Various quantitative parameters for estimation of salivary function using radioisotopic means have been suggested. Wide variability of the quantifiable parameters has been reported (*2*). Hermann et al. (*2*) discussed the noncentrality and dispersion of quantitative indices of salivary function. We believe that two crucial points need to be considered in arriving at a conclusion.

1. Dependability of quantified parameters: The partition percentage (PP) proposed as an index to identify composite function of individual salivary glands becomes inappropriate when its computation is extended up to 45min. By this time significant portions of salivary contents are lost on account of unstimulated salivary secretion. It is for this reason that the authors reported a PP of 66% for parotid glands and 34% for submandibular glands. These findings are discordant with the well-known finding that approximately 70% of salivary secretions are contributed from the submandibular glands. Of the total salivary secretion of approximately 1500 mL/day, it is known that the submandibular gland contributes approximately 1000 mL.

   Considering the episodic discharge pattern seen, the computation of net uptake ratio (NUR) as maximum counts of the glandular time-activity curve divided by initial postinjection counts (in short, salivary gland-to-background ratio) becomes unreliable. This argument is supported by the fact that the authors did observe a wide range of NURs for submandibular glands reaching 11-fold differences, whereas the range was narrower for parotid glands. The PP or any such index of salivary uptake may work if it is restricted to the first 3–5 min as done by Vigh et al. (*3*).
2. Selection and grouping of patients: The age group in the study of Hermann et al. ranged from 18 to 91 y; 25 of 31 subjects were women. In healthy subjects too, the salivary function varies with age and menstrual status. Mucin content and the concentration of the IgG and IgM decrease with age. Recently, a study of the patients selected from the 'Baltimore Longitudinal Study of Aging' has shown that premenopausal women had higher unstimulated submandibular secretions than postmenopausal women (*4*). Smoking, masticating and chewing habits too are known to modify salivary function. The authors (*2*) did observe weak relationship between age and functional indices, but, due to small numbers, the scatter was great.

The weakness of the quantitative parameters obtained should not be misconstrued as the inherent weakness of quantitative salivary scintigraphy.

The findings of Hermann et al. supplement our findings regarding the high frequency of unstimulated submandibular secretions (*5*). They have reviewed 32 salivary publications between 1971 and 1997 and surmise that the mean frame rate of dynamic salivary study was 231 s (range 30–1200 s). It appears that our study (*5*) was not noticed. We wish to point out that we used a frame rate of 5 s/frame and were able to segregate the so-called "episodic discharge" from the submandibular gland into various patterns. It varied from a sawtooth pattern (ripple pattern) to a slow and continuous discharge without any external stimuli. It would be interesting to know whether Hermann et al. noted such patterns in their 31 subjects. The authors (*2*) are right in pointing out that the submandibular glands contributed primarily to the oral pool activity before stimulation but state that about half the parotid glands showed multiple episodes of spontaneous nonstimulated excretion. We feel the incidence of parotid discharge reported is too high. Were the pattern and magnitude of parotid secretion similar to and coincident with submandibular secretion?

In conclusion, we agree that the indices proposed and evaluated by Hermann et al. are unlikely to be of use in decision making. A broader physiologic model for parametrizing such organ function has been proposed previously (*6*). We recently evaluated its application in salivary scintigraphy (*7*). The tracer input-output model has helped us in quantifying unstimulated secretion of salivary secretions, which we believe can be a major tool to evaluate xerostomia.

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"N of 1": A Respectable Pedigree

TO THE EDITOR: With all due respect to and best wishes for success to our new Editor-in-Chief, it is disheartening to see that one of his first official acts was to pronounce the death sentence on the venerable single-case study (*1*). I do not agree with Dr. Martin Sandler, however, that discontinuing publication of case reports "...to concentrate on more substantive, multicase studies" will *improve* the *Journal of Nuclear Medicine*. I would argue the opposite.

Dr. Sandler's motives and qualifications are not in question. He acts in the best interests of nuclear medicine in these uncertain and turbulent times, and his position is not particularly enviable. But his action represents a wrong-headed vision of science, one which holds that the skillfully told single event or occurrence is inferior to the so-called "more substantive, multicase studies." It violates an inate logic which understands that every beach begins as a single grain of sand.

Case studies—the medical equivalent of the historically revered narrative of the raconteur—have a critical place both in clinical medicine and in science. Science that disregards the importance of single observations is a truncated version of that branch of

knowledge whose strength lies in its capacity to consider and respect the multitudinous nature of human reality and experience and welcomes all under its fold. In my view, single and multicase studies are not contradictory but complementary and afford us the greatest scientific and clinical advantages when they are allowed to coexist in dynamic tension.

Where would we be today if the single-case studies of such clinical giants as Wernicke (*2*) and Broca (*3*) were submitted to journals with *JNM*'s bias? In recognition of the contributions of single-case studies to clinical medicine, no less a journal than the *Lancet* has recently reintroduced it to its august pages proclaiming, "Clinicians learn from anecdotes. . ." (*4*).

The careful and meticulous reporting of a single event, subjected to appropriate clinical criteria that conform to established scientific knowledge, is frequently the motivating event for a multicase study. "N of 1" is a benchmark with a respectable scientific pedigree and dismissing it out of hand is an act of both scientific and editorial irrationality.

Dr. Sandler's decision to end publication of single-case studies ignores medical history and deprives the nuclear medicine community of a valuable commodity. It cocoons nuclear medicine in an ideology in which the singular tale is sacrificed on behalf of the illusion that that which is once well told is inferior to the telling of many things.

Single-case studies have two important purposes. First, they can provide the impetus for multicase studies. Second, they can sometimes be exceptions to the rule, potent contradictors of the findings of multicase studies. The need for encouraging the possibility of exception was posited by the late physicist and Nobel laureate Richard P. Feynman who wisely noted, ". . .the exception proves that the rule is wrong" (*5*). Thus, single-case studies not only point the way but can also act as important sentinels against scientific untruths.

As an avid reader of and one-time single-case study contributor to *JNM* (*6*), I respectfully ask Dr. Sandler to reconsider his decision and commute the sentence of this innocent victim of the scientific wars to life without parole.

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