In the current landscape at our study site, the slightly eroded Tama pedon is located on the margin of a convex interfluve (summit), the moderately eroded Tama pedon is located on the upper backslope (immediately below the shoulder) at a 0.8 m lower elevation, and the severely eroded Tama pedon is on the middle of the backslope 2.5 m below the elevation of the slightly eroded pedon. In our judgement, the loss of 0.5 to 1.0 m of loess and an undetermined but substantial amount of loess from geological erosion (Ruhe et al., 1967) from the backslope and margin of the summit, could result in the suggested shifting of the two eroded pedons of Tama soil from the margin of the interfluve of the original land (loess) surface, immediately after deposition, to the upper backslope of the current landscape and result in an associated slope gradient change.

The comments by Daniel et al. suggest that other researchers including Olson and Beavers (1987) have failed to prove the amount of erosion at the site. We interpreted the existence of a 86 cm thick deposit of sediment overlying a buried A horizon as evidence that extensive erosion of the backslope had occurred after cultivation. More detailed documentation of the extent of sedimentation and erosion in the 10.5-ha watershed has been developed by Kreznor (1987). Approximately, 17 500 Mg of sediment was found on the 2.5-ha toeslope overlying a buried A horizon with a radiocarbon date of 1550 yr before present. The convex interfluve (summit) occupied 2.6 ha and the more sloping sideslopes covered 5.5 ha. Using an average sediment delivery ratio of 0.56 based on research from other similar small watersheds (Piest et al., 1975; Water Resource Planning Staff, 1984) an additional 22 280 Mg may have been delivered to the stream.

Sedimentation data suggests that the sloping areas could have been covered with an additional 54 cm of soil prior to cultivation. It is quite likely that some areas would have lost substantially more than the average depth, including the severely eroded Tama profile. These sediment thickness measurements and projected amount of soil loss from erosion after cultivation do not appear to differ substantially from the 1 m of soil loss in the last 1500 to 6500 yr (including both geologic and cultural) projected by Ruhe et al. (1967) for slopes in southwest Iowa. One approach used by researchers to quantify cultural erosion is to locate a sloping site in the area that was not cultivated and compare the soil property measurements to the cultivated site. On the sloping areas of nearby uncultivated sites, Kreznor (1987) found Tama soils with well-developed argillic horizons. Two representative Tama pedons located on a backslope at the uncultivated site on 8 and 9% slopes had lower boundaries of argillic horizons that were 40 cm deeper than the paired, moderately eroded, Tama pedon and 92 cm deeper than the paired severely eroded Tama pedon at the cultivated site. We believe these uncultivated site data support our proposed method to estimate soil loss from erosion.

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Comments on “Effects of Rubidium, Cesium, and Thallium on Interlayer Potassium Release from Transvaal Vermiculite”

In a recent paper Frantz and Carlson (1987) they measured the effects of Rb, Cs, and Tl on K release from Transvaal vermiculite. It seems that they have overlooked (or at least have not discussed in their paper) one important point that when this work was done it leads to different interpretation of their results. They found that their sample was not pure vermiculite but also about 40% true mica (presumably biotite). The sample initial K content (103 cmol K kg⁻¹) was not only allocated to mica since true vermiculite does not contain K. This was something to be expected since other researchers do not seem (or at least do not state it explicitly) to have applied the commonly used method of NaCl treatment for separation of rather mica (Jackson, 1969).

The fact that the sample used in their study contained both mica and vermiculite (and possibly some other minerals) complicates the situation and may affect the interpretation of their results. Was it the release of K from the mica component or the release of freshly added K that was actually measured? It is easy to differentiate between the two processes when observing their simultaneous occurrence when the sample is leached with 0.01 M CaCl₂. Thus, one can risk is that the release of freshly added K (and absorbed in the vermiculite interlayers) that was actually measured? It is not easy to differentiate between the two processes when the release is measured. In a recent paper Frantz and Carlson (1987) claimed that the release of freshly added K is faster than the rate of release of mica K. Under this line of reasoning, what interpretation can be given to the calculated activation energies of K release and to what fraction of K, the ease of releasing, pertain? One can argue that the K release energies pertain to the release of mica fixed K and not to K release from vermiculite. If the measured K release does not reflect the release of mica fixed K, then one must question the reliability of their data.

The authors interpretation of Alexiades and Jackson (1965) results is erroneous. Alexiades and Jackson (1965) determined average cation exchange capacities (CEC) for Transvaal vermiculite of 5 and 159 cmol kg⁻¹ for interlayer sites, respectively. However, the CEC for vermiculite was estimated at 154 cmol kg⁻¹ (Jackson, 1969). The authors interpretation of Alexiades and Jackson (1965) results is erroneous. Alexiades and Jackson (1965) did not measure the average CEC for Transvaal vermiculite. They measured the average CEC for Transvaal vermiculite. They measured the average CEC for Transvaal vermiculite.