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Letter to the Editor: Commentary on "Dennis McNevin, Bayesian interpretation of discrete class characteristics, Forensic Science International, 292 (2018) 125–130"

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Abstract

This letter to the Editor comments on the paper 'Bayesian interpretation of discrete class characteristics' (Forensic Science International, 292 (2018) 125–130) by Dennis McNevin.

Dear Editor,

In his recent paper 'Bayesian interpretation of discrete class characteristics', Dennis McNevin expresses concerns about the limited attention that prior probabilities about source level hypotheses receive. As a consequence of this, the author argues, posterior probabilities are "largely unknown" [8, at p. 125]. Based on an analogy with a deck of cards, McNevin proposes a way to assign prior probabilities that is based on "the total number of things (people, tools, weapons, cars, *etc*) equally capable of being the source of crime scene evidence" [8, at p. 126]. As a secondary result, the author derives a criterion to determine when a particular item of evidence is "favorable to the prosecution" [8, at p. 129]. Specifically, he defines evidence to be favorable to the prosecution if the resulting posterior odds is greater than one. Although we agree that it is important that we provide the court with means to integrate the value of forensic results in a case, the suggested proposals raise a series of issues on which we comment below. Some of these issues have a long and controversial history in forensic science and the law.

1. The analogy with cards. – Analogies are risky and the comparison of aspects of a criminal case with a card game leads to difficulties here. Indeed, in a card game, there are some assumptions that commonly hold (e.g., known number of cards, random draw) but that cannot be justified in the context of a criminal case. We have no difficulty in saying that our (prior) probability of drawing a given card in a deck of N cards, would be $1/N$. However, this does not hold in a forensic setting. In a criminal case, if the person of interest is brought to trial, there will have been an investigation of some sort. Leaving aside special cases of governmental misconduct, the person will *not* have been 'randomly' chosen, and there will be information in the case (forensic and non-forensic) that have led to an arrest. This information will not be known to the scientist because it is for the court to determine what is the probability that the suspect is, for example, the source of blood recovered on the crime scene.

2. The unnecessary attempts of scientists to opine on prior probabilities. – The topic of prior probabilities has stirred up controversies in the past both in forensic science and (criminal) law in general. In the mid 2000s, for example, the Expert Working Group Marks Conclusion Scale Committee of the European Network of Forensic Science Institutes (ENFSI) asserted that scientists "must make scientific estimations of the prior odds" [4, at p. 262]. This recommendation instructed scientists to consider equal prior probabilities by referring to principles such as

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Maximum Entropy.[1] This bears similarity with McNevin's current suggestion of considering the size of the suspect population and each member in this group as having the same prior probability of being the source of the evidence (e.g., questioned mark, impression, stain or material found on a crime scene). There are multiple problems with this suggestion:

- First and foremost, thinking in terms of the size of the suspect population is prone to encounter objections similar to those faced with (the demon of) the so-called reference class problem: how is one going to define and agree upon the size of the suspect population? This question faces numerous angles of attack, which may make it attractive for the defence, but in essence the question distracts from the main issues (on which we will further elaborate below).

- Second, the suggested procedure views a prior probability as the sole consideration of a single factor (i.e., the size of the suspect population) and the assumption of indifference, leading to a uniform prior probability distribution. This is a suggestion in the void, asking evaluators to make abstraction of whatever other evidence and case specific assessments that may be available, but that would be of value for forming meaningful prior probabilities of source-level propositions. In this sense, such probabilities will have an artificial flavour, and so will have the resulting posterior probabilities.[2] The common objection to this is to say that the uniform prior represents a 'neutral' starting point, but this is not the case. Howson, for example, notes that "(...) a uniform prior is just as informative as any other, or as misinformative" [7, at p. 55]. Similarly, de Finetti noted:

  > "The belief that the a priori probabilities are distributed uniformly is a well defined opinion and is just as specific as the belief that these probabilities are distributed in any other perfectly specified manner. Accordingly, there is no reason why the absence of an opinion on the distribution of the a priori probabilities should be taken as equivalent to the opinion that their distribution is of this or that specified form." [3, at p. 382]

  Further, the argument that one is a priori 'ignorant', but that invoking an abstract principle (such as the principle of indifference) could automatically transform such ignorance into knowledge (because any prior probability is a particular expression of partial knowledge), has been rejected as a claim of "epistemological magic" [9, at p. 66].

3. The value of the evidence. – McNevin defines evidence as favourable to the prosecution as evidence that is capable "to produce a posterior ratio greater than one" [8, at p. 129]. This is confusing because it conflates the notion of likelihood ratio, conventionally considered as the expression of the value of the evidence, with beliefs about the competing propositions. It goes without saying that there is no requirement whatsoever with regards to posterior odds in order for evidence to be probative: all that is required is that the posterior odds are different from the prior odds, in one sense or another, or – alternatively – that likelihood ratios are different from one. McNevin's assertions are problematic because they suggest that probative value requires assumptions about prior odds. This bears the risk of some likelihood ratios to be suggested as of limited use. McNevin's example of a likelihood ratio of 10^8 and an assumption of prior odds assessed based on the world population is particularly unsuitable in this sense. First, any likelihood ratio different from one expresses probative value, especially a likelihood ratio of 10^8. Second, the stereotype discussions about the population of potential sources framed in terms of the world population are besides the point, in forensic DNA as much as with fingerprints (which is another area mentioned by McNevin). What is more, if likelihood ratios on the order of 10^8 are suggested to be of limited help, this reflects negatively on other types of evidence, such as fibre, glass or handwriting where moderate likelihood ratios of up to a few hundred are most commonly obtained. In cases with such evidence, it would suffice for the defence to suggest that the appropriate size of the population of potential sources is the entire world – however far this may be from the actual case circumstances – for evidence to be said 'unhelpful', *despite* a likelihood ratio greater than one. This, of course, would be a serious misconstruction. One may object that McNevin's notion of 'favourable' does not mean 'probative'. But still, these assertions are unclear

[1] For a detailed analysis and rejection of these ideas see, e.g., [1].

[2] As an aside, note that the scientist should not provide posterior odds because they are unsuitable for the joint evaluation of multiple items of evidence [e.g., 10].

and bear a risk of unsound conclusions of the kind we mention above.

But what are 'the prior odds', if not those suggested by $1/N$? Our answers to this are, first, that prior odds are simply not the business of the forensic scientist. The scientist should not invade the area of competence of the recipients of expert information. Scientists should only help them revise their opinions, whatever those opinions are prior to considering the scientific evidence. As noted by de Finetti in the wider context of statistical experts: "(...) we do not advocate that the personal opinions of the statistician be foisted on the scientific community, the business executive, or other "clients" of the statistician. Rather, the role of the statistician is to help his clients mature and co-ordinate their own opinions." [2, at p. 144] Second, we do not even suggest that recipients of expert information actually do mathematically operate at the level of technicality and precision that a numerical likelihood ratio may require. At best, what we should hope for is that recipients of expert information capture the idea that evidence favours one proposition rather than a given alternative, within an order of magnitude that is given by the likelihood ratio,[3] though there are challenges to this [11]. Third, even if we indeed wished to frame the prior probabilities for source-level propositions more formally, there are ways to do so by logically connecting them to other assessments made in the case at hand (see, e.g., [1] for a development and examples). According to this view, prior odds are not just a one-off assessment based on a single consideration (such as the size of the relevant population), but can be coherently related to other case-specific assessments, such as the relevance of the evidence (i.e., the probability that the trace or stain comes from the offender). The basis of this argument are likelihood ratio formulae for higher level proposition [e.g., 5], beyond source level, that more closely suit the needs of recipients of expert information. Most interestingly, these developments make explicit assignments of prior probabilities for source-level propositions entirely superfluous.

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References

[1] A Biedermann, F Taroni, and P Garbolino. Equal prior probabilities: can one do any better? *Forensic Science International*, 172:85–93, 2007.

[2] B de Finetti. *Probability, Induction and Statistics, The Art of Guessing*. John Wiley & Sons, New York, 1972.

[3] B de Finetti. Recent suggestions for the reconciliation of theories of probability (Paper originally published in the "Proceedings of the Second Berkeley Symposium on Mathematical Statistics and Probability", held from July 31 to August 12, 1950, University of California Press, 1951, pp. 217-225). In B de Finetti, editor, *Probabilità e induzione*, Biblioteca di Statistica, pages 375–387. Cooperativa Libraria Universitaria Editrice Bologna, Bologna, 1993.

[4] ENFSI Expert Working Group Marks Conclusion Scale Committee. Conclusion scale for shoeprint and toolmarks examinations. *Journal of Forensic Identification*, 56:255–280, 2006.

[5] I W Evett. Establishing the evidential value of a small quantity of material found at a crime scene. *Journal of the Forensic Science Society*, 33:83–86, 1993.

[6] R D Friedman. The persistence of the probabilistic perspective. *Seton Hall Review*, in press:1–12, 2018.

[7] C Howson. Bayesianism in statistics. In R Swinburne, editor, *Bayes's Theorem*, Proceedings of the British Academy, pages 39–69, Oxford, 2002. Oxford University Press.

[8] D McNevin. Bayesian interpretation of discrete class characteristics. *Forensic Science International*, 292:125–130, 2018.

[9] W C Salmon. *The Foundations of Scientific Inference*. University of Pittsburgh Press, Pittsburgh, PA, 1966.

[10] F Taroni and A Biedermann. Inadequacies of posterior probabilities for the assessment of scientific evidence. *Law, Probability and Risk*, 4: 89–114, 2005.

[11] W C Thompson, R H Grady, E Lai, and H S Stern. Perceived strength of forensic scientists' reporting statements about source conclusions. *Law, Probability and Risk*, 17:133–155, 2018.

[3] On the issue of numbers, see, e.g., Friedman: "(...) I have avoided using any numbers, because they are not necessary and courts do not ordinarily use them either." [6]