Reply

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First of all I would like to thank Dr. Logan for her interest in my paper, entitled "On the atmospheric input of sulfur into the ocean". Her comment is entirely understandable to me, for the simple reason that after finishing my analysis, I also felt that the excess sulfate in wet deposition that I had calculated was surprisingly high in view of our present knowledge of the atmospheric sulfur cycle. However, I decided finally to submit the material for publication since 3-year observations seemed to me rather long and I did not see any reason to manipulate the data (i.e. to omit values for November and December 1977 gained on Samoa) because they did not agree with my ideas. It goes without saying that Logan is right that the result can be different if we use data for different sites and time periods. In this respect, I would like to point out that Várhelyi and Gravenhorst (1983) compiled the available information on sulfate in oceanic air and precipitation and concluded that the wet excess sulfur input into the ocean lies in the range of 53–78 Tg S yr^{-1}, which is about 1.9–2.8 times less than my estimate, based on a smaller data set. This indicates that I overestimated the sulfur deposition by a factor of about 2.5, the lowest value proposed by Logan.

Further, precipitation samples in the WMO network are collected by wet-only and not by bulk collectors. Even at Mauna Loa Observatory, wet-only collectors are used and have been used in the past (J. Miller, personal communication). This means that the data I took into account had been obtained by wet-only gauges.

Finally, it is not meaningful to control wet deposition data *measured* by emission estimates based, at least partly, on *speculation*. Among other things, we can deduce the possible magnitude of the source strength just from deposition calculations. Thus, even if my results are only correct to within a factor of 2.5, they indicate that biological sulfur sources may be larger than the other estimate quoted, by Granat et al. (1976). This possibility is the main point I wanted to stress in my paper.

REFERENCES

Granat, L., Hallberg, R. O. and Rodhe, H. 1976. The global sulphur cycle. In: B. H. Svensson and R. Söderlund (eds.) *Nitrogen, Phosphorus and Sulphur-Global Cycles*. SCOPE Report 7, Ecol. Bull. (Stockholm) 22, 89–134.

Várhelyi, G. and Gravenhorst, G. 1983. Production rate of airborne sea-salt sulphur deduced from chemical analysis of marine aerosols and precipitation. *J. Geophys. Res.*, to be published.