Letters to the Editor

WARDELL, D. G. (1997), "SMALL-SAMPLE INTERVAL ESTIMATION OF BERNOULLI AND POISSON PARAMETERS," *THE AMERICAN STATISTICIAN*, 51, 321–325: COMMENT BY BERGER AND COUTANT, AND REPLY

Wardell (1997) considered confidence intervals for Bernoulli and Poisson parameters. He claimed to "give a method for finding the confidence interval with the minimum interval width that gives the exact desired confidence level." Unfortunately, Wardell's method does not achieve this goal. Wardell's method does not ensure that the coverage probability is bounded below by the specified confidence level.

Consider a Bernoulli example with sample size $n = 5$ and confidence level CL = .95. Solving the optimization problem (3a–d) yields these intervals for the six possible values, t_0, of the binomial random variable. In the following table, θ_L and θ_H denote the lower and upper endpoints of the confidence interval, respectively.

t_0	0	1	2	3	4	5
θ_L	.000	.000	.034	.173	.343	.549
θ_H	.451	.657	.827	.966	1.000	1.000

At $\theta = 1/2$ the coverage probability of this method is

$$\begin{aligned} \Pr(\theta_L \le 1/2 \le \theta_H) &= \Pr(T = 1, 2, 3, \quad \text{or} \quad 4) \\ &= .9375 < .95 \end{aligned}$$

Hence, the method does not guarantee that the coverage probability is at least .95. A closer examination of the coverage probability reveals that it is less than .95 for all θ in the interval (.451, .549).

The difficulty with Wardell's method is that, in solving (3a–d), p_1 and p_2 (defined in (1) and (2) of Wardell's article) are allowed to vary with t_0. Indeed, (3a–d) is solved, for a fixed value of t_0, by finding the values of p_1 and p_2 that satisfy the constraints and yield the shortest interval. But, in the usual justification of the "statistical method," as in Mood, Graybill, and Boes (1974), the values of p_1 and p_2 must be the same for all values of t_0. The usual choice is $p_1 = p_2 = \alpha/2$.

There is one ambiguity in Wardell's method. Equations (1) and (2) cannot be solved for $t_0 = 0$ or n. For $t_0 = 0$, (2) cannot be solved because $1 - F_T(0 - 1; \theta_L) = 1$ for all values of θ_L, but $p_2 \le \alpha < 1$. For $t_0 = 0$, θ_L must be zero. Otherwise the coverage probability will be near zero for θ values near zero. In the above table, the confidence interval for $t_0 = 0$ is found by ignoring (2), setting $\theta_L = 0$ and $p_1 = \alpha$, and solving (1) for θ_H. An analogous modification is needed for $t_0 = n$.

On another topic, Wardell noted that the intervals defined by the statistical method will be approximate if tables for only certain values of θ are used in solving (1) and (2). But, these solutions can be easily computed exactly, using standard percentiles from the F distribution, using the formula given in some elementary textbooks (e.g., Zar 1984, p. 378). Because the inverse F distribution is found in many computer programs, including Microsoft Excel, small sample confidence intervals for the Bernoulli parameter that do ensure the coverage probability is above the specified CL are easily computed without numeric optimization. However, they are not the shortest possible intervals. Shorter intervals, such as those described by Casella (1986), do require more computation.

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REPLY

I appreciate the opportunity to reply to the comments made by Berger and Coutant. Since I published my article on small-sample interval estimation I have noted that several people have been interested in the method that I proposed. I think that some found it intuitively appealing while others were interested because they could see problems with the claims that I made. Berger and Coutant are in the latter category. As they aptly point out, there are problems with the method that I proposed. Others have found the same deficiencies. Some, like Berger and Coutant, have pointed the problem out to me directly, others have chosen to show the problem to a wider audience. Another example of the latter case is Byrne and Kabaila (in press). I thank these researchers for pointing out the errors in my procedure.

I sincerely believed that the method that I developed did what I claimed. The graphical interpretation made it especially appealing to me. As is clearly pointed out in the letter to the editor, however, I did not consider coverage probability in the development of the work. That was a serious oversight on my part and on that of the reviewers. I am aware of an article soon to be submitted to *The American Statistician* that investigates in detail the coverage properties of the intervals derived by my method. As my critics have pointed out, the problem with my method is in the formulation. I gave necessary and sufficient conditions for one problem, but not the right one. I made a Type III error: I solved the wrong problem.

Still, while some may think that this is to be expected from a researcher from the University of Utah (a la "cold fusion"), I do believe that my article has served to advance the state of knowledge in the area of small-sample confidence intervals. In fact, I believe that the paper and the ensuing criticism are a vindication of the scientific and publication processes. By making claims and then having those claims tested by others, we are able to make progress in all areas of science and technology. Perhaps it would have been preferred to have the difficulties in my article identified during the review stage, but in that case there may not have been as much opportunity for dialogue and discussion on small-sample interval methods. I am sure that I will continue to make mistakes trying out new ideas, and I am sure that other researchers will as well. I am happy to have the opportunity to do so, because without it I will not be able to learn and share that learning with my students. I thank *The American Statistician* and journals like it for making that learning possible.

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