This is a summary of the major revisions recommended by the reviewers. I will address them point by point. There are three reviews, which I will call A, B, and C. I am addressing the comments in the sequence of the paper.

A1 is the reviewer's overall assessment.

A2 I changed the introduction to introduce "probability kinematics" and Richard Jeffrey's book which first explained the concept and the need for it.

> Jeffrey conditioning is a method of update (recommended first by Richard Jeffrey in [8]) which generalizes standard conditioning and operates in probability kinematics where evidence is uncertain ($P(E)\neq{}1$).

A3 Partially, this recommendation was due to a missing section header, "Jeffrey's Updating Principle and the Principle of Maximum Entropy." I inserted it. Now the introduction reads as it should and concludes with a brief summary of sections to come.

A4 and **A6** I have completely changed this passage (now the end of section 3). The informal language is gone, and the formula for Jeffrey conditioning has been clarified with an example, especially the matrix notation in what is now equation (2).

A5 I was very impressed that the reviewer caught this mistake: the summation parameters in (2) needed to be fixed, which I did.

A7 I have inserted a paragraph explaining in more detail the distinction between synchronicity and diachronicity with the appropriate references to the literature.

> It is important to distinguish between type I and type II prior probabilities. The former precede any information at all (so-called ignorance priors). The latter are simply prior relative to posterior probabilities in probability kinematics. They may themselves be posterior probabilities with respect to an earlier instance of probability kinematics. Although Jaynes' original claims are concerned with type I prior probabilities, this paper works on the assumptions of Jaynes' later work focusing on type II prior probabilities. Some distinguish between MAXENT, the synchronic rule, and Infomin, the diachronic rule. The understanding here is that both operate on type II prior probabilities: MAXENT considers uniform prior probabilities (however this uniformity may have arisen) and a set of synchronic constraints on them; Infomin, in a more standard sense of updating, considers type II prior probabilities that are not necessarily uniform and updates them given evidence represented as new (diachronic) constraints on acceptable posterior probability distributions. Some say that MAXENT and Infomin contradict each other, but I disagree and maintain that they are compatible. I will have to defer this problem to future work, but a core argument for compatibility is already accessible in [13].

A8 All references to a companion paper have been deleted. I inserted brief explanations intelligible in the context of this paper to replace the references. This change is related to **B1**.

A9 I didn't follow the reviewer's comment here. I checked everything, and I think my procedure is sound.

B3 is an interesting comment. I originally submitted this paper to BJPS, but the reviewer pointed out that I *needed* \omega_{5} (the fatal flaw in the original submission), which required an extensive rewrite of the whole paper. I agree with the BJPS reviewer. I think the way it stands right now is correct: \omega_{5} is the catch-all hypothesis and since it is eliminated by standard conditioning it no longer shows up in equation (8).

B2 Good point. I clarified this in the paper (kappa_{ij}=0 iff \hat{P}(\omega_i\cap\theta_{j})=0).

A10 I used the reviewer's suggestion verbatim.

B1 is an important point. I inserted the following paragraph to address it:

> The solution that Wagner attributes to PME is misleading because of Wagner's Dempsterian setup which does not take into account that proponents of PME are likely to be proponents of the classical Bayesian position that type II prior probabilities are specified and determinate once the agent attends to the events in question. Some Bayesians in the current discussion explicitly disavow this requirement for (possibly retrospective) determinacy (especially James Joyce in [8a] and other papers). Proponents of PME (a proper subset of Bayesians), however, are unlikely to follow Joyce---if they did not, they would indeed have to address Wagner's example to show that their allegiances to PME and to indeterminacy are compatible.

A11 I appreciate what the reviewer has to say here: I have worked with Bernardo's Reference Posterior Distributions before. I am afraid that introducing them would take us too far afield, so I have chosen not to make any changes here. Similar reasoning leads me not to include any reference to Veveakis and Regenauer-Lieb, as suggested by **C1**, and to Debbah, Muller, Palmieri, and Ciuonzo, as suggested by **A13**.

A12 I am following the advice of the reviewer here and have deleted the passage. The addition of an appendix makes a proof accessible to readers which is not rigorous but which avoids the technicality of equations (12)-(15).

B4 addresses two sources of confusion which I have fixed. **(a)** M is indeed an m times n matrix and the matrix in (5) is *not* M, but an expanded version of M with the marginals. I have clarified this in the introduction to (5). **(b)** \zeta was actually defined several paragraphs earlier, but the reviewer correctly complains that that's too far away for the reader to remember. I created the reference for the reader.