Tellus (1989), *41B*, 562–563

Reply to R. Jaenicke

By W. G. N. SLINN, *Pacific Northwest Laboratory, P.O. Box 999, Richland, Washington 99352, USA*

(Manuscript received 30 January 1989)

I thank Professor Jaenicke for identifying typographical errors in Slinn (1988a)* [hereinafter, Sa] and apologize to readers for any inconveniences. In summary, the identified corrections are:

(1) In the unnumbered equation immediately above (3a), a bar should appear above the braces;

(2) In (3b), λ should be replaced by T_r [i.e., (3b) should be as it appears in the inset of Fig. 3].

Also, I note that in the first sentence of the paper, Junge's name is misspelled and in (1) the subscript c is missing on the symbol for the coefficient of deviation, f.

In response to Jaenicke's criticism of the notation in Sa, I mention that I used Σ rather than σ (for the standard deviation) in the inset of Fig. 2 and used f_c in (1) and Fig. 1 but F_c in (3) and Fig. 3 to emphasize that Σ and F_c depend on "peculiar" definitions, whereas σ and f_c were obtained from data and from the numerical calculations reported by Hamrud (1983). In response to Jaenicke's comment that "the curve presented by Slinn from Hamrud (1983) cannot be found in the publication," I refer the reader to the legend of Fig. 3 in Sa, which identifies the specific curve from Hamrud. And, in response to Jaenicke's comment that there is nothing "peculiar" about the F_c used by Junge, consider the two extremes described below.

It is well-known that there are many potential sources of concentration fluctuations, for example, time variations in source strengths, spatial inhomogeneities of the sources (coupled with variations in trajectories and in diffusion between sources and detector), variations in removal processes en route, etc. As an example of an extreme case, suppose that the only variations were from time variations of the source strength (which is a first-order approximation for atmospheric CO_2 in the Northern Hemisphere, with variations to a large degree dominated by the annual cycle of the biosphere). If there were no other sources of CO_2 concentration variations [i.e., if the atmospheric concentrations followed "lock stepped" with the time variations dictated by the source (and associated sink)], then using the coefficient of deviation shown in Jaenicke's comment would yield a *nonzero* value for f_c, which, however, would reflect characteristics of the source. In contrast, if the theory in Sa were applied to this extreme case being considered (with no spatial variations in the concentrations), then the corresponding F_c would be zero.

Now, consider the opposite extreme. Suppose that there were no concentration variations in time [viz., steady-state conditions when viewed in a fixed (Eulerian) coordinate system], but that there were variations in space, e.g., caused by steady removal processes. An approximate illustration of this case might be found in the concentration of some anthropogenic pollutant over an ocean "downwind" of a continent. Then, with no time variations, Jaenicke's coefficient of deviation would be zero, but the "peculiar" F_c calculated in Sa would be nonzero. Surely these differences suggest that something is peculiar about this (and Junge's) F_c.

More significant is the following. For the case of only fluctuating sources and sinks and a "lock step" concentration field (i.e., no net removal from the reservoir during time periods long compared with the characteristic time(s) of the source/sink variations), Jaenicke's *nonzero* coefficient of deviation would nevertheless give information neither about the net removal of the chemical from the reservoir (which would be

* See Jaenicke's *Comments* for literature citations.

zero) nor about the corresponding residence time of the chemical within the reservoir (which would be effectively infinite). Instead, his nonzero value would reflect variations of the chemical's source. In contrast, for the steady-state case, when Jaenicke's expression gives a zero value and the "peculiar" F_c in Sa is nonzero, then it is shown in Sa that this "peculiar" F_c does, in fact, contain information about removal processes within the reservoir (and the associated residence time of the chemical). Moreover, it can be argued, from (3) or (4) in Sa, that $F_c = 0$ for the "lock step" case because the residence time $T_r \to \infty$.

With respect to differences between Professor Jaenicke's t_{max} and the Δs in Sa, I have submitted a separate note to *Tellus B* that suggests an experimental test of the differences between the two theories. The test, which probably could be performed using available data, involves calculating the coefficient of deviation for sampling periods of different (Eulerian) durations, Δt_E (or for portions of a single data record). My interpretation of Jaenicke's theory leads me to conclude that his coefficient of deviation would be independent of Δt_E, i.e., it would depend only on his "meteorologically significant" t_{max}. In contrast, the submitted interpretation of the theory in Sa suggests that F_c will increase linearly with Δt_E, at least for small Δt_E. Further, the submitted note (accepted for publication) suggests that the theory in Sa can then be used to estimate the residence time by replacing Δs in eq. (4) of Sa by Δt_E and then by using the data to obtain the initial slope of F_c versus Δt_E. Thus, in summary, I suggest that any controversy should be left for data to resolve; paraphrasing a famous remark by Lord Kelvin: if you can't measure it, there's no use talking about it.