DISCUSSION.

ORIGIN OF LOESS—REPLY

Doctor Holmes' criticism appears to be based on internal evidence from my paper on lower Mississippi Valley loess rather than on actual familiarity with the deposits. It attributes to me several observations and opinions not stated in the paper and which I do not hold.

An impression is given that loess is associated with "slightly dissected" Pleistocene terraces, quite in contrast to my statements that "Other things being equal, a deeply dissected territory favors development" (p. 16), "In deeply dissected territory it creeps into valleys, where it accumulates to considerable thickness" (p. 24), or "Pleistocene terraces and underlying Tertiary formations have been deeply dissected in much of this [south of Natchez] territory, and loess mantles lead down slopes, with increasing thickness, to the deepest valleys. Extreme range in elevation, disregard for compass orientation, and movement across truncated strata are especially well exhibited south of Homochitto River" (p. 16). "When the Recent deposition started, the bluffs at Natchez were over 450 feet high, and subsurface borings show that the face now covered by alluvium was about as steep as the part exposed today . . . Each [loessification stage] may have been a glacial stage, when valleys were more deeply dissected and colluvial slopes were longer [than they are today]" p. 32.

It is possible that the last quotation given led Doctor Holmes to the idea that "Loessification is not now in progress in the lower Mississippi Valley." My own view is that "Loessification may be in progress today . . . but completion of the process may acquire an interval that has affected only [late Pleistocenee] pre-Prairie parent materials as yet" (p. 32). The right kind of parent material, long slopes, and sufficient time appear to be the necessary factors for loess formation.

In contrast to "any churning movements in loess formation apparently did not destroy the well-defined basal contact" are my statements, "In the lower parts of exposed limbs [of pseudoanticlinal exposures] there is a notable amount of gravel admixed in the loess, the thickness of the mixed material increasing downslope . . . Gravels in the admixed loess are gray; stones have random orientation and exhibit no evidence of stratigraphic continuity" (p. 11) . . . "there is no reason why a tooth or bone should not find its way into this material, just as gravel does" (p. 22), "Gravels are genetically foreign to loess but become incorporated during mass move-

ment . . . Gravels may in all cases be traced upslope to their source beds. Loess creeping over gravel layers picks up stones and incorporates them in the process of carrying them downslope" (p. 20).

The criticism attaches significance to "churning movements" that I did not emphasize, as may be appreciated from the fact that in my summary of the process of loessification (p. 30) no reference to churning occurs. The churning mentioned is the type commonly recognized by pedologists, contrasting with laminar translocation. It is well known that such movements cause the rise of larger particles to the surface, whether churning be caused by alternating freezes and thaws, the growth of plants, or the creep of soil and similar materials down slopes

After quoting me to the effect that "Surface losses of fine material occur for various reasons" the statement appears that "The 'various reasons' might include wind action, but the attempt is to avoid appeal to eolian processes." My statement refers to processes responsible for concentrating coarser particles residually. Wind appears to play an important part in leaving residual desert pavements in the arid Southwest. It may play a similar rôle in the lower Mississippi Valley when it raises dust, but residual concentration of coarse particles is not an essential part of the eolian hypothesis of loess origin. The hypothesis is concerned primarily with deposition. "It seems improbable that either wind or water could move with the uniform velocity required to permit the accumulation of material so homogeneous that at least half and ordinarily about three-fourths of its particles (by weight) fall within the limited diameter range 0.01-0.05 mm." (p. 24).

The discussion "Problems of mineralogy" refers mainly to observations that may be checked as to factuality. Criticism based on the introduction of contradictory facts has far greater weight than that based on opinion or speculation. The assumption that calcite grains in loess are detrital is wholly at variance with any facts known to me. The secondary nature of the carbonates is clearly emphasized throughout my paper. Polynov's assertion that the mineralogical investigation of carbonates such as occur in loess is practically a virgin field for research (p. 26) is true as far as I know.

To the assertion, "No evidence that secondary-carbonate enrichment ever extended throughout the deposit classed as leached loess," I may cite, "The newer cuts along U. S. 61 south of Vicksburg display deloessification zones many feet thinner than those along old roads in the vicinity. The zone is always thickest on the face of a cut, and ordinarily its base slopes sharply upward in the first few feet . . . Effective leaching removes carbonates, shells dissolve completely, root tubes suffer various degrees of destruction, and aggregates start breaking down . . . All features related to competence gradually disappear" (p. 33).

My paper would have been strengthened by the inclusion of grain-sized analyses of backswamp deposits. At the time of writing it seemed sufficient to include only a typical loess-like sample from the lowest terrace and a sample of material that has almost become loess on a slope leading from an upper terrace. The ratio clay to silt in backswamp clays is by no means as high as the term clay would ordinarily lead one to believe, and I regret that my paper is not sufficiently emphatic on that point.

The problem of disposal of fine material is not difficult. No significant topographic changes are required. Most of the sorting goes on while A-horizons are being developed in parent materials. The concentration of coarse particles in A-horizons is characteristic of most mature soils and is mainly the result of translocation of finer particles to B-horizons below. Mass movements on slopes leading to loess deposits affect mainly the surface, where coarse particles have accumulated residually. Concentration of coarse particles is characteristic of colluvial phases of most soils. These processes explain the concentration of silt in loess, whereas finer materials are left behind in the subsoil horizons of parent material areas. These places are topographically higher than those where loess accumulates. The quantitative aspects of the problem are not serious. Slopes of parent material are widespread in the loess belt but individual deposits of loess are relatively small.

The statement that I offer but two facts in support of colluvial origin of loess appears unfair. That one of them is the tail of fine grain sizes extending far beyond the coarse silts that constitute an overwhelming proportion of each sample is an interpretation that I did not anticipate. This fact was stated quite incidentally, rather than as a vital link in an argument. I regard it as confirmatory and believe that practically all sedimentologists will agree that it indicates sorting of the type described. The weight of the evidence in support of colluvial origin lies mainly in conclusions derived from intensive field work in stratigraphy. Laboratory investigations confirmed gradations noted in the field. Pedogenic theory appeared to be favorable to conclusions reached. A rather thorough investigation of literature yielded many confirmatory observations and little valid contradictory evidence Personal familiarity with many European loess localities helped provide background for the major thesis presented.

The statement, "No field or laboratory fact cited seems to disprove eolian origin for loess in the lower Mississippi Valley" appears to indicate complete disregard for most of the observations on which my paper is based. Is it possible to explain the distribu-

ion, orientation, shapes, internal characteristics, and stratigraphy of loess deposits under a theory of eolian origin? A serious attempt, extending over many years, to do that very thing convinced me that it is impossible.

I refuse to accept credit for championing "a new process hitherto neglected or unknown." References in my paper show that Hilgard, Leverett, Mabry, Harris and Veatch, Todd, and Shaw saw that "loess grades into alluvial clays" (p. 26). Hilgard, Moyer, Worthen, Winchell, Chamberlin, and Todd suggested possible colluvial origin of loess. Relationships between colluvial origin and uniformity in grain size were recognized as early as 1866 by Worthen and in 1886 by Wahnschaffe. The transition to less loessial characteristics with increasing distance from bluffs was noted by both Todd and Leverett in 1897. Even the term "loessification" was suggested to me by A. Penck (p. 2).

One may not expect immediate acceptance of a thesis that runs counter to firmly established belief, however flimsy the evidence upon which the belief rests. In venturing into a position where it seemed necessary to renew an argument that appeared to be settled, I realized that various antagonisms might be aroused and brought the matter to print only after many years of careful investigation. If this venture renews an interest in field work that will lead either to the confirmation of my thesis or to other explanations even more positive and conclusive, my efforts will have been well repaid.

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