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COMMENTS AND LETTERS TO THE EDITOR

Ammonia Volatilization from Flooded Rice Soils

The recent publication by Mikkelsen et al. (1978) leaves the reader with some serious questions. The most important point to be raised concerns the methodology. Ammonia losses were estimated by two methods: (i) by scrubbing air before and after passing it through a jar placed over a soil, or, (ii) by collecting NH_3 in a static acid trap placed under a plastic canopy suspended over the water surface.

The authors rightfully list wind velocity as one factor influencing ammonia volatilization, yet fail to recognize that the same is true in method (i) (2, 3, 7). The rate of airflow imposed in these experiments appears one order of magnitude below reportedly required air velocities (3, 6, 7, 8) and thus becomes the limiting factor reducing soil, N source, or N management effects on NH_3 loss.

The method (ii) employed in these experiments is a novel approach. The question of what the collected NH_3 represents (e.g., does air move freely under the canopy or is it stagnant, what surface area contributes to the acid trap) has not been addressed by the authors.

Interpretation, in terms of absolute magnitude of NH_3 loss, of data collected by the two methodologies employed will require extreme caution. Reported agreement with other data that the authors reviewed is mainly due to similarities in measurement techniques. Alternative methods of measurement appear to lead to much higher estimates of NH_3 volatilization (1, 6).

A second point to bring to the attention of the readers deals with the floodwater chemistry. Firstly, the authors inaccurately quote Stumm and Morgan (1970, p. 127) by using equations that hold only for pure H_2O–CO_2 systems in equilibrium with atmospheric CO_2 ($pCO_2 = 3.5$), and applying them to natural waters. This leaves the reader with the impression that all natural waters maintain a pH of 5.65. The relevance of the electroneutrality equation for pure water in a study with water fertilized with urea or ammonium sulfate is doubtful in the first place.

Along this line, it remains unclear how the authors arrived at their bicarbonate and carbonate concentrations in Fig. 3 and 4. Assuming that 3 days after fertilization with N some of this N is still present in the floodwater to be volatilized, one must assume that the alkalinity titration includes the titration of NH_3 to NH_4^+. At a pH of 8.5 (Fig. 3) the contribution from NH_3 to this alkalinity may be predominant, yet the authors do not seem to take this into account. Besides, the authors do not reveal the source of N used (Fig. 3, 4), leaving the impression that there is no difference between the carbonate equilibrium of urea [$(NH_4)_2CO_3$] fertilized water and ammonium fertilized water. At least for Luisiana clay this seems at odds with results given in Fig. 2, as well as with my own findings (6, 7).

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PAUL L. G. VLEK

International Fertilizer Develop. Center
Box 2040
Muscle Shoals, AL 35660

Vlek's comments draw attention to the fact that collection of volatile ammonia under natural conditions always presents problems and that virtually all the devised systems are deficient in some respects. The authors have recently reported in "Ammonia volatilization from wetland rice soils" in Proceedings of the Symposium on Nitrogen and Rice, 18–21 Sept. 1978, The International Rice Institute, on these methods and cite at least 40 references where air-train-acid trap and static acid trap collectors have been used. The alternative methods of collection and measurement referred to be Vlek (1, 7, 8) possess deficiencies similar to others that have been published and do not agree with the main body of data in the literature.

A question raised by Vlek relates to an inadvertent manuscript error wherein the air exchange rate reported in the air-train-acid trapping device was printed at 6 per hour rather than the correct value of 6 per minute. Correction of this error has been requested of the Managing Editor. Vlek suggests that a "required" air exchange should be used. In reality there can be no "required" air exchange since ammonia volatilization is a function of wind velocity under field conditions and is subject to at least four additional major transfer coefficients as well as environmental and biological determinants. The authors choose the frequently used air-train-acid trapping collection method for this research fashioned after Fenn and Kissel (2), wherein an air exchange of 3.5 to 5.2 volumes per minute was adequate for maximum removal of ammonia collected over soil. We did not use a 2- to 3-fold "safety factor" of 14 to 16 volumes per minute as did these authors since adverse effects were created on evaporation rates, temperature, pressure, and air density. Unfortunately little information is available on ammonia transfer from aquatic rice systems where all the important variables have been integrated.

Vlek indicates that the authors inaccurately quote Stumm and Morgan (5) by using equations appropriate for pure H_2O–CO_2 systems in equilibrium with atmospheric CO_2. We state in our paper that the natural aqueous system is complex and that the reactions cited were "simplified". Vlek is apparently unaware, however, that 91.5% of the total world rice production occurs in Asia where an estimated 47% is directly rainfed and much of the balance irrigated by rainfed systems with relatively pure water. It was not the intent of the authors to characterize the carbonate systems occurring in all natural waters in our paper (4) but to characterize an important biological component in the aquatic system affecting ammonia volatilization.

Data reported in Fig. 3 and 4 of our paper (4) reflect the CO_3^{2-} and HCO_3^- values obtained by titration according to the standard methods for the examination of water. No provisions are made in these methods to correct for potential alkalinity due to undissociated ammonia.

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D. S. MIKKELSEN

Department of Agronomy & Range Science
University of California
Davis, CA 95616

S. K. DEDATTA

The International Rice Research Institute
Los Banos, Philippines

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Tolerating New Ideas

Occasionally a paper is submitted for publication with a message which is very unique and/or highly controversial and unpopular. When this happens, our review system (which is usually beneficial and effective) can fail miserably. A reviewer, who is typically a recognized authority on the topic being considered, can also be so biased that he fails (or refuses) to recognize the merit of such a paper. Many examples in the history of science can be cited in which erroneous notions have persisted for decades, or even centuries, because of reluctance to consider alternative views.

A possible way to improve our existing system is to submit one copy of each controversial manuscript to a reviewer who has not published in that particular niche (hence has no vested interests) and who is convinced that science thrives on new ideas. He would not necessarily be asked to judge the technical merits of the manuscript. If he thinks it is worthy of consideration by others, even if the expert reviewers disagree, then it would be accepted for publication. It might be condensed and located at the rear of the journal and entitled "Speculation" or otherwise made as inconspicuous and inobtrusive as possible.

In every science there is a need for some daydreamers who will simply throw caution to the winds and provide some speculative notions regarding unknown or poorly known phenomena. These rare individuals are the ones who have not become obsessed with fear of making a mistake in print and, like mad, impetuous turtles, are willing and able to stick their necks out. We need their ideas and their talents. Let's do something to reduce the battering they usually receive from reviewers. There is a relative abundance of the rest of us who are placidly ruminating over the scheme of things from within the security of our own carefully circumscribed little turtle shells. Newton noted that the rest of creation apparently lay undiscovered about him like sand grains on a seashore. It still is . . . or, at least, it still may be.

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RALPH A. OLSEN

Montana State University
Department of Chemistry & Chemistry Station
Bozeman, MT 59717

Reply to "Tolerating New Ideas"

The concerns raised by Dr. Olsen are certainly worthy of our consideration. There is undoubtedly a tendency on the part of many Associate Editors to ask colleagues to make reviews. An attempt to broaden the base of reviewers, therefore, is needed. I cannot, however, agree with Dr. Olsen that papers should be published when recommended by scientists that have not published on the subject when reviewers within the subject matter area advise against publication. The review process by peers has a long and trusted record in spite of its shortcomings. A good example is the following quote taken from a recent letter: "Thank you for your letter releasing our manuscript. To date we have been unable to come to a satisfactory resolution of the questions raised by the reviewers. Their comments have, at least, helped us avoid publication of what may be a serious error." The review process is sound, but we sometimes make mistakes because we are human.

The Society has recently obtained a computer to assist with membership records. We are exploring ways to use this equipment for identifying reviewers. A possibility is for members to indicate at the time they pay their annual membership dues their willingness to serve as a reviewer. If we can work out a manageable process in which a reviewer's willingness can be categorized by Divisions and specializations, then a list of willing reviewers for particular subjects can be sent annually to Associate Editors. This could enlarge our base of reviewers and might also tend to speed the review process.

We thank Dr. Olsen for his comments because they remind us how important the review process is and what a responsibility it is to review a manuscript. Every person who has the opportunity to serve the Society as a reviewer should forever be conscious of this responsibility and the necessity for objectivity.

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B. A. STEWART

Editor-in-Chief, *SSSA Journal*