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11 JUNE 1996

Total Pages = 8 (eight)

Dear Dr. Simpson,

Thank you for your delightful letter of 22 mai 1996 (received 3 jun). I would like to extend a heartily-offered, small ***BRAVO*** for your analysis, enlightening insights, and interests. Your letter generated a lot of algebraic amusement on my part. I do not recall having met you previously (I don't travel much these days), but I assure you that your reputation precedes you in that I immediately recognized your name, particularly in regard to your recent TDCR LS work.

Why you may have found my little harmonic mean paper to be "extremely interesting" is beyond me. It certainly was not meant to be directed at serious radionuclidic metrologists like yourself. I have, in fact, been absolutely shuttering at the thought of someone like my friend Jorg Müller (a true physicist/mathematician/statistician/philosopher) at BIPM reading it; uncovering its possible logical flaws; and jeering at its triviality! In contradistinction, I am just an old-fashioned radioanalytical chemist who attempted to present a rather simple solution (without any intended generality) to a problem that arose. I was told that the solution would be of interest to others.

I highly encourage you to submit your alternative interpretation (and nicer analysis) to *Radioactivity and Radiochemistry*, and would be willing to promote its publication with the editor (Thetis McFarland) with whom I have a relatively good working relationship. Nevertheless, you may first wish to further reflect on a few considerations (noted below) that are attendant to your analysis. I'd like to caution you on the purported generality of your time-weighted mean approach, on what its larger implications may be, and on attempts at generalizing your statistical analysis approach.

You may find the following comments to be in order.

1. I did not approach the original problem posed to me in any kind of rigorous analytical fashion (in terms of any generality). I probably should have before dashing off the paper (which I was encouraged to do so, by several colleagues). Rather, as I stated in the paper's introduction, I was presented with a tabulation of measurement data and analyses which just didn't "sit well" with me (or seem "right"). The first thing that popped into my mind was to use the ("to-me-obvious") harmonic mean approach. I just did not think of using the algebraically-equivalent time-weighted mean as you did (and apparently routinely do!). Your letter, of course, immediately demonstrated the equivalence (by absolute algebraic necessity) of the two approaches; and it now, of course, seems self evident (to me).

2. Originally, it equally was not "immediately obvious" to me that the harmonic mean was appropriate for the Example of Table 1 (which required the normalization factors). My scientific instinct, however, suggested that it was. Believe it or not, I actually performed your identical weighted-analysis procedure (but not as explicitly) to convince myself that the harmonic mean was indeed appropriate. I basically performed the same arithmetic exercises, but overlooked expressing it in some general form (as you nicely did). In short, I did not recognize it for what it really also was; *viz.*, a time-weighted arithmetic mean. To me, it was still just a harmonic mean.

3. The most profound statement of your analysis is in your "In summary ....". That is, "... if the weighted average is routinely used, one need not be concerned over the type of averaging required for a specific case -- it comes about naturally." Yet, I wonder about the generality of this statement, and how one might select a weight (or weighting factor) in all cases. Let's suppose one of several possible hypothetical experiments in which C is fixed for any given counting interval (with variable t), but C varies between counting intervals. The data for this experiment follows. Let's call it Table 2.

C (fixed)	t (variable)	$R = C/t$
100	93	1.0777
500	525	0.9250
1000	966	1.0353
2000	2234	0.8954
5000	5108	0.9788

What "average" should I use for the data of this table, given that our "intuitive result" (in the absence of any required normalization factors) is just $\Sigma C_i / \Sigma t_i = 8600/8926 \simeq 0.96348$.

4. I never suggested (much less stated) that one should use the harmonic mean in such a case since the fixed C is not constant between counts. My treatment in the paper considered only that for averaging data (of presumed equal statistical weights) in which C was fixed (and equal between counting intervals) and t was variable.

5. I still contend that the use of the harmonic mean for my original case was logical. You demonstrated that it is, however, mathematically equivalent to the time-weighted arithmetic mean. Your letter states that I "justified the use of the harmonic mean by numerical example only." There is no rigorous proof for the inherent validity and/or justification for the use of one type of mean over any other. Nor, for that matter, can there be a "proof" for the use of any measure of central tendency (see my Endnote #6 in the paper). I could extol endlessly on Daniel Bernoulli's (1700-1782) original arguments. The choice is always arbitrary and subjective as I tried to clearly point out in Endnote #6. I inelegantly expressed my criteria as basing the choice "on its reasonableness and usefulness for the situation" (termed "intuitive correctness").

6. I would not use the harmonic mean (unweighted) approach for the example in Table 2 since it is not intuitively correct to me. Nor would I use your time-weighted arithmetic mean since it equally is not intuitively correct to me (but here, I must admit that I confront a dichotomy that will be addressed below).

7. Now, at this point, I am perplexed since I don't know exactly when I would intuitively use your time-weighted mean. Let's consider a few cases for constant $R = C_i/t_i$:

   (i) t fixed and constant between counts; C variable .... I'd use an unweighted arithmetic mean (which is identical to your time-weighted mean).

   (ii) t fixed and different between counts; C variable .... I'd use an error-weighted arithmetic mean, and base the weights on the reciprocal variance of the "counting statistics" (*sic*) as is normally done. I would not use a time-weighted mean.

   (iii) t variable; C fixed and constant between counts.... I'd use the harmonic mean (unweighted) as given in my little paper (which you have demonstrated is the same as taking a time-weighted arithmetic mean).

   (iv) t variable; C fixed and different between counts..... I'd use a error-weighted harmonic mean, and again base the weights on the "counting statistics". This is, of course, the case covering the above example in Table 2. Again, I would not use a time-weighted mean.

8. Let's now return to the example in Table 2 [which is case (iv)]. You may contend that your time-weighted arithmetic mean will give the correct answer for all four cases. I'd like to know. I think we must instead enter the realm of that gray, fuzzy area that I called "intuitive correctness"... and look at the axiomatic basis (or justification for reasonableness) of what we are doing. I'll treat my four cases sequentially:

Do you agree with using an unweighted arithmetic mean for case (i). If so, why? I do because I'm assuming that $R = C/t$ is approximately normally distributed (for a Poisson process) and C is the variable.

Do you agree with using a weighted arithmetic mean for case (ii). Again, why? And what weighting factors would you use? I'd use weights of $w_i = 1/s_i^2$ on each R_i where $s_i = (\sqrt{C_i}/C_i)R_i = \sqrt{C_i}/t_i$; and where the mean R is obtained from $\Sigma w_i R_i/\Sigma w_i$ and with minimized standard deviation of $s(R) = \sqrt{(1/\Sigma w_i)}$. The basis of my choice is that I am assuming that the error on each R_i devolves solely from the Poisson process (the so-called "counting statistics" misnomer). Would you use a weighting factor of $w_i = t_i/\Sigma t_i$ to get a mean $R = \Sigma w_i R_i$ as you did in the analysis of your letter? And if so, on what axiomatic or inherent, logical basis?

We appear to agree that we can treat case (iii) by either the unweighted harmonic mean or time-weighted arithmetic mean (which are equivalent). I still prefer my intuitive choice because again I am assuming that C_i (for constant t) is normally distributed (for the Poisson process), but that t (in the denominator of the rate $R_i = C/t_i$) is the variable.

Now, at last, we can get to my new Example in Table 2 [case (iv)]. By extension, I think we need a weighted harmonic mean. I'm not sure yet what this beast is (since I only thought of it since receiving your letter). What is my reasoning? Again, I contend a harmonic mean of R_i (appropriately weighted) is the more intuitively correct choice because t (in the denominator of the rate R) is the variable. I would also choose to use weighting factors associated with the "counting statistics" since again I am assuming that these correspond to the error on each R_i. I'll need a little time to deduce exactly what this weighting factor should look like [perhaps its similar to that for case (ii) and perhaps not] and how to apply it. To a first cut, I think an appropriate weighting factor may be $w_i = 1/s_y^2$ where $s_y^2 = (\partial y/\partial R)^2 s_R^2$ for $y = 1/R$. The weighted harmonic mean might then be just $\Sigma w_i/[\Sigma w_i(1/R_i)]$. This is my first guess for one that would minimize the error on R, but I am not wholly convinced yet. As a practical alternative (considering the wide range in C and t for Table 2), I might be inclined to use the error-weighted arithmetic mean [as for case (ii)] to treat the example.

Given all of the above considerations, I'd now like to further reflect on a few additional thoughts that are in my mind. Regrettably, they are not necessarily very systematized and may appear to be rather disjointed and rambling. For this, I apologize. But, I am also trying to get this response off to you in a timely fashion.

9. The actual logical basis for selecting your time-weighted arithmetic mean approach is very curious to me. I can see how it applies to my case (iii), and see how it simply reduces to my case (i). But why should it apply to cases (ii) and (iv)? Surely, case (ii) can be treated by the arithmetic mean weighted by the "counting statistics" (which for convenience I've called the "Poisson error-weighted arithmetic mean"). Are all of the numerous texts on radionuclidic metrology, experimental nuclear physics, radiochemistry, etc. (which give this treatment) really wrong!? And why not treat case iv (e.g., Table 2) by weighting with the counts instead of by the time? That is, why not set the weighting factors to be $w_i = C_i/\Sigma C_i$ and obtain the mean R from $\Sigma w_i R_i = \Sigma[C_i R_i/\Sigma C_i]$? After all, it must be recognized that for some "true" R, the expectation value for variable C with fixed t is $E(C_i) = Rt$ and that the expectation value for variable t with fixed C is $E(t_i) = C/R$. That is, with constant R equal to unity. $E(C_i|_t) = t$ and $E(t_i|_C) = C$. Hence, C and t are always scaling in such a way that it is not obvious to me that one should automatically use time weights, but not count weights. Am I missing something fundamental? Furthermore, what is sacred about weighting with counting times (instead of counts) for any case?

10. I must humbly grant that your time-weighted arithmetic mean indeed gives my originally-defined "intuitively correct" result for the example of Table 2. But, I'm not sure why this is so. Actually I know why (it occurs algebraically), but I'm not sure that it is logically consistent with trying to characterize a mean R for a Poisson process that selects R_i with either variable C_i (and fixed t) or variable t_i (and fixed C). Yesterday evening, my dear spouse, Mme. Dr. K.A. Maroufi-Collé reminded me of what the ancient Greek mathematicians called "the curiosities" (loosely translated) and of how the early French mathematicians (at the advent of probability theory) explained, in part by dismissal, such curiosities. These good Frenchmen summarily contended that some curious relationships between various mathematical entities, that at times appeared to be illogically inconsistent (or dichotomous) with other beliefs, were largely a result of "flukes" (very, very loosely translated) that in turn result from the "physical forms" of the relationships themselves. Your demonstration of the equivalence of the time-weighted arithmetic mean and harmonic mean (unweighted) in case (iii), or that the time-weighted mean also gives the correct "intuitive" result for case (iv) may be illustrations of just such "flukes" that arise, of necessity, merely because of their algebraic forms. That is to say, for example, that the

intuitive (to me) harmonic mean was shown to be the same as the unintuitive (to me) time-weighted mean only because of the algebraic form of the two means. This obviously enters the realm of philosophical (or metaphysical?) discourse. Perhaps I am leading to a change in my original definition of "intuitively correct" as it applies to case (iv). You may scream at this audacity [i.e., for the defined axiom shift], but I really do not believe case (iii) and (iv) are identical cases. In short, I am suggesting that your time-weighted arithmetic mean for the Example of Table 2 gives a correct "intuitive" result (as originally defined by me) , but does so as a result of what I've termed the mathematical fluke that comes about merely because of its algebraic form. Unless you can demonstrate to me otherwise, it does not comport with my intuition in regards the Poisson counting process. In short, if you can use a time-weighted arithmetic mean for case (i) [which reduces to the unweighted arithmetic mean under conditions of t fixed and constant between counts] and for case (iii) [which reduces to the unweighted harmonic mean under conditions of variable t and C fixed and constant between counts], do you then suppose that you can equally use this approach for case (ii) and (iv)? Any contention to do so for case (ii) will be at variance with all of the extant literature in regards minimizing the variance on R for the Poisson error. By extension, shouldn't one impose comparable requirements on case (iv)? Please inform me if I am misinterpreting your arguments.

11. Attached is a hand-written tabulation (labelled ANNEX) in which I've tried to succinctly summarize six different mean values calculated for the data of Table 2. [Hopefully, I've eliminated all of my early and silly algebraic and arithmetic mistakes. Please excuse me, if I haven't caught them all.] I am providing this tabulation under the proviso that I am *a priori* questioning which is the best mean I should use. I really don't know, and need to consider these things at greater length. At this point, I'm really not sure, and I can not reconcile: (1) that my intuitive error-weighted harmonic mean makes sense from the underlying physics; while (2) your time-weighted arithmetic mean reproduces my originally-defined "intuitive" result. Perhaps you'll be able to enlighten me on this? The annex also contains reduced expressions for the various means under the conditions of constant C and constant t. One may immediately be struck (I was) by the parallelisms. Such as: that the count-weighted arithmetic mean with constant C gives the arithmetic mean, and that with constant t it gives a form $[\Sigma R_i^2/\Sigma R_i]$ that is the same as the error-weighted harmonic mean with constant C; or that the error-weighted arithmetic mean with constant t gives the time-weighted arithmetic mean with constant C (i.e., the harmonic mean); etc. Obviously, there seems to be a lot of good "curiosities" here -- with similarities perhaps to those originally found by the ancient Greeks for their various means (Endnote #3)!

12. The last remaining comment involves your use of the expression for obtaining the second central moment (about the mean) from the first and second moments about the origin. This calculation certainly will give you some calculated statistical estimator for the precision or

dispersion in the R_i data. But what will it mean, and when it is applicable? For certain, it will give you an unbiased estimator for the variance if you are dealing with a nice normal distribution. This is easy to demonstrate (almost by definition). But what if you are not? The estimator may or not be biased, depending on what you are actually measuring. To wit, if you assume that C_i is normally distributed for constant t (for large C in a Poisson process) then you can reasonably assume that $R = C_i/t$ is also normally distributed for constant t. In contradistinction, you can not automatically assume that some transform of R_i is normally distributed for constant C and variable t_i. Another way of looking at this is to re-consider Endnote #7 in my paper. One can certainly calculate the sample variance from the residuals about a mean, i.e., from $var = \Sigma(x_i - m)^2/(n-1)$ for mean m (which is an identical expression for your moment approach). Yet, if m is the harmonic mean, then this var is not an unbiased estimator. I have not worked out all of the cases where your moment-calculation approach for obtaining a variance may work (and when it may not). I merely bring it up to offer some caution on trying to express it in terms of some general applicability.

I now am not sure if this is the kind of response you expected from me in regard to the comments in your letter. Did I answer your questions and commentaries? I sincerely hope I have not gone off "on tangents" too much.

You have my regards, and I look forward to any subsequent thoughts you may have.

Sincerely,

R. Collé

ANNEX

		WITH CONSTANT C	WITH CONSTANT T	VALUE FOR TABLE 2 DATA
ARITHMETIC MEAN	$A = \frac{1}{n}\sum R_i$			(0.9874) N/A
HARMONIC MEAN	$H = \frac{n}{\sum\left(\frac{1}{R_i}\right)}$			(0.9834) N/A
TIME-WEIGHTED ARITHMETIC MEAN (à la Simpson)	$M_S = \sum \frac{t_i R_i}{\sum t_i}$ $= \sum \frac{C_i}{\sum C_i/R_i}$	$H = \frac{n}{\sum\left(\frac{1}{R_i}\right)}$	$A = \frac{1}{n}\sum R_i$	0.9635 "intuitive" ;
COUNT-WEIGHTED ARITHMETIC MEAN	$M_C = \sum \frac{C_i R_i}{\sum C_i}$ $= \sum \frac{t_i R_i^2}{\sum t_i R_i}$	$A = \frac{1}{n}\sum R_i$	$\frac{\sum R_i^2}{\sum R_i}$	0.9655
ERROR-WEIGHTED ARITHMETIC MEAN	$A_E = \frac{\sum C_i/R_i}{\sum C_i/R_i^2}$ $= \sum \frac{t_i}{\sum t_i/R_i}$	$\frac{\sum\left(\frac{1}{R_i}\right)}{\sum\left(\frac{1}{R_i^2}\right)}$	$H = \frac{n}{\sum\left(\frac{1}{R_i}\right)}$	0.9614
ERROR-WEIGHTED HARMONIC MEAN (?)	$H_E = \frac{\sum t_i R_i^3}{\sum t_i R_i^2}$ $= \frac{\sum C_i R_i^2}{\sum C_i R_i}$	$\frac{\sum R_i^2}{\sum R_i}$	$\frac{\sum R_i^3}{\sum R_i^2}$	0.9676