in $[0,1]$ and then observe the outcome of ten tosses, half of which turn out to be heads. This is some evidence for the real bias being around .5. How do you represent your stances before and after the observations? If you deploy the principle of insufficient evidence, you start with $\mathsf{P}_0(H) = .5$ and end with $\mathsf{P}_1(H) = .5$, as if nothing changed. If you do not deploy the principle of insufficient evidence, what do you do?
- PP has problems with formulating a sensible method of probabilistic opinion aggregation Stewart & Quintana (2018). A seemingly intuitive constraint is that if every member agrees that X and Y are probabilistically independent, the aggregated credence should respect this. But this is hard to achieve if we stick to PP (Dietrich & List, 2016). For instance, a *prima facie* obvious method of linear pooling does not respect this. Consider probabilistic measures p and q such that $p(X) = p(Y) = p(X|Y) = 1/3$ and $q(X) = q(Y) = q(X|Y) = 2/3$. On both measures, taken separately, X and Y are independent. Now take the average, $r = p/2 + q/2$. Then $r(X \cap Y) = 5/18 \neq r(X)r(Y) = 1/4$.

One key difference between Kyburg's EP and IP is that on the latter we use sets of probability measure instead of intervals. This makes the approach not only more general (as now, for instance, the resulting probabilities of a proposition in question do not have to form a closed interval), but also provides a more general and less idiosyncratic picture of learning from evidence, that is a a natural extension of the classical Bayesian approach. When faced with new evidence E between time t_0 and t_1, RA's representor should be updated point-wise, running the standard Bayesian updating on each probability measure in the representor:

$$\mathbb{P}_{t_1} = \{\mathsf{P}_{t_1} | \exists\, \mathsf{P}_{t_0} \in \mathbb{P}_{t_0}\ \forall H \left[\mathsf{P}_{t_1}(H) = \mathsf{P}_{t_0}(H|E)\right]\}.$$

How is weight of evidence to be measured on IP (Joyce, 2005; M. Kaplan, 1996; Sturgeon, 2008)? One line, analogous to the one taken by Pedden is to take the edges of the resulting interval that captures changes in the weight of evidence (Walley, 1991). If you know the proportion of beans is between .02 and 1, your interval is wider than it is if you know the proportion is between 0.72665. In this sense, this approach, admittedly, handles cases such as the beans example.

The main question here, though, is that it is not clear how you are supposed to learn that the proportion of beans is such and such. This is related to the problem of belief inertia (Levi, 1980). Say you start drawing beans knowing only that the true proportion of red beans is in $(0,1)$ and then draw two beans both of which are red. On IP your initial credal state is to be modeled by the set of all possible probability measures over your algebra of propositions. Once you obser ve the two beans, each particular measure from your initial representor gets updated to a different one that assigns a higher probability to "red", but also each measure in your original representor can be obtained by updating some other measure in your original representor on the evidence (and the picture does not change if you continue with the remaining 2998 observations) . Thus, if you are to update your representor point-wise, you will end up with the same representor set. Consequently, the edges of your resulting interval will remain the same.

Relatedly, recall that the main selling point for IP was its ability to account for how credal states are responsive to the evidence and the amount thereof. But it is not clear how to make sense of evidential constraints in a way that makes them go beyond testimonial evidence. On IP the representors are

[4] For the development of IP see (Fraassen, 2006; Gärdenfors & Sahlin, 1982; Joyce, 2005; J. Kaplan, 1968; Keynes, 1921; Levi, 1974; Sturgeon, 2008; Walley, 1991), (Bradley, 2019) is a good source of literature.

somehow to obey the evidential constraints: they are supposed to be point-wise updated on the evidence, and to contain only those probabilistic measures that are not excluded by the evidence obtained so far. But how exactly does the evidence exclude probability measures? This is not a mathematical question: mathematically (Bradley, 2012), evidential constraints are fairly easy to model, as they can take the form of the *evidence of chances* $\{\mathsf{P}(X) = x\}$ or $\mathsf{P}(X) \in [x, y]$, or be *structural constraints* such as "X and Y are independent" or "X is more likely than Y." While it is clear that these constraints are something that an agent can come to accept if offered such information by an expert to which the agent completely defers, it is not trivial to explain how non-testimonial evidence can result in such constraints.

Most of the examples in the literature start with the assumption that the agent is told by a believable source that the chances are such-and-such, or that the experimental set-up is such that the agent knows that such and such structural constraint is satisfied. But outside of such ideal circumstances what observations exactly would need to be made to come to accept such constraints remains unclear. And the question is urging: even if you were lucky enough to run into an expert that you completely trust that provides you with a constraint like this, how exactly did the expert come to learn the constraint? The chain of testimonial evidence has to end somewhere!

Admittedly, there are straightforward degenerate cases: if you see the outcome of a coin toss to be heads, you reject the measure with $\mathsf{P}(H) = 0$, and similarly for tails. Another class of cases might arise if you are randomly drawing objects from a finite set where the real frequencies are already known, because this finite set has been inspected. But such extreme cases aside, what else? Mere consistency constraint wouldn't get the agent very far in the game of excluding probability measures, as way too many probability measures are strictly speaking still consistent with the observations for evidence to result in epistemic progress.

Bradley suggests that "statistical evidence might inform [evidential] constraints [. . . and that evidence] of causes might inform structural constraints" [125-126]. This, however, is far cry from a clear account of how exactly this should proceed. Now, one suggestion might be that once a statistical significance threshold is selected, a given set of observations with a selection of background modeling assumptions yields a confidence interval, and perhaps that the bounds of this confidence interval should be the lower and upper envelope—this is in line with the example used by Pedden (and our objections to such a use of conidence intervals apply here as well). Moreover, notice that whatever problems Bayesian statisticians raise against classical statistics apply here. To mention a few: the approach uses MLE and so is not sensitive to priors (or, in other words, is equivalent to always taking maximally uninformative priors), the estimates are sensitive to stopping intention, and there are no clear methods for combining various pieces of information of this sort (if this was easy, there would be no need for meta-analysis in statistical practice).[5]

Even supposing that a sensible mechanism of the exclusion in question has been proposed, measuring weight along the lines of (WK) or by the absolute distance between the edges of the interval has a weakness we already signaled. It is a function of two points and is completely insensitive to what happens between them. If we are looking at a somewhat monolithic class of distributions, say all beta distributions, looking at where the 1st and the 99th centiles are located might be a good rough guide to what is happening in between. But without such a restriction, not so much.

Here is an example of how easily more complicated cases that could be misrepresented by looking only at the envelopes might arise. Suppose you start with knowing that the coin bias lies within $(.4,.6)$ (Situation A). Then you hear from two equally reliable witnesses: one tells you that the real bias is exactly .4 and the other one tells them the real bias is exactly .6 (Situation B). It seems that you now have more evidence than before, but it is unclear how this difference is to be captured by the edges of the interval of non-excluded values, as the edges are exactly the same.

Another stab at explicating weight of evidence within the IP framework has been made by Joyce (2005). Joyce uses a density over chance hypotheses to account for the notion of evidential weight. He conceptualizes the weight of evidence as an increase of concentration of smaller subsets of chance hypotheses:

$$w(X, E) = \sum_x |c(ch(X) = x|E) \times (x - c(X|E))^2 - c(ch(X) = x) \times (x - c(X))^2| \qquad \text{(Joyce)}$$

This looks a bit complicated, so let us take this slow. Suppose you only consider three chance hypotheses,

[5] Admittedly, there are formulae for calculating confindence intervals based on two confidence intervals if they are based on separate independent observations in an experiment of exactly the same design, but this is a very idealized setup.

that the coin bias is one of $.4, .5$, and $.6$, that is, the hypotheses are $ch(X) = .4, ch(X) = .5$, and $ch(X) = .6$. For each $x \in \{.4, .5, .6\}$) you attach a prior credence $c(ch(X) = x)$ to the corresponding hypothesis. Say you start with equal priors, that is for all $x \in \{.4, .5, .6\}$ you have $c(ch(X) = x) = 1/3$. Then, your expected value of X, which Joyce takes to be your credence in X simpliciter is $\sum_x c(Ch(X) = x)x$, which is .5.

Now consider your evidence: you tossed the coin and observed, say, seven heads out of ten tosses. We need $c(ch(X) = x|E)$. By Bayes, we have:

$$c(ch(X) = x|E) = \frac{c(E|ch(X) = x)c(ch(X) = x)}{c(E)},$$

so we need to calculate the likelihoods, $c(E|ch(X) = x)$. We assume you are probabilistically coherent, that you defer to chances, and know the experimental setup, so that the likelihoods are calculated using the binomial distribution, i.e. if the evidence is a heads and b tails:

$$c(E|ch(X) = x) = \binom{a+b}{a} x^a (1-x)^b$$

In our example, the likelihoods (rounded) are $.042, .117$, and $.214$ respectively. The denominator is calculated by taking $c(E) = \sum_x c(E|ch(X) = x)c(ch(X) = x)$, which in our case turns out to be $.124$. Putting these together, the values of $c(ch(X) = x|E)$ are $.113$ $.312$, and $.573$ (rounded). Then, your expected value, which Joyce to be your credence in X simpliciter conditional on E is $\sum_x c(Ch(X) = x|E)x$, which is $.54$.

Once that we went over an example illustrating the quantities employed in (Joyce), before we plug these into the final formula, let us try to understand Joyce's motivations for such an explication. The idea here is that weighty evidence should make the credence resilient, and resilience makes the difference between the posterior credence in chances $c(ch(X) = x|E)$ and the prior credence in chances $c(ch(X) = x)$. The complication is that the impact of this difference should be lower for for those values of x that are close to $c(X|E)$ for the posterior and close to $c(X)$ for the prior. Hence, the formula for w is takes (the absolute value of) the difference between posteriors and priors weighed by, these (squared) distances. The weightier the evidence, the smaller w is supposed to be.

Accordingly, in our example the weights for the prior are $-.1^2, 0, .1^2 = 0.01, 0, .01$, the weights for the posterior are $.021330539, .002120582$, and $.0029$ and w is 0.003241822. For comparison, if instead we observed 70 heads in 100 tosses, w would be $.006689603$.

There are various issues with this approach. One is that now to evaluate the weight of evidence E with respect to proposition X now you need to have and use in your calculations your estimation of chances of X. Let us put aside the worry that it is not obvious that we can meaningfully talk about chances of arbitrary propositions. Even then, the name of the game for the imprecise probabilist was to express the uncertainty about X in terms of a representor, a set of probability measures. However, one can have a representor with respect to a set of object-level propositions including X without having a single credence about chances, so now the calculations of weight of E with respect to X do not fall out whatever was supposed to capture the agent's uncertainty about X, E and their relationship.

Moreover, the reader might have observed that the values of w for our example are not very telling. A ten-fold increase in sample size, with frequency being fixed, results in w dropping by 62%, and both are small numbers that are hard to interpret. This raises the question: how does the measure behave with respect to binomial trials and are the outcomes intuitively acceptable? Let us take the measure for a ride.

First, suppose we keep the same priors, and calculate w depending on how many successes we have observed in the 10 tosses. The results are as follows:

note: grows slower if evidence is against what you believe!

The behavior of w is even more unusual if the sample size is higher. In Figure 4 we illustrate what happens with $n = 100$, for various possible outcomes of 100 Bernoulli trials.

So this measure might result in drastic shift in weights even if the observed frequenciesa are not too far from the chance hypotheses. This we find undesirable.

In Figure **??** we illustrate two other phenomena, which might come up when the observed frequencies are kept fixed, but the sample size increases.

What is the reason for this strange behavior? The shaping of Joyce's weight is a balancing act. For instance, for frequency .1 with equal priors the weight is maximized at $n = 90$ and starts dropping at $n = 100$. Why? We start with three chance hypotheses, $.4, .5, .6$ with equal priors. Once the observations

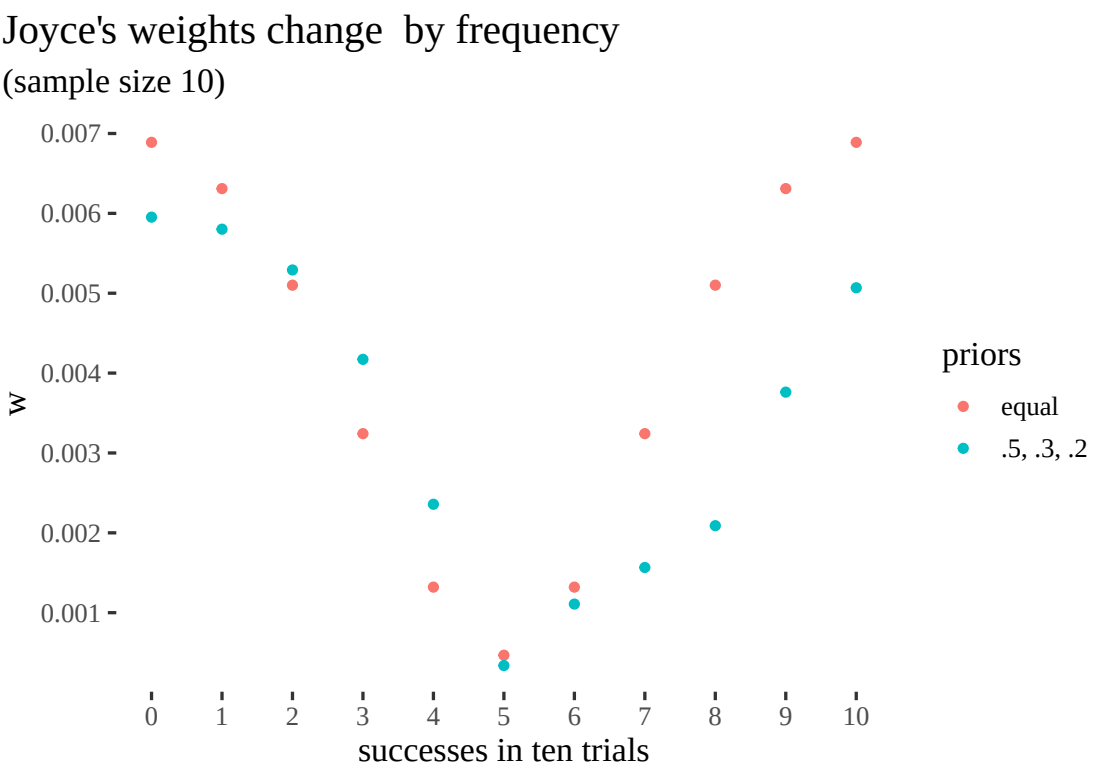


Figure 2: Joyce's w (the lower it is, the higher the weight) for various observed successes in 10 Bernoulli trials. Three chance chypotheses: $.4, .5, .6$, and two sets of priors: equal and $.5, .3, .2$ respectively. Note how the weightiest evidence is obtained with five observed successes and how its drop is fourteen-fold, if the observed frequency is 0.

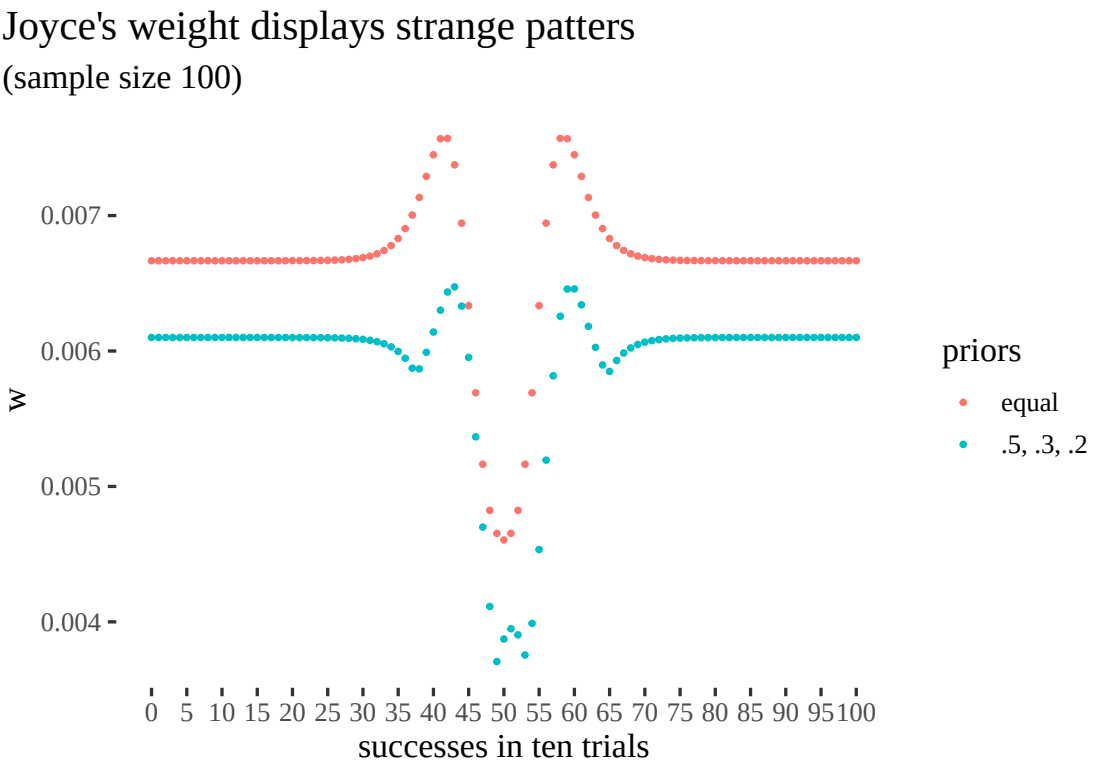


Figure 3: Joyce's w (the lower it is, the higher the weight) for various observed successes in 100 Bernoulli trials. Three chance chypotheses: $.4, .5, .6$, and two sets of priors: equal and $.5, .3, .2$ respectively. Agan, the weightiest evidence is obtained with successes close to the expected value, with large variation for observed frequencies not too far from the expected values, fairly flat otherwise.

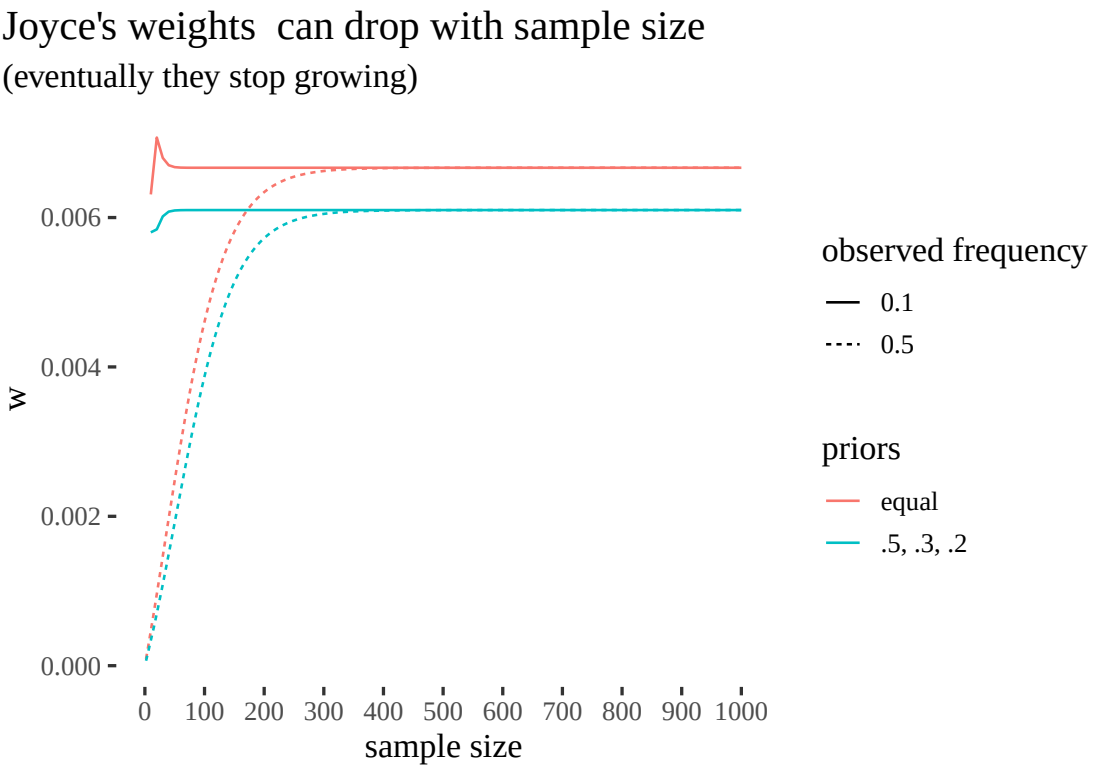


Figure 4: Joyce's w (the lower it is, the higher the weight) for two fixed success ratio across various observed successes in Bernoulli trials (lines are used for smoothing). Note large shifts with possible decrease in the beginning, and a flattening afterwards.

have been made, the posterior for the chance hypotheses is focused at the chance hypothesis .4 (its posterior is more or less .99999), and so is the credence in X simpliciter (this expected value is .4000003). Now the weights are build by measuring square distance from the credence in X simpliciter and since the expected value is nearly equal to the lowest chance hypothesis under consideration, the weight for the lowest chance hypothesis is $8.260660e-14$, so while the posterior for this hypothesis is very high, the weight is very low and its contribution to the weight calculation is severely limited. Ultimately, what happens with weight is now a matter of balancing the uneven posteriors with squared penalties (or rewards, really) for the distance from the expected value (which is pretty much the most likely hypothesis once you have made enough observations). Once you observe 10 successes in 1000 trials, the credence in X simpliciter becomes .4000001, so the distance of the lowest chance hypothesis to it drops, and this weight drops "faster" than the resulting increase in the probability of the lowest chance hypothesis itself. The stabiliziation is achieved because further on the posterior for this hypothesis can only get closer to one (and closer to zero for all the other hypotheses).

Moreover, this approach is of limited applicability. For one thing, as Joyce admits, it is supposed to work when RA's credence is mediated by chance hypotheses. Depending on applications, such a mediation might be unavailable. Another issue is that this might work for unimodal distributions when we only consider the influx of new data points, but it's unlikely to give desired results if, say, the evidence obtained is the testimony of disagreeing witnesses. This is because an essential part of the calculations relies on taking the expected value, and it is not too hard to imagine cases of diverging items of evidence resulting only in a small chance of the expected value.

Should I further elaborate?

The approach insists that an agent's stance towards a proposition should be represented as the expected value of the chance hypothesis—and we will argue against this view later on. At this point, what is crucial for us, the proposal employs probabilities or probability densities (if we go continuous) over parameter values. Even if we do not assume these are chances and treat them as, say, parameters that are potentially rational to accept in light of the evidence, by using this approach we no longer can represent uncertainty about a proposition in terms of a set of probability measures over any algebra containing the proposition itself (as the algebra now also needs to contain the chance hypotheses as well!). Perhaps,

this is as it should be. But then, as I will argue later one, there are useful ways to go this way without turning to IP—after all, notice how the notion of a representor plays no role in Joyce's explication of weight whatsoever!

9 Troubles with imprecise probabilism

The interval-based IP approach to weight of evidence runs into two specific problem: insensitivity to what happens between the edges, and sensitivity to risk-related decisions made which are not even represented in IP's preferred uncertainty representation methods. There are however more general problems with IP, which suggest we should move on, and to some extent the direction in which we should do so.

- IP has no means of distinguishing the situation in which you are about to toss a coin whose bias is either .4 or .6., and the one in which you are about to toss a coin whose bias is also either .4 or .6,, but the bias .4 is three times more likely than .6, at least not without moving to higher-order probabilities, in which case it is no longer clear whether the object-level imprecision performs any valuable task.
- if one decides to favor IP over PP because PP involves seemingly artificial precision, such artificial precision can't be avoided by moving to IP (Carr, 2020). Take the well-known Jellyfish example (Elga, 2010): I'm pulling stuff out of my purse, there seems to be no rule as to what I have pulled out so far, how likely is it that the next thing I pull out will be a jellyfish? The impreciser is committed to there being a precise *range* of probabilities to be assigned to the jellyfish hypothesis. Say it's $[.2-.8]$. But why this rather than, say $[.2,.80001]$?
- IP gives wrong comparison predictions (Rinard, 2013). Suppose you know of two urns, GREEN and MYSTERY. You are certain GREEN contains only green marbles, but have no information about MYSTERY. A marble will be drawn at random from each. You should be certain that the marble drawn from GREEN will be green (G), and you should be more confident about this than about the proposition that the marble from MYSTERY will be green (M). In line with how lack of information is to be represented on IP, for each $r \in [0,1]$ your representor contains a P with $\mathsf{P}(M) = r$. But then, it also contains one with $\mathsf{P}(M) = 1$. This means that it is not the case that for any probability measure P in your representor, $\mathsf{P}(G) > \mathsf{P}(M)$, that is, it is not the case that RA is more confident of G than of M. This is highly counter-intuitive.
- IP, as already mentioned, faces belief inertia. Here's another example from (Rinard, 2013). Either all the marbles in the urn are green (H_1), or exactly one tenth of the marbles are green (H_2). Your initial credence $[0,1]$ in each. Then you learn that a marble drawn at random from the urn is green (E). After conditionalizing each function in your representor on this evidence, you end up with the the same spread of values for H_1 that you had before learning E, and no matter how many marbles are sampled from the urn and found to be green.[6]
- Another problem arises when we reflect on the notion of the accuracy of imprecise credal states. A variety of workable **scoring rules** for measuring the accuracy of a single credence function, such as the Brier score, is available. One key feature that some key candidates have is that they are *proper*: any agent will score her own credence function to be more inaccurate than every other credence function. After all, if an agent thought a different credence is more accurate, they should switch to it. The availability of such scoring rules underlies an array of accuracy-oriented arguments for PP (roughly, if your credence is probabilistic, no other credence is going to be more accurate whatever the facts are than yours). When we turn to IP, there are limitation results to the effect that no proper scoring rules are available for representors, and so no accuracy-oriented foundations for IP have been developed (Campbell-Moore, 2020; Mayo-Wilson & Wheeler, 2016;

[6] Some replies on behalf of IP are available. One might insist that vacuous priors should not be used and that the framework gives the right results when the priors are non-vacuous. Another strategy is to say that in a state of complete ignorance a special updating rule should be deployed. (Lee, 2017) suggests the rule of *credal set replacement* that recommends that upon receiving evidence the agent should drop measures rendered implausible, and add all non-extreme plausible probability measures. This however, is tricky: one needs a separate account of what makes a distribution plausible or not. Elkin admits that he has no solution to this: "But how do we determine what the set of plausible probability measures is relative to E? There is no precise rule that I am aware of for determining such set at this moment, but I might say that the set can sometimes be determined fairly easily" [p. 83] He goes on to a trivial example of learning that the coin is fair and dropping extreme probabilities. This is far from a general account. One also needs a principled account of why one should use a separate special update rule when starting with complete ignorance.

Schoenfield, 2017; Seidenfeld, Schervish, & Kadane, 2012).

- Here's another difficulty, which comes up when you consider aggregating probabilistic opinions of various sources, such as experts. One strategy, proposed within PP, is linear pooling, which, among other conditions, satisfies the *reasonable range* assumption, according to which for any group of peers, G, whose credences in a proposition X range from x to y, the aggregated credence is within the reasonable range for members of G, that is within the closed interval $[x, y]$. IP has a related feature: if the aggregation of representors is their union, the upper and lower envelopes with respect to X after aggregation will be simply the maximum and the minimum of the individual expert's envelopes. The effect of this phenomenon is that if the uncertainty of a representor is to be captured by the range of its envelopes (Walley, 1991), there is no way aggregation could increase certainty, also on IP. However, there seem to be examples in which—intuitively—learning that a peer has a different credence should in some sense boost RA's original credence. Take an example from (Christensen, 2009): there might be a doctor who is fairly confident that a treatment dosage for a patient is correct (.97) and considers the opinion of a colleague, who is slightly less confident that this treatment dosage is correct, say the colleague's credence is 0.96—this, intuitively, should be taken as confirming evidence that warrants a confidence boost. The challenge—both for PP and IP—is to make sense of this intuition.[7] There are also other problems with pooling as representor summation. If all you do when you aggregate experts with two representors is put the sets together, the strategy isn't very subtle. For one thing, you don't pay much attention to what the experts think of particular measures. On one hand, IP has no means of representing and using the information about the experts thinking some measures to be more plausible candidates than others, and on the other, whether a certain measure is in both representors in not is not going to be reflected in the result of the aggregation, and so the framework does not seem to capture at least some power of experts' agreement. Thus, making sense of opinion pooling and synergy remains a challenge even from the perspective of IP.
- IP has been marketed as an approach on which credal states are more evidence-responsive than they would be on PP. The question is, whether it delivers. We already discussed general problems how intervals are to be learned from evidence. There is another problem lurking in the neighborhood, as to whether accuracy considerations ever recommend the kind of evidence sensitivity that IP seems to be promising. The problem has been raised by Schoenfield (2017): it seems that if an accuracy measure for imprecise credences satisfies certain fairly straightforward constraints, the intermediate value theorem jointly with the requirement that no probabilistic credal state (precise or imprecise) should be dominated by another credal state entail that—at least in a simple coin-tossing set-up—for any imprecise credal state one might have there is a precise one with at least the same accuracy. If this result generalizes, it will be very hard for one to claim that what justifies an agent's acceptance of an imprecise credal state instead of a precise one is accuracy considerations.

10 A second-order approach to uncertainty

There is, however, a view in the neighborhood that fares better: a second-order perspective. In fact, some intuitions very much like this approach shines through some of the comments and moves made by the proponents of IP. One example that we have already seen is Joyce's use of chance hypotheses in his explication of weight. Another case is Bradley, who in his discussion of belief inertia compares particular measures in a representor as of committee members and explains that '...the committee members are "bunching up". Whatever measure you put over the set of probability functions—whatever "second order probability" you use—the "mass" of this measure gets more and more concentrated around the true chance hypothesis' [BRADLEY p. 157] Note however, how such bunching up cannot be

[7] Perhaps, the key aspect here is that the colleague isn't really an epistemic peer: her experience, evidence and therefore knowledge are somewhat different, and so by incorporating the colleague's credal state in the judgment the doctor in fact incorporates whatever new evidence her colleague have experienced. One could argue that therefore using linear pooling is not appropriate as it is meant to be used for the opinion aggregation of epistemic peers who have exactly the same evidence and exactly the same competence. If that's the case, the method seems to be devised to work for ideally spherical cows in a vacuum. Most cases of belief aggregation are not problems of this sort, and so a systematic approach to belief aggregation of credal states of agents even if they are *not* epistemic peers is a more urging problem.

modeled by IP.[8]

The idea that one should use higher-order probabilities has also been suggested by a critic of IP. (Carr, 2020) argues that indeterminate evidence does not require representors: instead, imprecise evidence requires uncertainty about what credences to have. On Carr's approach, one should use vague credences, assigning various weights to probabilities—agent's credence in propositions about either what credences the evidence supports, or about objective chances. Unfortunately, Carr does not develop this suggestion into a full-fledged proposal, does not explicate her ideas formally, and does not explain how this approach plays out when we talk about the difficulties pestering PP and IP.

This is our goal at this point. To properly develop the higher-order alternative to the expression of uncertainty, argue that it handles the problems that IP runs into. Once this alternative is in place, we will propose an explication of the notion of weight of evidence developed in a principled manner relying on the ideas from information theory, and argue that it performs much better than its predecessors.

The key idea is that uncertainty is not a single-dimensional thing to be mapped on a single one-dimensional scale such as a real line. It is the whole shape of the whole distribution over parameter values that should be taken under consideration. [9] From this perspective, sometimes, when an agent is asked about her credal stance towards X, they can refuse to summarize it in terms of a point value $\mathsf{P}(X)$, instead expressing it in terms of a probability (density) distribution f_x treating $\mathsf{P}(X)$ as a random variable. Coming back to an example we already argued IP cannot capture, when the agent knows that the real chance is either .4 or .6 but the former is three times more likely, she might refuse to summarize her credal stance by saying that $\mathsf{PR}(H) = .75 \times .4 + .25 \times .6 = .45$. More generally, on this perspective, against Joyce, the agent might deny that $\int_0^1 x f(x)\, dx$ is their object-level credence in X, if f is the probability density over possible object-level probability values and f is not sufficiently concentrated around a single value for such a one-point summary to do the justice to the complexity of the agent's credal state.[10] This approach in fact lines up with a fairly common practice in Bayesian statistics, where the primary role of uncertainty representation is assigned to the whole distribution, and summaries such as the mean, mode standard deviation, mean absolute deviation, or highest posterior density intervals are only inferior means or representing the uncertainty involved in a given study, to be used mostly due to practical restrictions.

REF

From this perspective, the various scenarios we discussed (which IP has hard time distinguishing between) can be easily represented in the manner illustrated in Figure 5.

Summaries of the distributions, such as the expected value, are exactly that: a simplified and therefore somewhat inadequate representations of the underlying uncertainty. However, for some purposes—when simplification is desirable and brings no serious harm—they might be useful. One summary that comes in handy is the highest density interval (HDI). It is the narrowest interval containing a specified probability mass. HDIs are to be contrasted with credible intervals, which span between $\alpha/2$ and $1 - \alpha/2$ quantiles of the probability mass. The key difference is that credible intervals symmetrically get rid of tails of a distribution, which might make sense if the distribution is fairly symmetrical, but fails to be intuitive in other cases.

How to use the higher-order approach in qualitative comparisons? Suppose the agent's HDIs for probability parameters a and b associated with propositions A and B respectively have limits a_l, a_h, b_l, b_h (a low, a high, ...) respectively. We can say that the agent definitely considers A at least as likely as B ($A \geq B$) just in case $a_l \geq b_l$ and $a_h \geq b_h$, that $A > B$ iff $A \geq B$ but not $B \geq A$, and that the agent considers A plausible just in case $a_l > t$ for some sensibly high threshold t. This allows for clear-cut cases, but also for cases in which the agent is undecided, either about a comparison or about the plausibility a single proposition. This approach handles Rinard's GREEN-MYSTERY argument against the supervaluationist approach to qualitative comparison in IP. Now we are comparing HDIs instead. For the GREEN urn, the HDI is just $g = [1, 1]$, and since the distribution is uniform for the MYSTERY urn, its corresponding HDI is $m = [0, 1]$. In this setting, clearly $g_l > m_l$ and $g_h \geq m_h$, and so $G \geq M$, but not $M \geq G$, and therefore $G > M$. That is, the agent is more convinced about G than they are about M, as desired.

[8] He seems to be aware of that, which would explain the use of scare qutoes: when he talks about the option of using second-order probabilities in decision theory, he insists that 'there is no justification for saying that there is more of your representor here or there.' ~[p.~195]

[9] Bradley admits this much [90], and so does Konek in his rejection of locality [59]. For instance, Konek disagrees with: (1) X is more probable than Y just in case $p(X) > p(Y)$, (2) D positively supports H if $p_D(H) > p(H)$, or (3) A is preferable to B just in case the expected utility of A w.r.t. p is larger than that of B.

[10] Whether such expectation should be used in betting behavior is a separate problem, here we focus on epistemic issues.

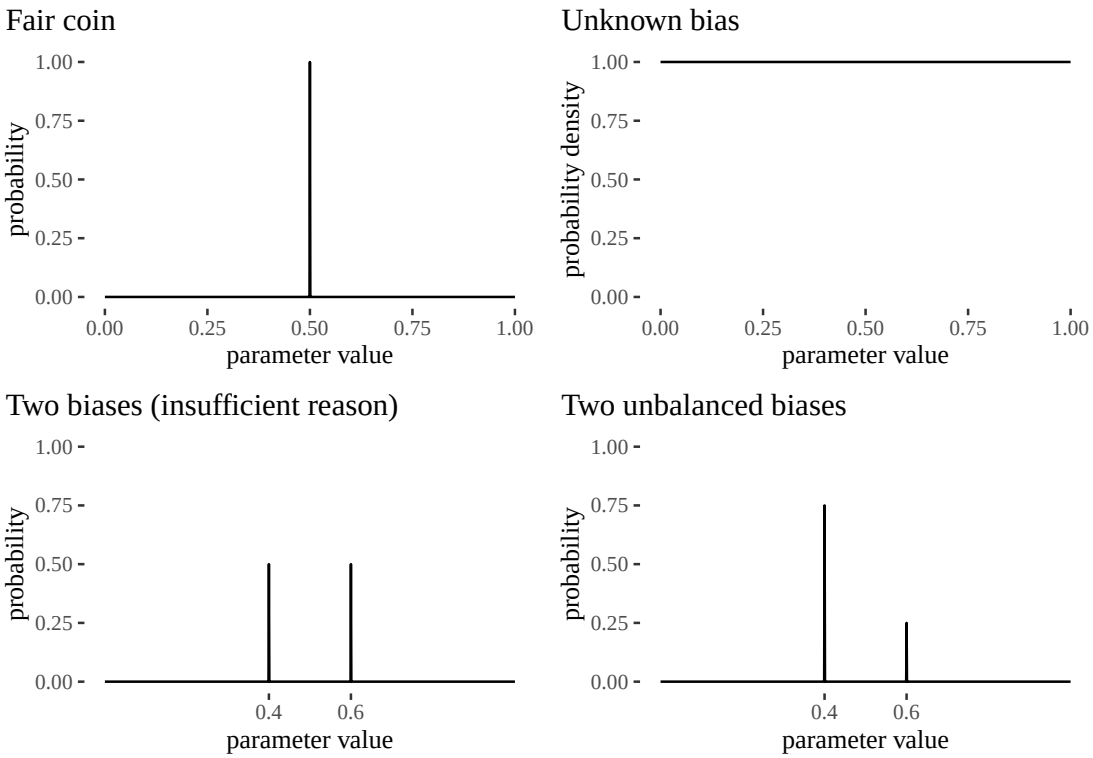


Figure 5: Examples of RA's distributions responding to various types of evidence for typical cases brought up in the literature.

Another difficulty for IP is belief inertia. In this framework, the problem does not arise, as there is no problem with modeling learning from observation starting from a uniform prior.

If you just start with a uniform density over $[0, 1]$ as your prior, use binomial probability as likelihood, observing any non-zero number of heads will exclude 0 and observing any non-zero number of tails will exclude 1 from the basis of the posterior. Let's see an example with a grid approximation ($n = 1k$). For simplicity assume there are only green and black balls. Our prior is uniform, and then, in subsequent steps, we observe one green ball, another green ball, and then a black ball. This is what happens with the posterior as we go (Figure 6).

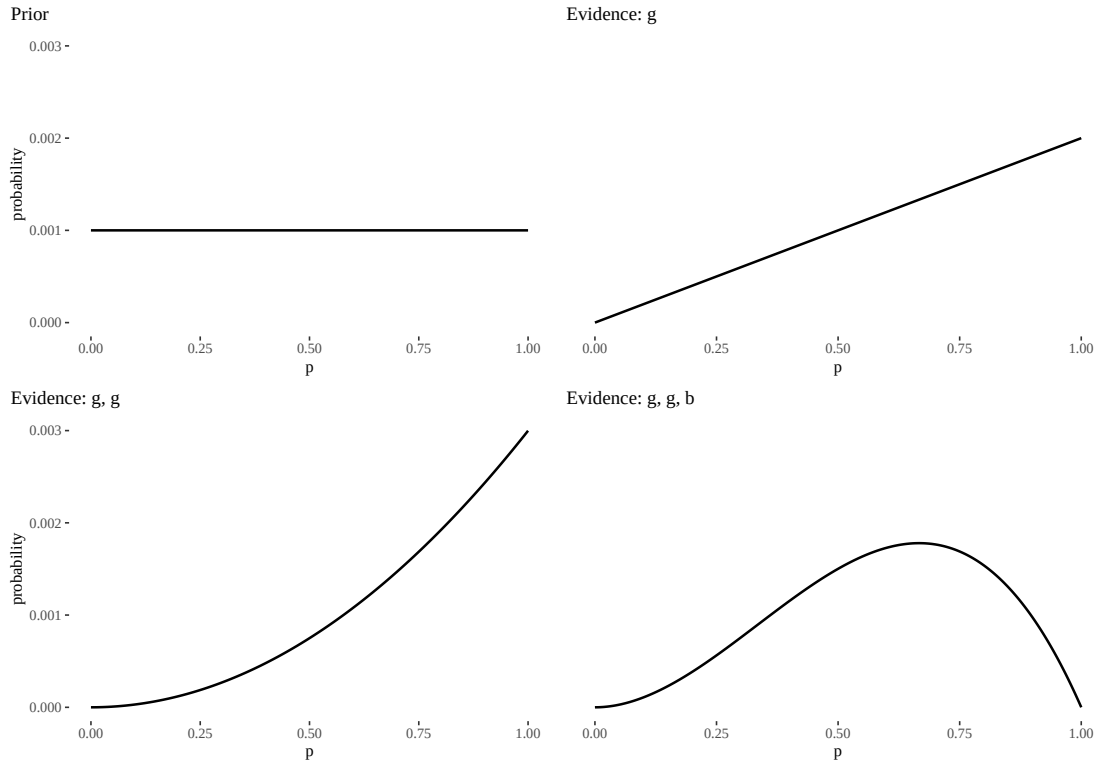


Figure 6: As observations of green, green and black come in, extreme parameter values drop out of the picture and the posterior is shaped by the evidence.

To see how this approach is also capable of modeling Rinard's example of inertia, lets start with MaxEnt recommending even priors of the two chance hypotheses. In Figure 10 we see what usual calculations revise these priors to, as we obtain new evidence, again, say: green, green, black. This behaves completely as expected with no inertia in sight. Note how the observations initially support H_1, but exclude H_1 in the last stage.

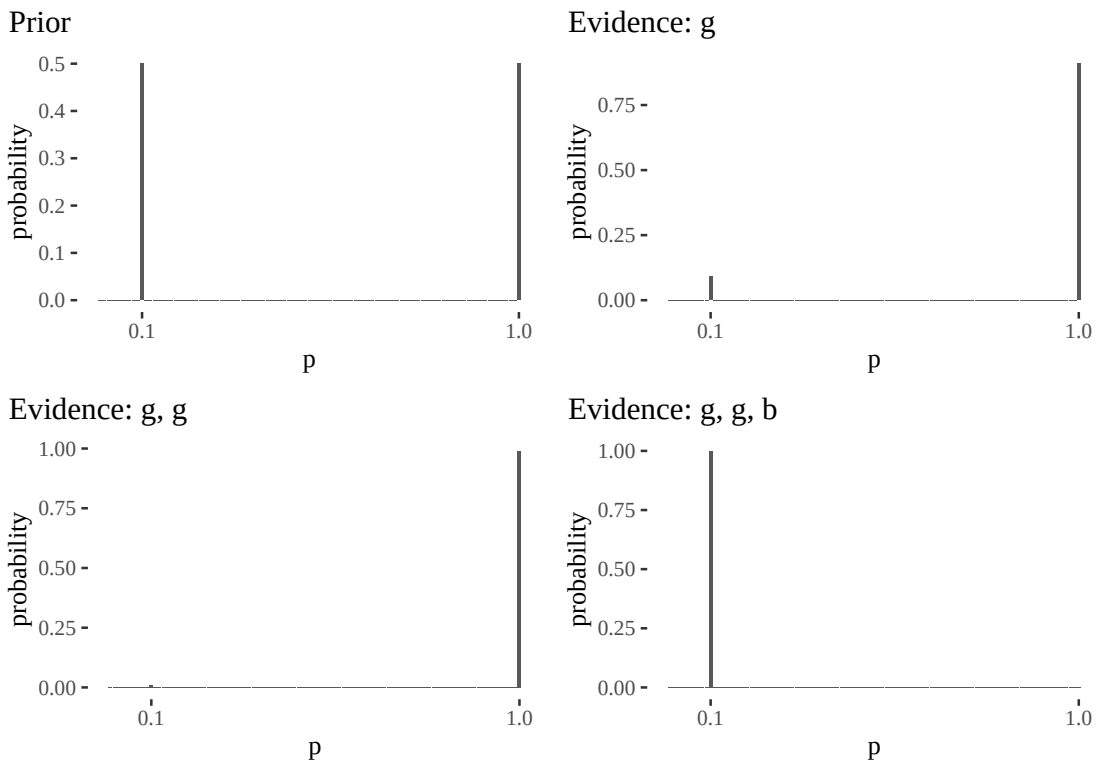


Figure 7: Learning in Rinard's example of belief inertia.

Talk about other objections here

11 Weight of a distribution

In order to be able to explain our explication of the notion of weight of evidence, we first need to introduce the information-theoretic background against which the explication will be developed. Consider a fairly simple binary case. Suppose you want to navigate from A to D, with the uninformed prior, at each junction thinking that each choice is equally likely to be the right one, your choices are visualized in Figure 8.

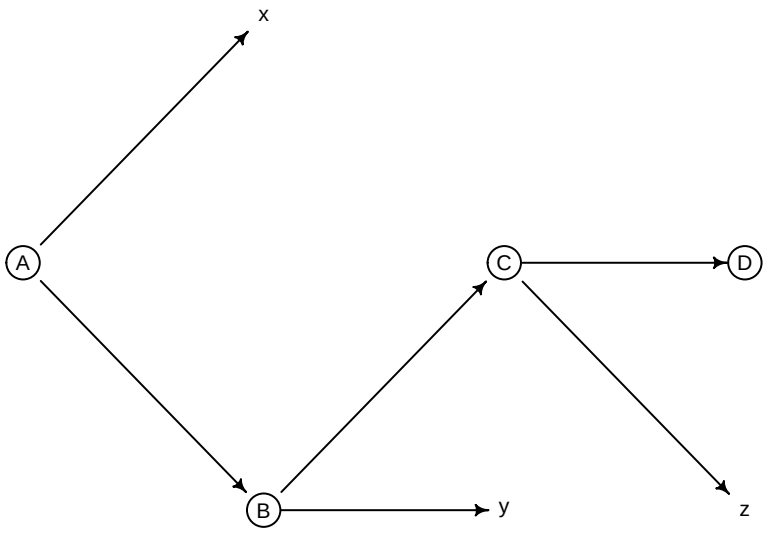


Figure 8: You want to navigate from A to D with the uninformed prior.

The route can be described using three digits. Suppose at each point the path on the left is marked 1, and the one on the right is marked 0. The right path is then 011. There are $m = 8$ possible destinations that could be reached by making decisions at $\log_2(8) = 3$ forks.

How much information are you given if I just tell you to take path 0 at the first fork? Initially, you thought the probability that it is the right one was .5. Now you know it is the right one. One natural measure is *surprise*, $1/.5 = 2$: there is a sense in which you now have twice the information that you had. If to make sure your measure of information is also additive, you transform surprise logarithmically, the *Shannon information* is $\log_2(1/.5) = 1$. That is, you receive one *bit* of information. If you receive the complete instructions, assuming your probabilities were independent, you receive $\log_2(1/.5^3) = 3$. Thus, intuitively, Shannon's information tracks the information you received in terms of how many binary decisions you are now able to make assuming you initially thought the options were equally likely and independent. Further, notice that $\log_2(\frac{1}{a}) = -\log_2(a)$ in general, so the official definition of Shannon information goes:

$$h(x) = -\log_2 \mathsf{P}(\mathsf{x})$$

If the outcomes are equally likely, $h(x)$ doesn't depend on the choice of x. However, if the distribution is not uniform, this will not be the case. A measure of (lack of) information contained in a whole distribution, is *entropy*, which is the average Shannon information:

$$H(X) = \sum \mathsf{P}(x_i) \log_2 \frac{1}{\mathsf{P}(x_i)} = -\sum \mathsf{P}(x_i) \log_2 \mathsf{P}(x_i)$$

Note that entropy is not the measure of information contained in a distribution. It is rather the opposite: the expected amount of information you receive once you learn what the value of X is. The less informative a distribution is, the more you expect to learn when you find out the value of X, the higher the entropy. Also, note that entropy is the function of the measure itself, so normally it makes sense to talk about the entropy of distributions rather than variables.

Interestingly, the move to continuous distributions is not straightforward,[11], so in what follows we prefer to stick to entropy proper. One reason is that we will want to meaningfully compare information

[11] One might expect that entropy in the continous case could be made by binning and taking the limit. For instance, suppose we divide X into bins x_i of length Δ, so that we discretize X into X^Δ. The discrete case definition applies:

$$H(X^\Delta) = \sum \left[\mathsf{P}(X \text{ is in the } i\text{-th bin}) \log_2 \frac{1}{\mathsf{P}(X \text{ is in the } i\text{-th bin})} \right]$$

If you think of the histogram of the distribution of X^Δ with total area A, each bin has area a_i and height p_i. Suppose we normalize so that $A = 1$, then the probability of each bin is $\mathsf{P}_i = p_i \Delta$ and p_i can be thought of probability density. Then we have:

$$\begin{aligned}
H(X^\Delta) &= \sum \mathsf{P}_i \log_2 \frac{1}{\mathsf{P}_i} \\
&= \sum p_i \Delta \log_2 \frac{1}{p_i \Delta} \\
&= \sum \left[p_i \Delta \left(\log_2 \frac{1}{p_i} + \log_2 \frac{1}{\Delta} \right) \right] \\
&= \sum p_i \Delta \log_2 \frac{1}{p_i} + \underbrace{\sum \underbrace{p_i \Delta}_{\mathsf{P}_i}}_{1} \log_2 \frac{1}{\Delta} \\
&= \sum p_i \Delta \log_2 \frac{1}{p_i} + \log_2 \frac{1}{\Delta}
\end{aligned}$$

Accordingly, when we try to go continuous by taking the limit, we get:

$$H(X) = \left[\int_{-\infty}^{\infty} p(x) \log_2 \frac{1}{p(x)} \, dx \right] + \infty$$

This is as it should: the entropy of a continuous variable increases with the precision of measurement, so infinite precision gives infinite information. For this reason, for the continuous case it is usual to drop the rightmost part of the equation and talk about *differential entropy*:

$$\mathsf{H}(X) = \left[\int_{-\infty}^{\infty} p(x) \log_2 \frac{1}{p(x)} \, dx \right]$$

conveyed by discrete distributions to that conveyed by continuous ones. A convenient way to do so is to abandon the idea that we should be infinitely precise, fix a certain number of bins (that is a certain level of precision) and keep it fixed in our comparison. This is what we will do: effectively, we will be using *grid approximations* of continuous distributions: we will split X into a 1000 bins and use the normalized densities for their centers to obtain their corresponding probabilities. As long as we do not change our level of precision (which would inevitably lead to changes in entropy) in our comparisons, this is not a problem. An additional advantage is that now we do not have to deal with the intricacies of explicit analytic calculations for continuous variables and comparing apples (entropy) with oranges (differential entropy).

Now, let us move forward towards a way to measure differences between distributions. First, the notion of *cross-entropy*. Suppose events arise according to a distribution P but we predict them using a distribution Q. The *cross-entropy* in such a situation is

$$\mathsf{H}(\mathsf{P},\mathsf{Q}) = \sum \mathsf{P}_i \log_2(\mathsf{Q}_i)$$

This value is going to be higher than the entropy of P itself, if Q is different from P. Now think about the additional entropy introduced by using Q instead of P itself, called *Kullback-Leibler divergence* (KL divergence):

$$\begin{aligned}
\mathsf{DKL}(\mathsf{P},\mathsf{Q}) &= H(\mathsf{P},\mathsf{Q}) - H(\mathsf{P}) \\
&= -\sum \mathsf{P}_i \log_2(\mathsf{Q}_i) - \left(-\sum \mathsf{P}_i \log_2 \mathsf{P}_i\right) \\
&= -\sum \mathsf{P}_i \left(\log_2 \mathsf{Q}_i - \log_2 \mathsf{P}_i\right) \\
&= \sum \mathsf{P}_i \left(\log_2 \mathsf{P}_i - \log_2 \mathsf{Q}_i\right) \\
&= \sum \mathsf{P}_i \log_2 \left(\frac{\mathsf{P}_i}{\mathsf{Q}_i}\right)
\end{aligned}$$

That is, KL divergence is the expected difference in log probabilities. In particular, if $\mathsf{P} = \mathsf{Q}$ we get $DKL(\mathsf{P},\mathsf{P}) = \sum \mathsf{P}_i(\log_2 \mathsf{P}_i - \log_2 \mathsf{P}_i) = 0$, which works out as it intuitively should be.[12]

Now, we can introduce the notion of weights as associated with distributions. In this sense, weights are just transformed distances from uniform distributions, giving us an information-theoretic measure of how uneven (or informative) a distribution is. Once we go over this, we will explain how to implement this approach to evidence. Once we can measure how uneven the posterior is compared to the prior, we have an explication of the notion of weight of evidence. It will be, of course, dependent on the priors—but this we take it to be intended and in line with the intuitions standing behind the earlier proposals (and if you prefer to compare posterior with the posterior given the negation of the evidence, the move is rather straightforward).

Discuss how deep we want to get into this

The idea is that the more informative a piece of evidence is, as compared to the uniform distribution, the more weight it has, on scale 0 to 1: if the drop from uncertainty is complete, the entropy drops to zero, and we would like the weight to be 1, if the drop is null we would like to be zero, and if the drop is half, we would like to be .5 (and so on for other proportions). This can be achieved by the following definition:

$$\mathsf{w}(\mathsf{P}_\mathsf{i}) = 1 - \left(\frac{H(\mathsf{P})}{H(\mathsf{uniform})}\right)$$

where P is the discrete probability distribution for a given number of bins n, and uniform is the discrete uniform distribution for the same number of bins.[13] Note that the entropy of a uniform distribution is

[12] Note that often in the context of Bayesian inference, the natural logarithm function is used in the divergence calculations; this only is a shift of scale and doesn't make much difference.

[13] In some contexts it might make sense to measure improvement with respect to a non-uniform prior. In such cases, $H(\mathsf{uniform})$ is to be replaced by $H(\mathsf{prior})$.

pretty straightforward, so we can simplify:

$$\begin{aligned} H(\mathsf{uniform}) &= \sum_{i=1}^{n} 1/n \log_2 \frac{1}{1/n} \\ &= \log_2(n) \\ \mathsf{w}(\mathsf{P_i}) &= 1 - \left(\frac{H(\mathsf{P})}{\log_2(n)} \right) \end{aligned}$$

Let's first see how this plays out with beta distributions.[14] The advantage of looking at them first is that they have a fairly straightforward interpretation: $\mathsf{beta(a, b)}$ is the distribution one should have when tossing a coin with unknown bias, having observed (or imagining to have observed) a heads and b tails, imagining that seeing one heads and one tails leaves you uninformed. From this perspective, $\mathsf{beta(1, 1)}$ is the uniform distribution, $\mathsf{beta(40, 10)}$ is the likelihood corresponding to 40 heads and 10 tails, and so on. To get a feel for what beta distributions look like, inspect Figure 9. Remember we're working with a grid approximation ($n = 1k$).

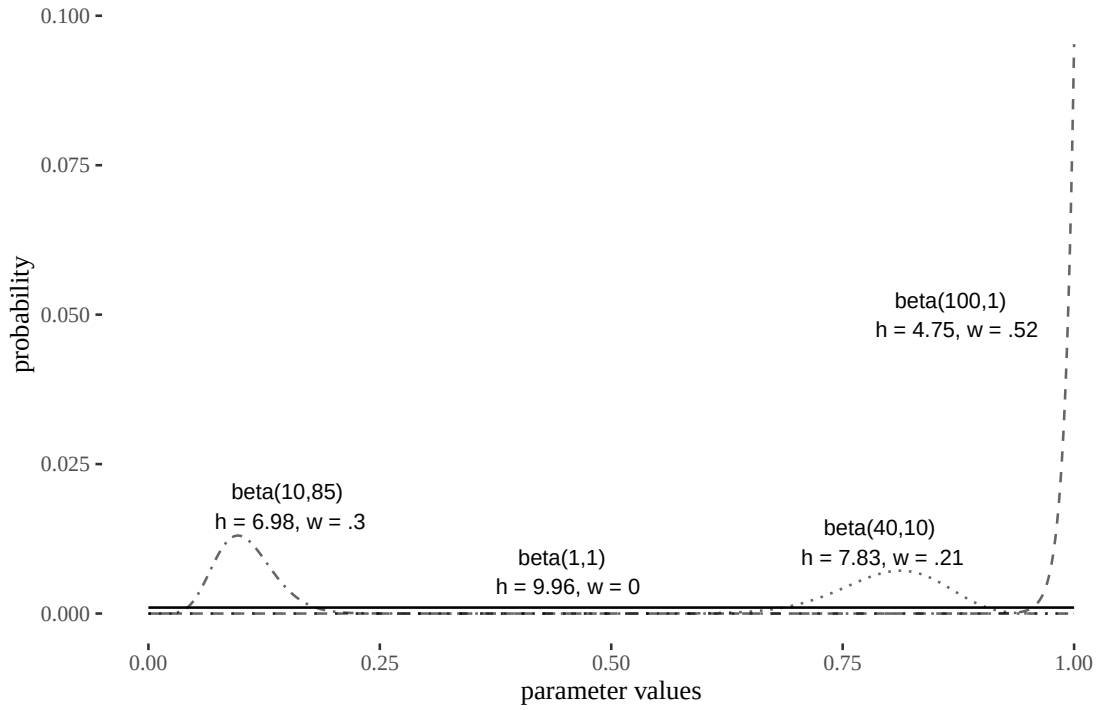
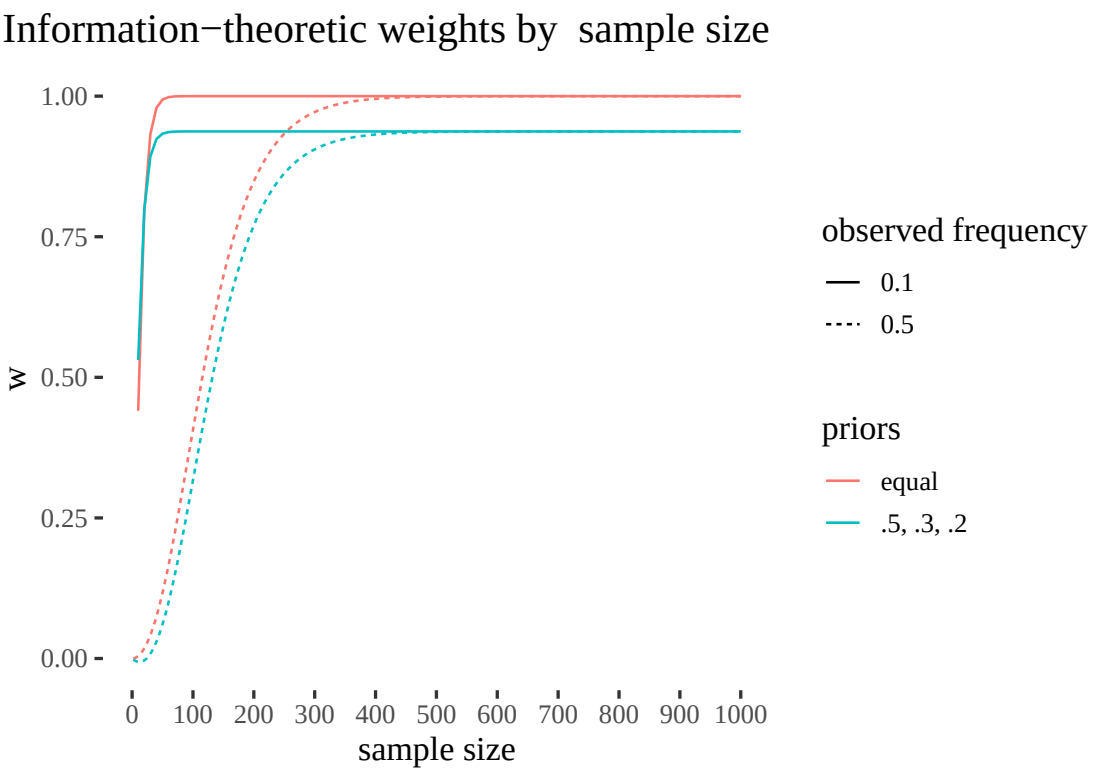


Figure 9: Examples of beta distributions with entropies and their weights with grid approximation ($n = 1000$). Note that distribution weight does not strongly depend on its expected value.

Now let us get back to the example we used to inspect the behavior of Joyce's proposal. Figure 11 illustrates dependency on frequency and on priors for sample size 10, and **??** for sample size 100.

[14] The reader might ask: why not to use the Kullback-Leibler divergence from the uniform distribution instead? Because this divergence does not measure the difference in how informed the distribution is. For instance, the divergence between a uniform distribution with a grid of $1k$ and a distribution that gives probability 1 to one chance hypotheses and 0 to all others, measured either way, is not going to be large, whereas for our purposes the weight should be maximal: we just went from complete lack of information to complete certainty. This intuition is vindicated when we use w.



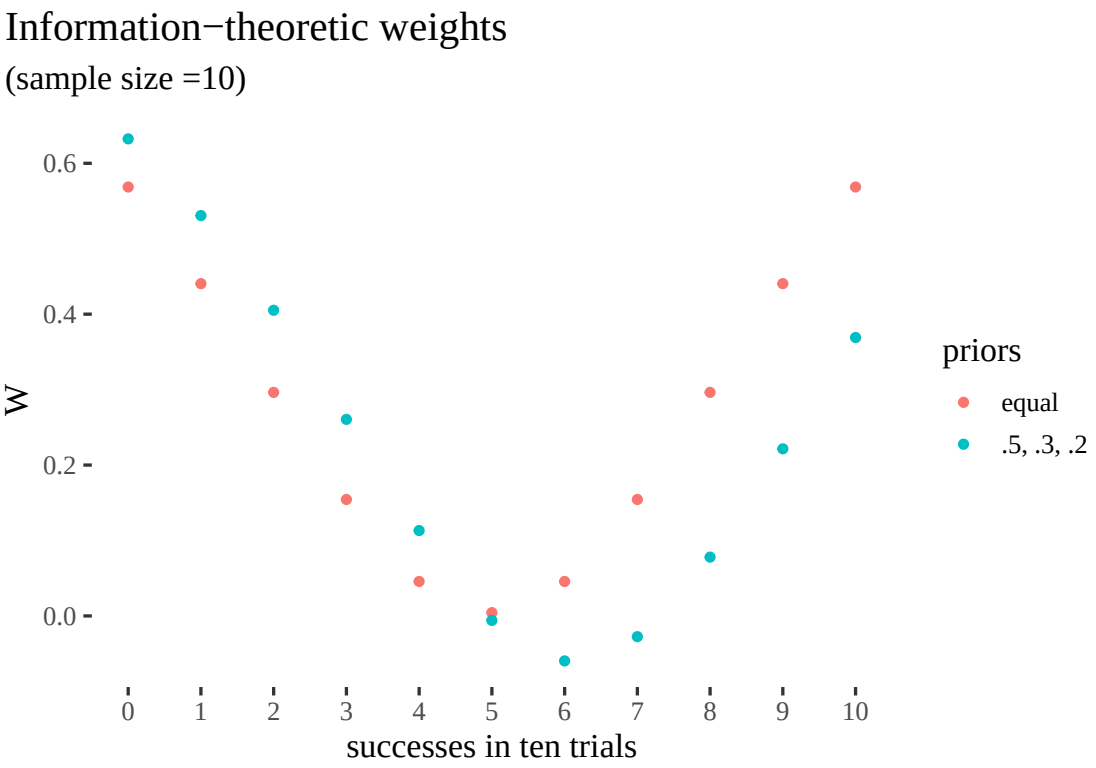


Figure 10: Entropy-based weight for for various observed successes in 10 Bernoulli trials. Three chance chypotheses: .4,.5,.6, and two sets of priors: equal and .5,.3,.2 respectively.

We also should inspect the behavior of w when the frequency is kept fixed, but the sample size increases (Figure 12).

compare with Joyce, revise comments there and add comments here

Now, let us generalize and see how the measure behaves with respect to beta distributions. Two phenomena are as expected. First, the entropy decreases with the number of observations, and second,

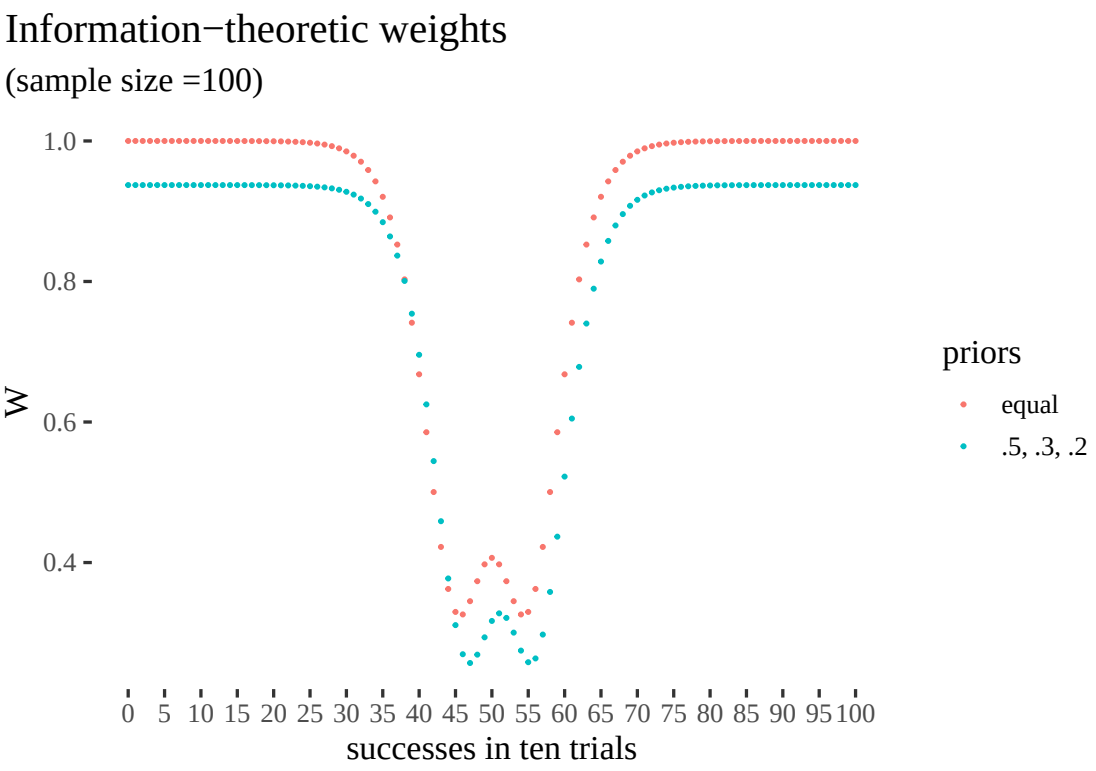


Figure 11: Entropy-based weight for for various observed successes in 100 Bernoulli trials. Three chance chypotheses: .4,.5,.6, and two sets of priors: equal and .5,.3,.2 respectively.

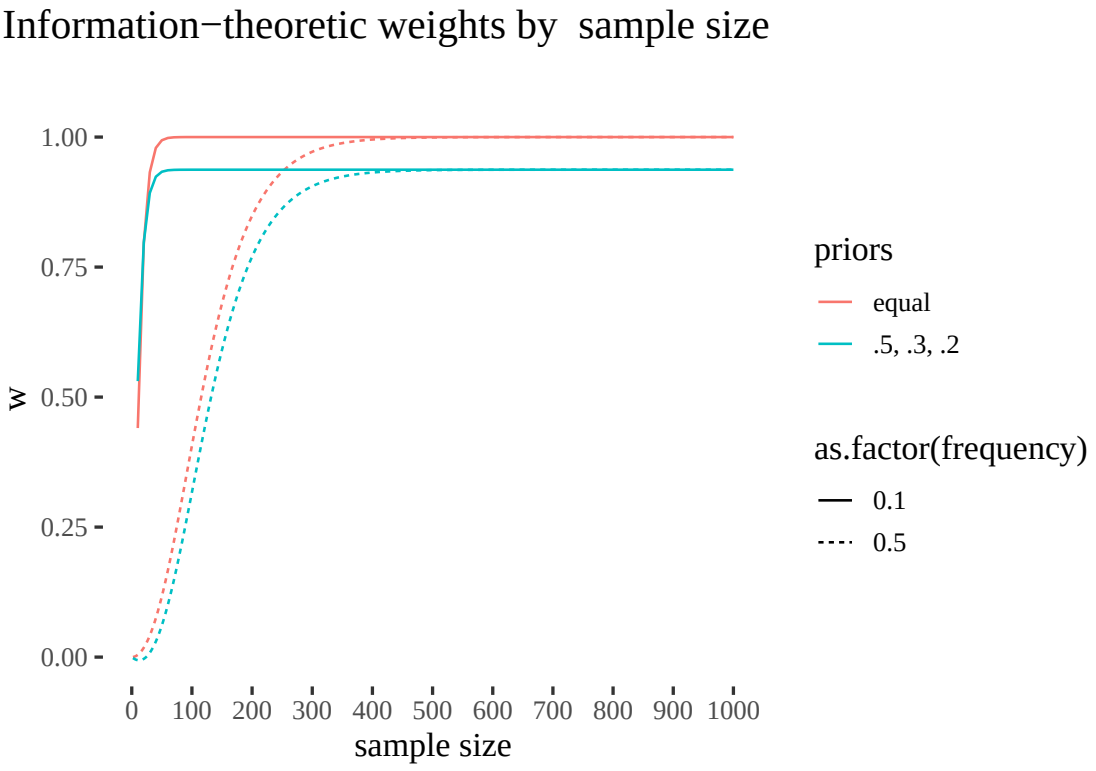


Figure 12: Entropy-based weight for two observed frequencies for various sample sizes (lines used instead of points for smoothing). Three chance chypotheses: .4,.5,.6, and two sets of priors: equal and .5,.3,.2 respectively.

it decreases faster if the proportions are closer to the extremes. This is mirrored by the corresponding weights (Figure 13).

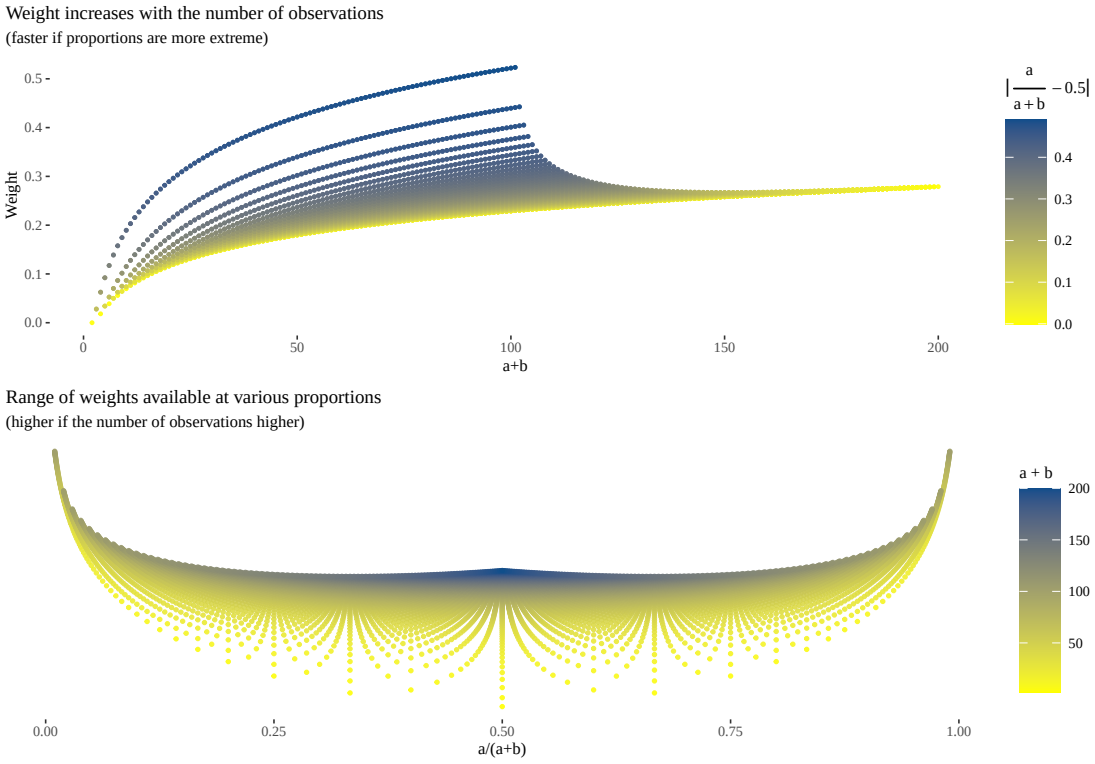


Figure 13: Weights of beta likelihoods for a, b ranging from 0 to 100, versus the number of observations and versus absolute distance of the proportion from .5.

Moreover, the framework is capable of sensible comparison of weights for distributions of various shapes, including those involving all weights focused on a particular point (strictly speaking, a single bin in the grid approximation). Here are some examples of shapes worth looking at (Figure 14).

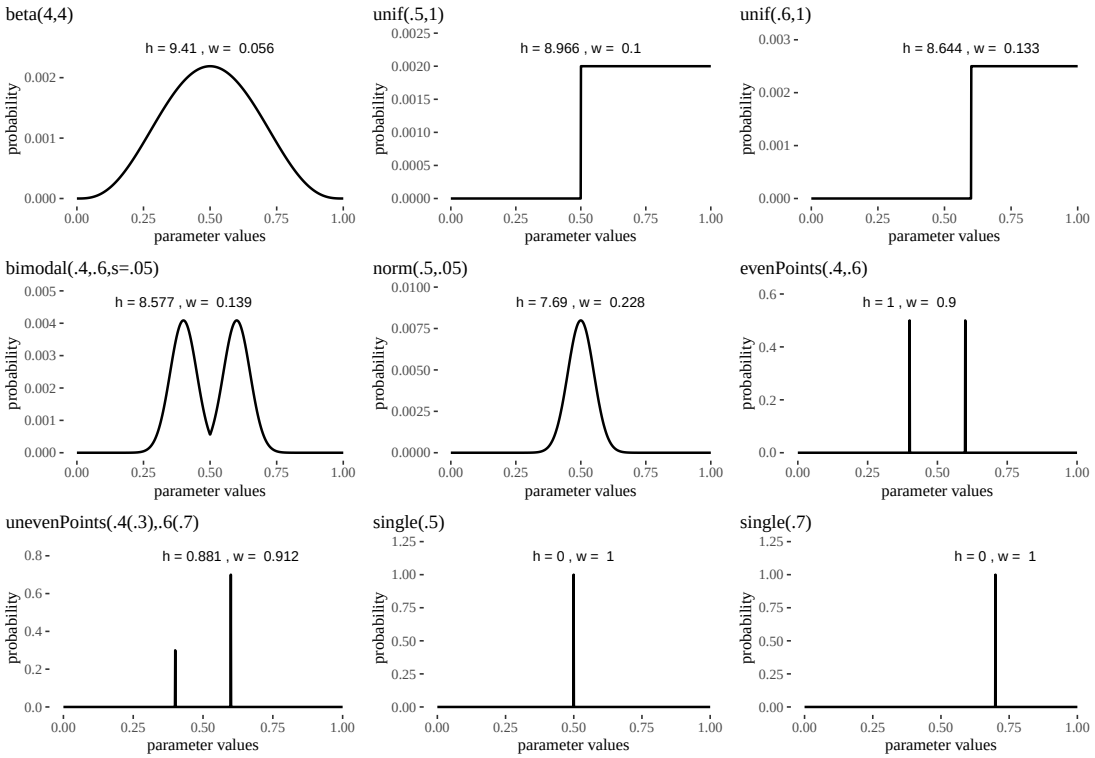


Figure 14: Examples of various distributions with their entropies and weights, ordered by weights. (1) beta(4,4), (2) uniform starting from .5 to 1, (3), uniform strating from .6 to 1, (4) two normal distributions centered around .4 and .6 with standard deviation .05, glued at .5. (5) normal centered around .5 with the same standard deviation, (6) one that assigns .5 to each of .4 and .6, (7) One that assigns .3 to .4 and .7 to .6., (8) one that assigns all weight to .5, and (9) one that assigns all weight to .7.

Note that the ordering of weights is as expected. Partial uniform likelihoods which exclude at least half of parameter values have more weight than a weak beta, and the weight increases as the non-zero interval of the partial uniform distribution decreases. A bimodal normal distribution "glued" from two normal distributions carries less weight than a unimodal normal distribution with the same standard deviation centered around the mean of the two modes, all these are way below point estimates. If multiple points have non-zero probability, the weight depends on how uneven the distribution is, whereas if full weight is given to a single point, the value of the parameter is known, the weight is maximal (=1) and does not depend on what the parameter is.

Comment about the desiderata listed in the beginning

12 Higher order probability and weight in BNs

Consider the (simplified) BN developed by CITE FENTON to illustrate how conviction was unjustified in the Sally Clark case (Figure 15).

The point to be illustrated was that with a sensible choice of probabilities for the conditional probability tables in the BN, conviction was not justified at any of the major stages (Table 16).

One reason the reader might worry is that the choice of the probabilities is fairly specific, and it is not obvious where such precise values should come from. The usual response REFS FOR SENSITIVITY ANALYSIS is that a range of such selections should be tested, perhaps with special focus on extreme but still plausible values. This approach, while resulting in more robustness, shares some of its difficulties with imprecise probabilism.

- Different probability measures are not distinguished in terms of their plausibility, and so this plausibility is not accounted for in the analysis.
- If you were worried that the precise choice of one probability measure is unjustified, you might

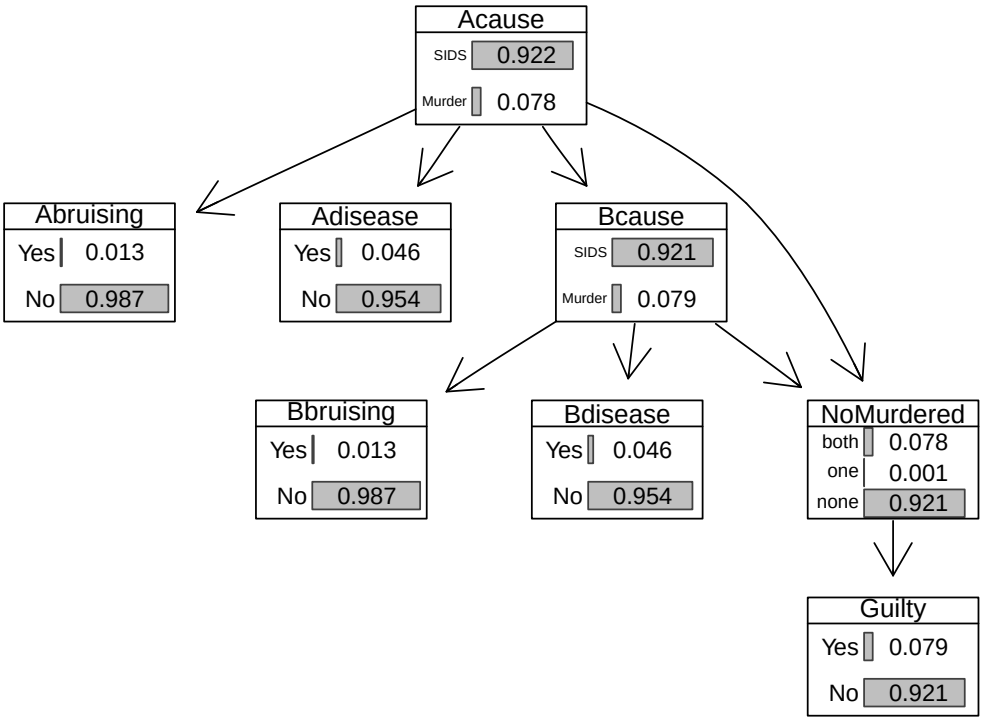


Figure 15: The BN developed by FENTON ET AL., with marginal prior probabilities.

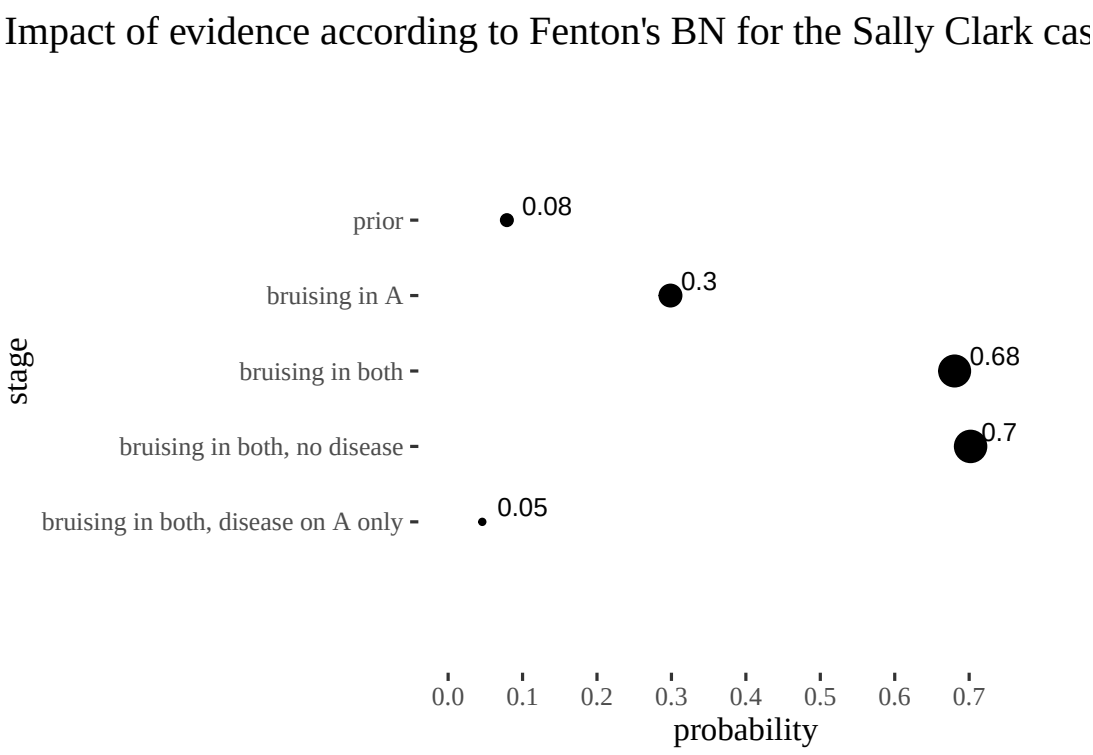


Figure 16: The prior and posterior probabilities for Fenton's Sally Clark BN.

have a similar concern about the precise choice of the few extreme combinations relied on in the sensitivity analysis.

- If in the sensitivity analysis the further decision is guided by the results for the extreme measures, they might be play an undeservedly strong role. [STORY ABOUT MAKING DAILY DECISION THIS WAY TO ILLUSTRATE]

Some of these concerns are at least dampened when we deploy the higher order probabilities in the BN. For instance, your conditional probabilities might look as illustrated in Figure 17.

describe the sampling approach

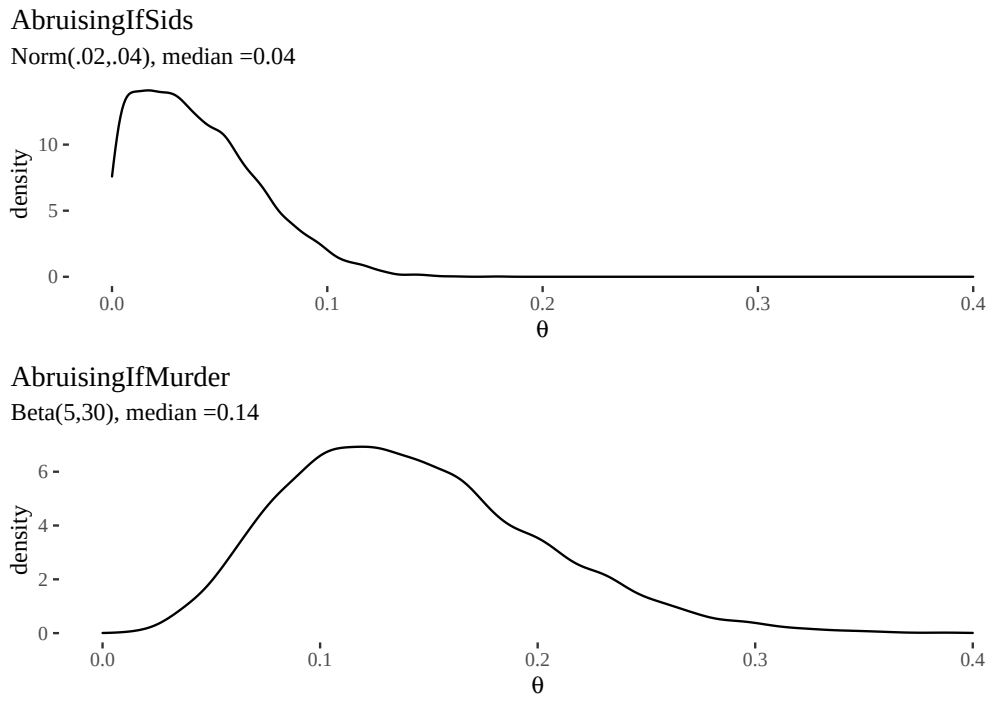


Figure 17: Example of approxmiated uncertainties about conditional probabilities in the Sally Clark case.

Using these we can investigate the impact of incoming evidence as it arrives (Figure 19)
And we can use this tool to gauge our uncertainty about the likelihood ratios (Figure 20).

Do we still call them BNs? What do we call them?

13 Weight of evidence

So far we have discussed the weight of a distribution, meant to measure how informed an agent is about an issue. If the agent starts with a uniform prior, this is a good enough approximation of how informed the evidence made them. But in general, how much more information is obtained is context-dependent. We want a prior-relative notion of weight, following the intuition that weight consideration should guide our information gathering also in making us stop collecting further evidence in light of what we already know. But for weight of evidence to have this feature, it has to depend on what we already know.

So here is a general recipe. In a given context, consider your distribution for the target hypothesis H given what you already know. Then update on the evidence. This might increase the weight for H, if the evidence confirms your conviction, or decrease it, if it goes against what the previous evidence tells you. Take the difference between the prior weight and the posterior weight (Δw) as your measure of the weight of evidence in that context. If you prefer to think that weight of evidence should be always positive, you might prefer the absolute value thereof. We, however, prefer to keep track of whether the evidence makes you more or less confused. The calculation goes along the following schema:

1. Start with a prior distribution over the parameter space of interest, and with distributions expressing the agent's uncertainty about other probabilities involved in the calculation of the posterior.
2. Sample from these distributions.
3. For each sample, treat it as a selection of precise probabilities, apply Bayes' theorem to calculate the posterior.
4. The set of the results is the sampling distribution expressing your posterior uncertainty.

For instance, suppose that you learn that if a child has been the victim of abuse, your prior given everything else you know is $\texttt{beta}(2,4)$ with median at .31, the conditional probability that they will have the habit of rocking if they have been a victim has a point estimate of .3. How strong is the evidence when you observe a given child rocks? Well, this depends on how probable rocking is given that the child has not been abused. This is the lesson that we learned in the chapter on likelihood ratio. So first, consider two scenarios, in which this conditional probability also receives a point estimate. First, .2, then .05. Finally, with conditional probabilities also being uncertain, say $\mathsf{P}(E|A)$ coming from normal distribution with $\mu = .3, \sigma = .1$, truncated to $(0,1)$, and $\mathsf{P}(E|\neg A)$ coming from normal distribution with $\mu = .05$ and $\sigma = .05$, corresponding to the idea that the sample of non-abused children is much larger, and the uncertainty about rocking about non-abused children is lower. The corresponding shifts from the prior to the posteriors (with sample size $1e7$) are pictured in Figure 21.

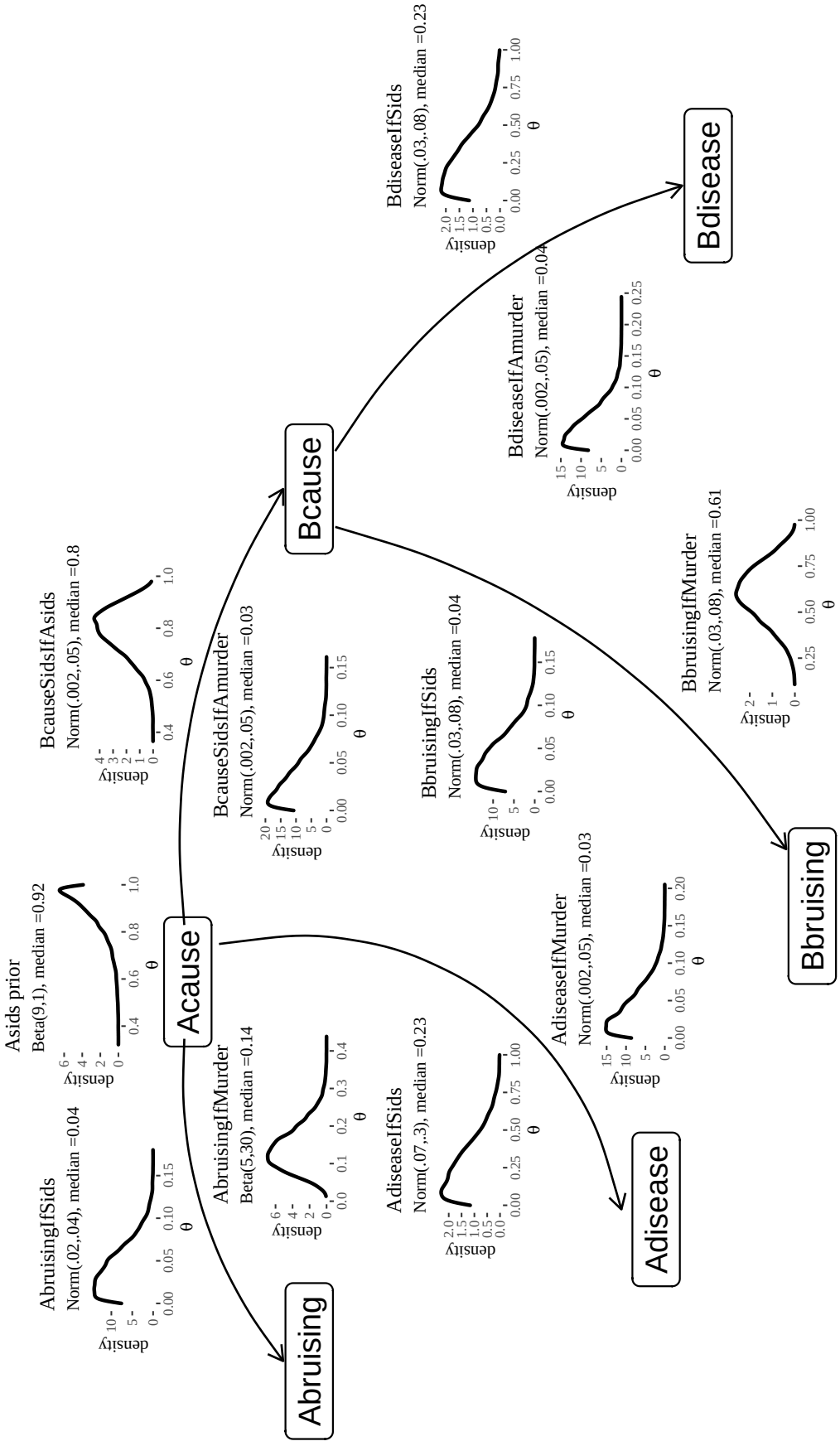


Figure 18: Example of a HOP approach tor the Sally Clark Case approximated by sampling probabilities and constructing 10k BNs.

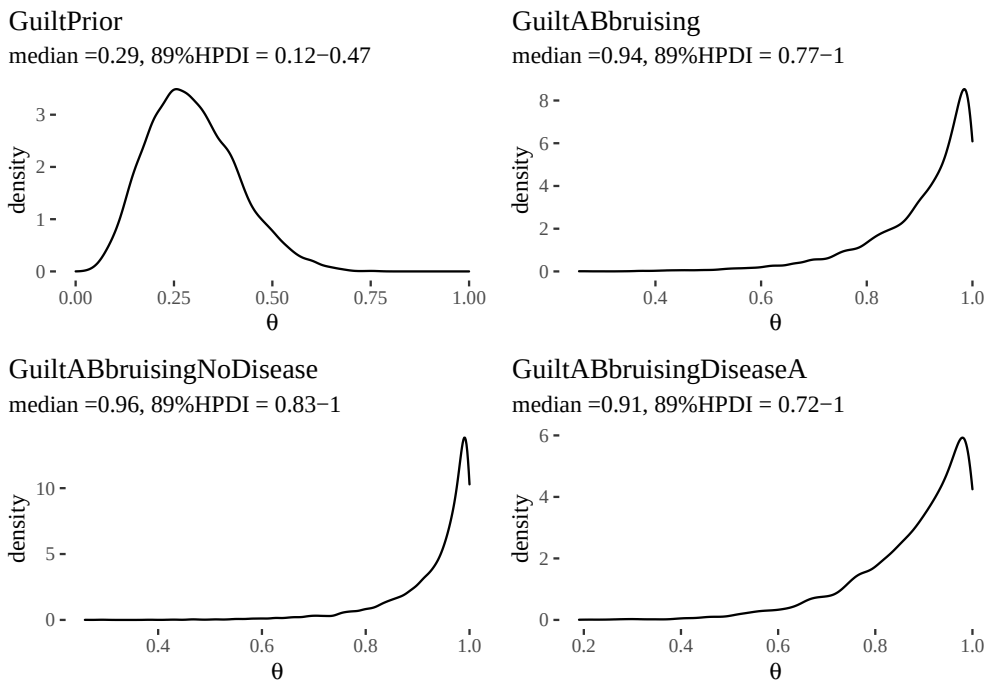


Figure 19: Impact of incoming evidence in the Sally Clark case.

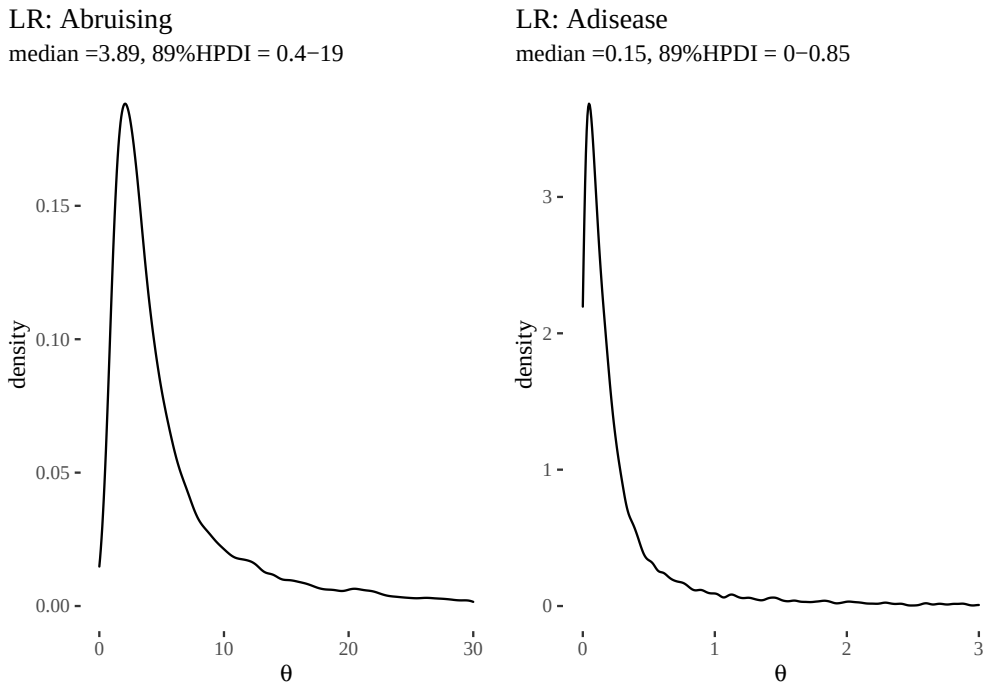


Figure 20: Likelihood ratios forbruising and signs of disease in child A in the Sally Clark case.

Prior and posteriors in the rocking example
(with weights and weight shifts)

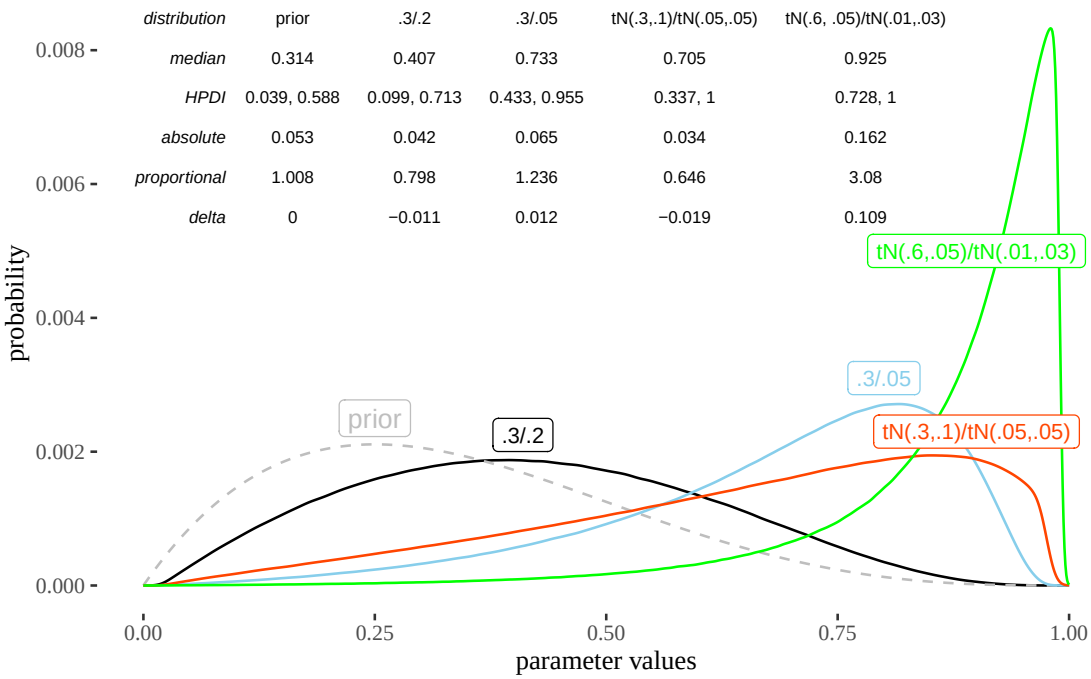


Figure 21: Shifts from prior $\texttt{beta}(2, 4)$ to the posteriors, given various conditional probabilities.

tried to find stats on rocking in abused vs non-abused children, failed

14 Weights in Bayesian Networks

What results do we get when we apply the weight calculations to the guilt node in the Sally Clark BNs?

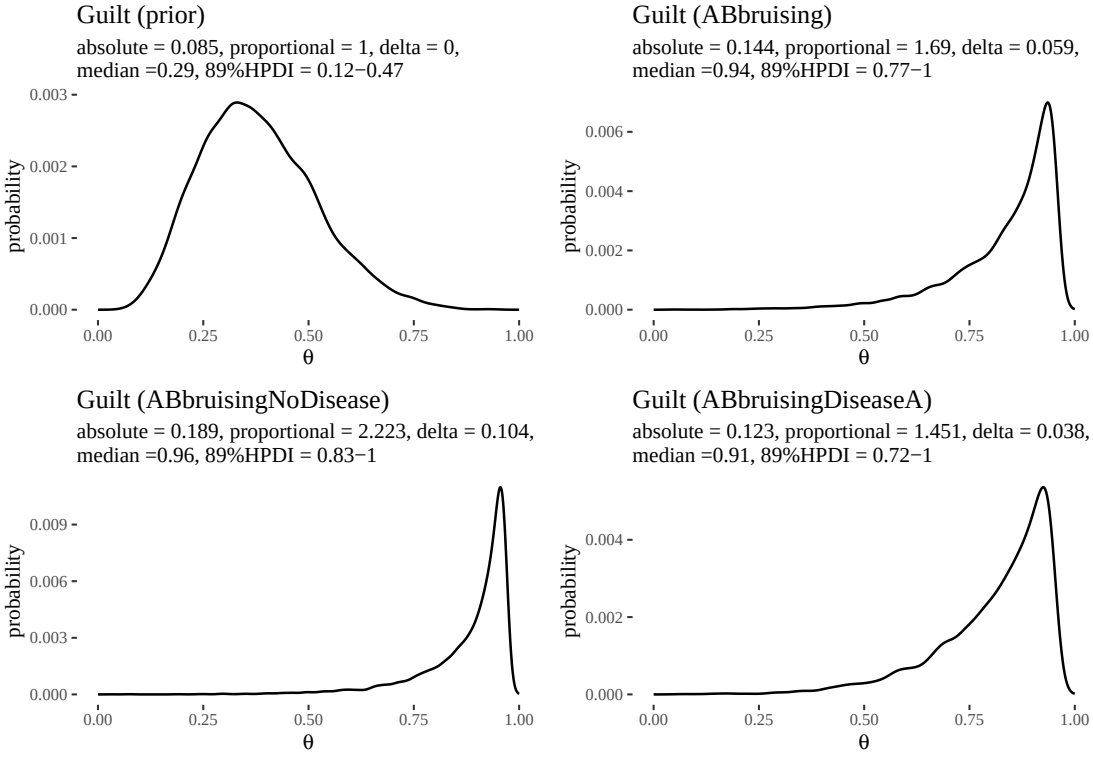
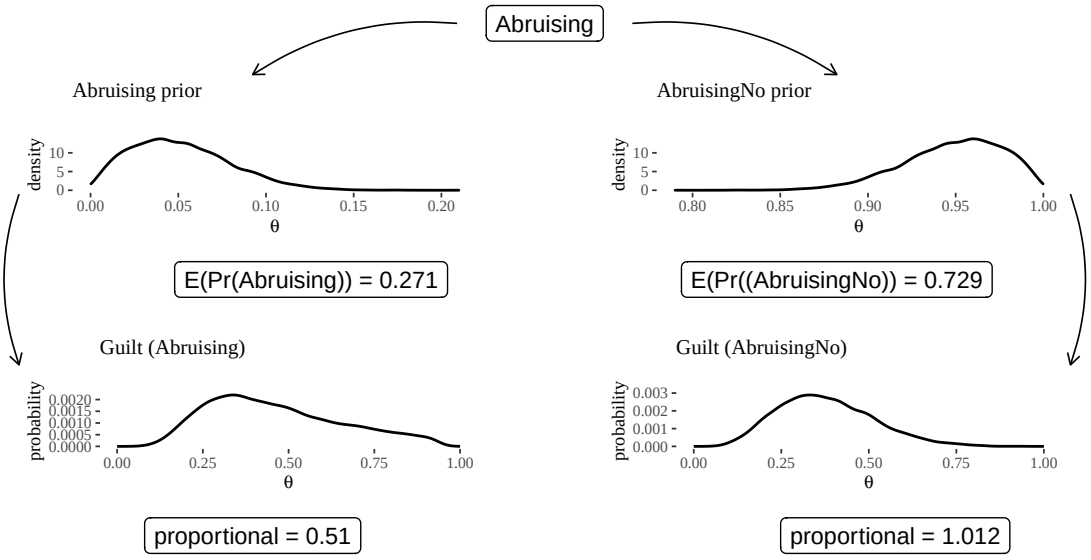


Figure 22: sds

One use of weight is to use expected weight of still missing pieces of evidence to guide evidence gathering, especially with limited resources. Since we don't want to possible impacts of evidence to cancel each other, we use absolute distance of proportional change in weight from 1 in the expectancy calculations and in looking at possible outcomes, to which we should also be sensitive (Figure **??**).

14.1 Expected weight



	abruising	adisease
E(Pr((TRUE))	0.271	0.282
proportional (absolute value) if TRUE	0.49	0.508
E(Pr(FALSE))	0.729	0.718
proportional (absolute value) if FALSE	0.012	0.222
E(|proportional|)	0.141	0.303

??

Figure 23: Expected size (absolute value) of the proportional weight change calculations. Illustrated for bruising in child A, calculated for bruising and signs of disease in child A.

15 Completeness tends to improve weight

16 Accuracy

Here is a question asked by [COHEN 1986 TWELVE p. 276]: is it worth while knowing the weight of an argument without knowing its probability? In our terminology, questions inspired by Cohen's are: what's the point of weight considerations if we already have the distributions? Can weights be put to use if we do not have the distributions?

First, we need to explain how accuracy is to be measured in this framework. We already know what accuracy measures are on the market for single probability measures, and we know that proper scoring rules for representors are not forthcoming. What is the situation with the higher-order approach that we propose? The idea is conceptually straightforward. We take the inaccuracy of a (discretized) distribution p from a given true chance/single rational probability hypothesis by taking the Kulback-Leibler divergence from the indicator distribution of this hypothesis to p.

More formally, take probability distribution p over a grid-approximated parameter space, assigning probabilities $p_1,\ldots,p_n$ to $\theta_1,\ldots,\theta_n$ respectively. It is to be evaluated in terms of inaccuracy from the perspective of a given "true' ' value θ_k.[15] The inaccuracy of p if θ_k is the "true" value, is the divergence between $IndI^k$ and p. [16]

$$\begin{aligned}\mathcal{I}^2_{\mathsf{DKL}}(p,\theta_k) &= \mathsf{DKL}(Ind^k, p)\\ &= \sum_{i=1}^{n} Ind_i^k \left(\log_2 Ind_i^k - \log_2 p_i\right)\end{aligned}$$

It has been proven [CITE AUTHOR'S PAPER] that the resulting measure of inaccuracy is a proper scoring rule. Another interesting feature of the framework is that the point made by Schoenfield against IP does not apply here: there are cases in which accuracy considerations recommend an imprecise stance (that is, a multi-modal) distribution over a precise one. Here is a quick example. Suppose the opponent will produce two coins, one with the distribution of Heads either normal around .3, and one normal around .5, both with the standard deviation of .05, randomly pick one of these coins and then toss it. The agent knows the setup. Consider the following three (out of many) possible stances that the agent could take:

[15] Two caveats: if you prefer to think of evaluation with respect to truth and falsehood only, the only "true values" to be used are 0 and 1. If on the other hand, you think the true distribution does not have to be an indicator distribution assigning value 1 to exactly one hypothesis, evaluate the Kullback-Leibler distance from this more complex distribution. Such decisions can be accommodated in our framework.

[16] Another option is continuous ranked probability score (CRPS) of a distribution p with respect to a possible world w:

$$I(p,w) = \int_{-\infty}^{\infty} |\mathsf{P}(x) - \mathbf{1}(x \geq V(w))|^2 \, dx$$

where P is the cumulative probability corresponding to a given density, and

$$\mathbf{1}(x \geq V(w)) = \begin{cases} 1 & \text{if } x \geq V(w) \\ 0 & \text{o/w.} \end{cases}$$

The intuition here is that the measure takes the Cramer-Von-Mises measure of distance between densities, defined in terms of the area under the squared euclidean distances between the corresponding cumulative density functions:

$$\mathcal{C}(p,q) = \int_0^1 |P(x) - Q(x)|^2 \, dx$$

and uses it to measure distance to an epistemically omniscient chance hypothesis, which either puts full weight on 0, if a given proposition is false, or on 1, otherwise. We do not use this measure, as it is more complicated than necessary, uses square distances instead of information-theoretic notions which makes it somewhat more arbitary (and we have seen how squaring played out in our discussion of Joyce's proposal), and because it has unintuitive features when it comes no multi-modal distributions (SEE AUTHOR'S PAPER FOR DETAILS)

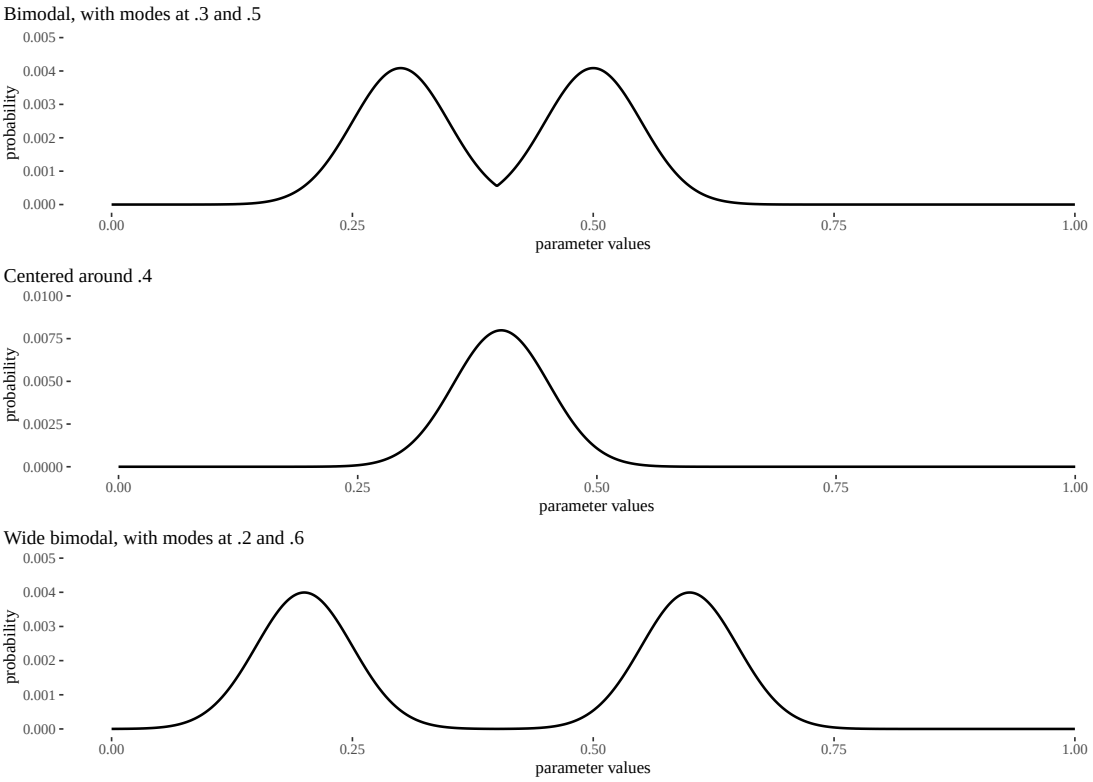


Figure 24: Three (out of many) candidate distributions for a Schoenfield-inspired example. All distributions are built from normal distributions with standard deviation .5, the bimodal ones are "glued" in the middle.

For the three distributions we're discussing in this chapter, the inaccuracies calculated using CRPS and KL divergence with respect to various potential true probability distributions look as in Figure 25.

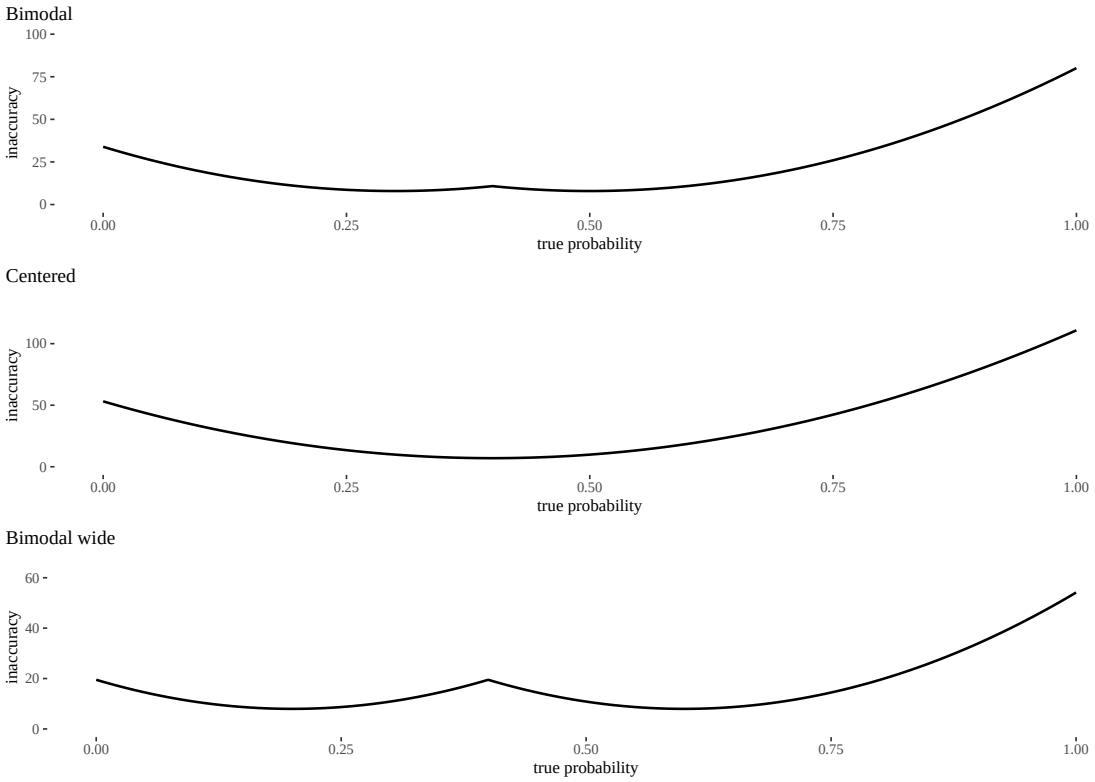


Figure 25: KL divergence inaccuracies vs (omniscient functions corresponding to) n true probability hypotheses for the three distributions discussed in this section.

17 Weight and accuracy

18 Literature to discuss

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Good, weight of evidence, survey

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