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Letter to the Editor

Are alternatives to Dempster's rule of combination real alternatives?

Comments on "About the belief function combination and the conflict management problem"—Lefevre et al

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In the previous issue of this journal, Lefevre et al.'s paper [1] raises the important question of how to combine belief functions from different sources and how to manage conflicts. They propose a parametrized combination rule that includes several existing combination rules as special cases. Their method is based on so-called *weighting factors* that determine how conflicting masses are to be distributed among the subsets of the corresponding frames of discernment. Since my understanding of Dempster–Shafer theory and my practical experience with belief function modelisation and corresponding computations is quite different, I do not support several points in their approach.

From a practical point of view, I have several major concerns. First of all, their method requires to specify actual weighting factors for every combination operator. This may be feasible for simple problems where the corresponding model consists of a few belief functions only. However, in cases where many belief functions are to be combined (several hundreds or thousands are common in many practical cases), the problem of specifying weighting factors becomes completely impracticable. Second, weighting factors are specified over a family of subsets of the combined frame of discernment. This is fine as long as the combined frame of discernment is relatively small, but since a frame of discernment has exponentially many subsets, it gets very difficult and expensive as soon as the frame has a certain size. In practice, it is furthermore common to have multi-variate frames of discernment, where there are not only exponentially many possible subsets, but where the sizes of the frames themselves depend exponentially on the number of variables involved. I do not see a practical way of assigning weighting factors in such cases of double exponentially many subsets. Finally, as the authors admit in their paper, their general operator is not associative. This is highly problematical, especially in a multi-variate setting, where belief function computations on the basis of Dempster's rule properly fit into Shenoy's axiomatic framework of *valuation networks* [2–5]. Associativity is one of the basic axioms that finally allow local computations. In cases where many multi-variate belief functions are given (hundreds or thousands), local computation is crucial because it tremendously reduces the complexity of the computations from NP-complete with respect to the number of belief functions involved down to NP-complete with respect to the largest domain of the underlying join tree. I do not see how to treat such complex cases without an associative combination operator.

From a philosophical point of view, I do not agree with the idea of introducing other combination operators besides Dempster's rule. I think it's not justified to simply say: "Dempster's rule of combination sometimes leads to counter-intuitive results, thus other rules are necessary". This corresponds to saying: "model X with method Y produces counter-intuitive results Z, therefore method Y is not correct". Possibly, but method Y may just as well be correct and model X is wrong (or not complete). I have seen so many at first sight counter-intuitive examples of Dempster's rule, but each case quickly turned out to be a problem of an incorrect or incomplete modelisation. More formally, let m_1 and m_2 be two mass functions and $m_1 \otimes m_2$ their combination obtained from Dempster's rule. If the result $m_1 \otimes m_2$ is counter-intuitive, then instead of replacing $\otimes$ by an alternative operator $\otimes'$, I propose to refine the model given by m_1 and

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m_2 into m'_1 and m'_2 and to compute $m'_1 \otimes m'_2$, again using Dempster's rule. For example, consider the case of unreliable sources as discussed in their paper. Unreliable sources are one of the author's major arguments raised against Dempster's rule. In such a case, however, the method of *discounting coefficients* [6,7] produces *discounted* mass functions m'_1 and m'_2 and perfectly works without the necessity of changing Dempster's rule. The conflicting mass resulting from combining two or more such unreliable sources reflects then a natural phenomenon that arises when statements from different sources are combined. Under the closed-world assumption, i.e. if one particular element of the frame of discernment (usually a vector of values) is supposed to represent the true state of the world, I consider normalization as a reasonable and necessary procedure in the sense of Sherlock Holmes' statement in Sir Arthur Conan Doyle's "*The Sign of Four*" (1890): "When you have eliminated the impossible, whatever remains, however improbable, must be the truth". In fact, without additional information, I do not see any plausible reason for not distributing the conflicting mass proportionally among the remaining subsets. Furthermore, I consider the open-world assumption of Smets' combination rule [8] as questionable, because non-exhaustive frames of discernment mean nothing else than other cases of incomplete modelisation. Note that every frame of discernment can easily be extended by an additional element that covers all other possible states of the world.

Finally, from a mathematical point of view, Dempster–Shafer theory (on the basis of Dempster's rule of combination) is a very simple and straightforward way of extending classical probability theory. Despite its success as a well-founded and general model of human reasoning under uncertainty, belief functions are rarely used in practice. One of the most significant arguments raised against using belief functions in practice is their relatively high computational complexity, especially in comparison with methods based on classical probability theory. Since the paper proposes a family of combination operators, the simplicity of the theory is somehow lost. Furthermore, since non-associativity prevents their method from being used in a framework of local computations, the complexity problems gets even worse. Thus, I consider the idea of having more than one combination rule as counterproductive, making it even harder for the theory to get more attention and increasing popularity.

My perspective on Dempster–Shafer theory is strongly influenced by Kohlas' and Monney's *Theory of Hints* [9] and by work on *probabilistic argumentation systems* [10–12]. A hint describes the masses of a belief function as the probabilities of different *interpretations*. Hints are thus semantically richer than belief functions. Note that the combination of two hints is justified by the laws of probability theory and turns out to be Dempster's rule if the hints are reduced to belief functions. Even more general are probabilistic argumentation systems. They result from a simple combination of classical logic with probability theory. From a qualitative point of view, probabilistic argumentation systems provide arguments and counter-arguments for the given hypothesis of interest. By considering the (conditional) probabilities that the hypothesis is supported or refuted by arguments (given that the true world is not in conflict with the knowledge base), one gets two numerical measures called *degree of support* and *degree of possibility*. If probabilistic argumentation systems are translated into a number of belief functions [13–15], then degree of support and possibility correspond to belief and plausibility, respectively, and the conjunction of two independent parts of the knowledge base again turns out to be Dempster's rule. Furthermore, by looking at the technique of discounting coefficients from the point of view of probabilistic argumentation systems, one gets a clear semantical understanding in which every discounting coefficient is interpreted as the probability of the source's reliability. Finally, probabilistic argumentation systems even allow a clear and precise modelisation of information from sources that are not independent (e.g. the case covered by Smets' disjunctive combination rule). Thus, I consider probabilistic argumentation systems as a convenient and powerful modeling language to be put on top of Dempster–Shafer theory (see [16] for examples), and because qualitative computations are usually quite expensive, I propose Dempster–Shafer theory to be used as an efficient computational tool for probabilistic argumentation systems. Notice that the close connection between belief functions, hints, and probabilistic argumentation systems is immediately lost as soon as Dempster's rule is replaced by something else.

To summarize, I think combination rules other than Dempster's rule are at the same time impracticable, unnecessary, somehow confusing, and thus counterproductive for the future success of Dempster–Shafer theory as a theoretical and practical model for reasoning under uncertainty. As a consequence, I consider the research discussed in Lefevre et al.'s paper as well as other similar approaches as inappropriate. In my eyes, the true challenges of Dempster–Shafer theory are to find more efficient computational methods (in comparison with other numerical approaches to uncertainty management) and to investigate more convincing practical applications.

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