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SOME GEOLOGICAL MYTHS

Introduction. In the teaching of geology and physiography one soon finds a number of statements that have been repeated and repeated in the textbooks for many years after they have been shown to be incorrect. One may tell students ~~repeatedly~~ that such and such statements have been shown to be errors or that they to not have the significance which was originally thought, but it does little good; the spoken word is always weaker than the written statement and is soon forgotten. Makers of text books apparently have been unable in the magnitude of their task to study all of the latest literature, buried as much of it is in publications of limited circulation. Furthermore, the older reports that covered wide areas are most easilly cited and modern papers on limited regions are necessarily neglected. Not all texts contain all the errors and misaprehensions cited below and ~~to avoid injuring the feeling of the authors~~ for obvious reasons no reference will be made to publications ~~that xxxx~~ which include the statements in question. Some of the criticisms are made on the basis of the writer's own field experience but in the case of citations of authors no attempt has been made to compile exhaustive bibliographies.

The loess. The subject of the origin of loess long perplexed the geologists of two continents and it was not until the comparativley recent work of Shimek and Udden ~~(1)~~ that its eoloian origin was finially demonstrated. In spite of this fact certain misapprehensions still linger. One of these is that the loess of the Mississippi Valley was entirely derived from freshly-deposited glacial drift and the floodplains of glacial streams. That ~~some~~ much dust was so formed is undoubtedly true and ~~such forms~~ this makes the silty soils ~~over~~ of many outwash terraces and young drift areas, but the great bulk of the loess contains mild-climate fossils and ~~xxxxxxxxxxxxx~~ which rests upon drift that had been weathered and eroded for a long time. Even in the so-called Iowan drift area of northeastern Iowa loess is present above a zone of pebbles that were concentrated by erosion. The source of little of this material could have been freshly ~~eroded~~ deposited drift

neither nor one in northwestern Iowa where similar conditions hold for this area is ~~xax~~ situated in a position to ~~fxrxixxx xxxxfxrxixxxxxxx~~ supply dust to the west winds ~~that~~ which spread loess from ~~western~~ Nebraska and Kansas eastward to Ohio and southward nearly to the Gulf. ~~The fossils show definitly that~~ If the testimony of the fossils has been read correctly the main portion of this loess was deposted in interglacial time and its relation to stream bluffs and ~~to~~ other topographic features resulted from their effect on conditions of ~~deposition~~ lodgment and not to any extent because the loess was derived from the floodplains. The source of the American loess appears to have been originally similar to the same as in Asia, the deserts where mechanical weathering predominated over chemical decomposition. There is no reason for ascribing a low level of the land as a cause for loess formation nor to connect it in any manner with a particular glacial advance or retreat. Beneath the comparatively fresh ~~drxfx~~ loess of southern Iowa there is an old leached loess, Shimek's Loveland formation. Loess has been found buried under the Illinoian and Wisconsin drifts and non-fossiliferous loess is common on the latter. Loess was deposited at several times during the Pleistocene. It is high time that everyone should abandon the thought of there being an "Iowan Loess" and think of these wind deposits as formations normal to the borders of arid regions and ~~only~~ to a ~~very~~ minor extent as ~~necessary~~ accompaniments of glaciation. Loess tells only of past climatic changes, ~~and not of differences in elevation of the land or of particular kinds of glaciation.~~

Erosion by clear water. A common statement in textbooks and reports is to the effect that clear waters, such as the outflow of lakes, cannot cause erosion. One author even has gone so far as to use this statement to explain the lack of gravels along a ~~certain~~ supposed Pleistocene river channel in glacial drift. The absurdity of this statement is apparent when one thinks that it applies only to streams whose beds and banks are ~~made of~~ solid rock. Any water that flows over loose material soon aquires a load and can then ~~after-ward~~ erode solid rock that may be encountered farther downstream.

Meanders and the cycle of erosion. It seems to be a common idea that a river ~~which~~ meanders means that the region in which it flows either is in, ~~and~~ or has been in, the stage of the erosion cycle known as old age. As a matter of fact, meandering is primarily a function of grade of the river in relation to its volume and the character and amount of its load. Many ~~xxxxxx~~ postglacial streams meander because the initial slope on which they began to flow was gentle, and other streams have begun to meander because of the silting up of the valleys as above mill ponds. ~~The latter~~ Aggradation may result from delta-building, sinking of the coast, deposition of glacial outwash in main valleys, tilting of the land, building of alluvial fans across valleys by tributaries, arificial dams, climatic changes, and probably other reasons which have no connection with stage in the erosion cycle. Field examination of almost any valley in the Driftless Area where the main streams were filled with outwash shows that the tributary streams which meandered on the alluvial fill are now cutting downward in the same meandering course and that cutoffs occured at numerous places during this process. It is evident that meanders have no relation to the stage of erosion in the surrounding country, and that areas adjacent to meandering streams should by no means be classed as in old age.

Stage in the erosion cycle. Most texts contain a lengthy list of criteria based on stream and valley characteristics by which one is supposed to distinguish between youthful and mature topography. Students memorize these with little thought that in the field many of these things may be obscured by differences in kind of rocks, recent aggradation of the valleys, and so forth. It seems very difficult to show them that the only real test is the proportion of the area that consists of divides that have not yet been affected by the work of running water and that maturity really means that nearly all of the region has been cut up by drainage channels. Aside from certain regions of very soft rocks there appear to be no regions of unmodified old age topography. Many of the ~~old~~ stock "old ago" areas seem to have been determined by the

Others are stripped areas or plains of differential erosion.

study of inaccurate topographuc maps and not by field work. For that matter, conclusions as to physiography and geology that are based on maps of the U. S. Geological Survey that were made before 1900 are, in many instances, virtually worthless, and for in fact some of their later maps are none too accurate in forest areas and regions far from the highways.

Even skylines. There appears to have been a time in the not far distant past when all that a geologist had to do to demonstrate the former existence of a peneplain in an area was to ascend a hill and observe the distant sky line; if the view showed that to be fairly even the proof was held sufficient. Only a very few like Tarr, and Martin, and Daly have ever ventured to protest against this utterly unscientific view. If we analize the problem we must first realize that vertical distances are everywhere small when compared with horizontal ones; an elevation of 5000 feet is generally termed a mountain but the eye sweeps over a mile of skyline without a thought. If the elevations occur close together and the observer is far enough away the sky line always appears even. If the rocks dip at a gentle slight angle of not over a degree or two the tops of individual ridges and of divides, both of which are located by the presence of strata harder than the average, always appear level. The "even sky line" in no more than the blending in the distance of these nearly level crests of sub-equal elevation; the appearance of a distant plateau rim is a "will-o-the-wisp" that forever recedes from the investigator who penetrates a highland, and which he can never reach. This illusion of an "upland plain" is best seen where the ridges are protected by a nearly horizontal formation of resistant rock. Even where the "plain" appears to bevel minor folds it is well to recall that at a distance one could not tell for certain if this were true for they are very low in comparison to their horizontal extent. It is also claimed that many such "uplands" bevel across entire geological formations. In such cases it is found that the gently inclined strata form a series of cuestas. At the crest of each cuesta, particularly

if the reistant ~~formation is a~~ beds are limestone or dolomite, which weathers by solution, the divides lie some distance below the original top of the formation. Going toward the next higher cuesta the stratigraphic horizon of the ridge tops rises until it lies in the soft formation at the foot of the escarpment. What more could apparently be needed to demonstrate that there had been a peneplain? But let us stop to think: the soft formation once covered the entire area in question and in the retreat of the escarpment it took time for its removal. The underlying hard formation was exposed only by the ~~removal~~ erosion of its cover and has been longest exposed and hence most reduced by weathering and erosion at the crest of the cuesta which it forms. Therefore there is nothing remarkable in this beveling; it is a perfectly normal phenomenon regardless of whether the area has been degraded by one or by more erosion cycles. It is high time that the "even sky line" test of peneplaination be relegated to the discard; there are dissected peneplains but more than this test is needed for their demonstration.

Dating of penenplains. Throughout geologic and physiographic literature one finds many references to penplains to which geological dates or names have been applied ~~and then extended~~ over wide areas. Such terms as "Harrisburg Peneplain," "Cretaceous Peneplain", ~~and so on~~ are current and have apparently been accepted by many as proven facts, ~~by a great many geologists~~. But let us stop to see how these things were determined. There is ~~really~~ no way to determine the age of a peneplained surface except by finding a fossiliferous sedimentary formation which overlies it. In some places where exposures of the rocks are exceptionally good this is not difficult, but ~~for~~ over the ~~most~~ greater part of the world the contact of the overlapping formation is seen ~~over~~ only in a narrow belt where its edge is being worn back. Down the dip from the outcrop zone knowledge of the supposed buried peneplain is derived only from scattering well logs often of equivocal character; in the other direction the grade so determined must be extended by use of a straight edge on a profile. If we ~~stop to~~ consider, first the imperfections of ~~most of~~ the data, and second, that after a long lapse of time any surface is sure to be more or less warped by

earth movements we will see that definite demonstration of the equivalence of any upland of the present cycle with a buried surface, which was more or less altered by marine action, is exceedingly difficult. Add to this the freedom given to the student of peneplains to reject any portions that seem too high as monadnocks and all parts that he thiks too low as later erosion, and we have left a very slender basis for correlation. ~~in many of the best known cases;~~ It is rare indeed that ~~the~~ a case of this kind will stand close critical scrutiny. ~~To~~ Turn to the tracing of supposed peneplains from place to place away from the ~~burying~~ overlying formations the matter is still more vague. Judging from available literature much of this pastime was carried out by assuming grades and intevals and then looking for facts to support them. All too few writers have ~~ever~~ presented accurate profiles on which the nature of the rocks was shown. Without that information the conclusions can scarcely be called more than guesses that are interesting if true. Furthermore few seem to have realized that to find remnants of partial peneplains not far from the coast is no evidence that such were ever developed in the interior of the continent.

Rejuvination of drainage. It is very common to see a statement to the effect that the erosion of an area ~~xxxxxx~~ was initiated by an uplift of the land. Such a statement is entirely correct for regions near the sea but one must stop to consider that in the interior of so vast a continent as North America it must take a ~~vast~~ lapse of time for any uplift to affect ~~the streams~~ erosion for ~~xxxxxx~~ rejuvination can only take place through the slow headward erosion of the steepened portions of the streams. One can see this process at work in the aggraded valleys of the Driftless Area where since the last glaciation the incised portions of the streams have not yet reached back many miles. How long then did it take for an uplift to affect the continental interior? Some physiographers have even gone so far as to discriminate between ~~xxxx~~ "incised" and "entrenced" meanders on the basis that the downcutting of the former was due to entrenchment of the main stream and of the latter to uplift of a peneplain. Just why the method of

stream ~~&~~ erosion should be differentiated is not clear, for in any case the main streams wear down before the tributaries can ~~xxx~~ intrench their courses. Even if the rejuvination was due to a tilting of the land the same holds true for the tributaries which were not affected by the change of slope, It seems far more likely that the absence of a slip-off slope in certain meanders merely indicates that their growth was checked at some time during the erosion because either of increased grade ~~of~~ the stream due to headward erosion or more probably to encountering of hard rocks. Most intrenced meanders are far too large for the size of the stream and must have grown during part of the time of intrenchment.

Effect of glaciation on soil conditions. An idea to which much credance had been given is that glaciation produced better soils than formerly existed in the same areas. Attention is called to the ~~generally~~ excellent soils of Iowa, Illinois, ~~xxx~~ Indiana, and southern Wisconsin as compared to conditions in the Driftless Area. The notoriously poor agricultural conditions of New England are explained as due to a predominance of glacial erosion over glacial deposition in that area. Elaborate computations from agricultural statistics have been presented to prove this idea in which the favorite area for comparison has been southern Wisconsin. But no mention is ever made of conditions in northern Wisconsin and northern Minnesota or of the fact that New England was quite close to the glacial boundary and does not show very much bare rock stripped by the glacier. No mention is made of the barren glacial outwash plains of these ~~xxxxxxxxxxxxxxxxxx~~ areas of generally poor soil, or is there any mention of loess, the important soil of both the early drift ~~areas~~ regions and the Driftless Area. It is evident that other factors have been at work that are not ~~generally~~ widely understood. The real key to the situation is that the areas of crystalline rocks, hard granitic and metamorphic rocks, furnished many large bowlders ~~xxxx~~ to the ice along with a great amount of sand. The till soils of the Superior Highland and New England are too stony

and the glacio-fluvial materials are too sandy for successful agriculture. Much of the famous good soil of Illinois and Iowa is loess whose origin is in no way directly associated with glaciation and for that matter the same material forms a very considerable portion of the soils of the Driftless Area. Were it not for the loess the weathered old drifts, a portion of which ~~are~~ is covered by gumbo, would make a much poorer agricultural showing than they do, ~~and the same would be true of the Driftless Area.~~ It is improbable that the soils formed by the decay of the limestones and shales of northern Iowa and central Illinois were materially improved by glaciation and indeed obstruction of drainage by glacial deposits probably fully offset the introduction of ground up limestone and other unweathered materials. In the sandstone regions of eastern Wisconsin and ~~even~~ in some of the more rugged limestone-sandstone areas, glaciation was distinctly an improvement by mixing limestone into old acid soils and by reducing the relief. It is not fair, however, to consider that all of eastern Wisconsin was ever as rough as the Driftless Area for there was much more limestone topography ~~that was only~~ gently rolling and the average relief was evidently much less than along the Mississippi River. It is well therefore, to pause before making any sweeping statements as to benefits or losses resulting from glaciation except in limited areas.

Lafayette gravels. The older literature and texts abound in references to the Lafayette Gravels. Indeed, there is scarcely a state on the Atlantic side of the continent where gravels have not been referred with more or less assurence to this formation which seems to have served as a catch-all for unconsolidated deposits without fossils. Originally described as a marine formation resulting from a deep submergence of the continent, deposits of diverse character and relationships appear to have been correlated as Lafayette. Many of these deposits ~~were~~ are thoroughly oxidized and all ~~were~~ are entirely unfossiliferous, both characters unlike known marine formations. Recently Shaw has demonstrated that the type locality of the Lafayette has no deposits above

the recognized Tertiary and Cretaceous marine sediments. There are elsewhere marine deposits formerly called Lafayette that are distinct, of late Tertiary age, and which deserve formational names, but it is high time for geologists to consider inland gravels as more likely fluvial than marine. Each case should be taken up on its own merits for it is probable that there are stream gravels of many different ages from Tertiary to late Pleistocene. We should no longer date events with reference to the ~~supposed~~ "Lafayette" formation.

Differences between the older drifts and the Wisconsin drift. There is a marked difference between ~~much~~ most of the pre-Wisconsin drift and the Wisconsin drift of its type locality. The former was before its erosion a featureless plain either devoid of moraines or with ~~only~~ low ridges, and everywhere contains little gravel. The Wisconsin drift of southeastern Wisconsin ~~is rolling with~~ has abundant drumlins, terminal moraines, and glacio-fluvial deposits which form striking kames, eskers, pitted outwash, and deltas. This apparent difference ~~has~~ led to many explanations; it ~~has been~~ was suggested that the earlier glaciers did not have the "moraine-forming habit", that they did not erode the rock but moved slowly or were very thin, that the drainage conditions were unfavorable for gravel formation, and that the load of drift was light, ~~and so on~~. A little thought shows that these supposed differences explanations are for the most part unreasonable or incompetent. Moraine forming depended upon how long the ice margin remained stationary in a given belt. The early ice sheets were so much more extensive that they must have been thick and as a matter of fact striae are rare in the older drift only because the conditions are unfavorable for their discovery in that good exposures of firm rocks are uncommon in those regions. A gread deal of drift was carried by the older ice sheets so that they could never have been lightly loaded. No matter what the altitude of the land the ice must have melted and even if its edge was in water deltas would have been formed. The fact of the case is that the drifts appear to have become progressively more and more stony. As shown by Sardeson this was because the first ice advanced over

preglacial residual soils and decayed rocks while later in the Pleistocene more of the drift was derived from ~~stripped~~ glaciated ledges and from earlier glacio-fluvial deposits, ~~of early drifts.~~ Furthermore, regions of hard rocks, both crystalline and sedimentary, furnished more stone to the drift than did the soft rocks of the Coal Measures, ~~states.~~ The angle of repose of stony till and of glacio-fluvial deposits, which were naturally more common in such regions, is greater than for clay till. Gravels form some of the most striking of the topographic features due to glaciation. Besides, if we compare the Wisconsin drifts of the soft rock states with the older drifts of the same areas and allow for the ~~softening~~ effect of erosion on the forms of the ancient drifts, the difference is less striking than the original comparison of drifts of regions with radically different bed rocks.

Lithological discrimination of glacial drifts. Many text ~~xxxxxx~~ books and geological reports state that glacial drifts belonging to different stages of the Pleistocene ice sheets have a definite lithologic composition. In some cases the differences are described as ~~lying~~ having in the percentage of ~~erratics~~ stones from different sources and in other cases a characteristic color was ascribed to ~~the~~ each drift. Such expressions as "typical Kansan drift" or "Iowan bowlders" are common. For the most part no explanation whatever was attempted of these phenomena but they were evidently supposed to be of diagnostic value as are lithologic differences between some marine formations. There is no question but that ~~such~~ differences actually exist in some places, as for instance where the deposits of successive stages were formed by different glacial lobes or where the later ice advanced over glacial lake sediments. But in states like Iowa and Illinois, where the several ice advances followed the same path and derived their debris from the same formations, it is far more plausible to suppose that the differences in the tills are mainly due to ~~differences~~ various conditions of postglacial weathering, excepting only the progressive increase in stoniness mentioned above.

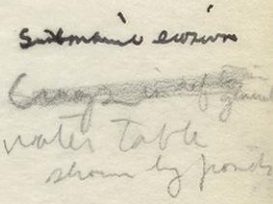
lakes, and marshes
Falls, ~~and~~ rapids, in the Driftless Area. It is commonly assumed
lakes, ~~and~~ marshes
that there are no falls ~~or~~, rapids, in ~~the streams of~~ the Driftless Area. As a matter of fact, there are a number of such, for instance Black River Falls at the city of that name, Trout Falls, and Tarr Falls, both near Sparta. These were formed by the superposition of streams over rock spurs as a result of outwash and local aggradation during Pleistocene time. There are many lakes along the ~~Mississippi flood plain~~ flood plains of ~~a great number of~~ the streams of the Driftless Area; many of these are cutoff oxbows. The same areas are also very marshy, particularly the heads of aggraded valleys whose mouths were blocked by glacial outwash. In some instances local filling failed to keep pace with the outwash and lakes were formed in these valleys but most such have been drained by postglacial erosion. In addition, there are many marshes where springs issue over impervious rocks and in sink holes on limestone ridges.

Deep soil on peneplains. There are many statements that deep residual soil is a characteristic of peneplained surfaces; it is assumed that as the slopes grow less weathering ~~will~~ gained on erosion. Reflection seems to cast doubt on this conclusion for as slopes of an area grow low the water table necessarily comes close to the surface. Decomposition does not extend far ~~from~~ below permanent ground water level as is well shown by the weathering of the Pleistocene drifts. Therefore it is unlikely that peneplains could have had deep residual soils and ~~the deep soil of~~ where such are present, as for instance on the Peiedmont Plateau, ~~probably~~ they were largely formed after the uplift of the region ~~but~~ before stream dissection had been extensive.

Fossils in sandstone and conglomerate. An idea that has been widely disseminated is that fossils are rare in sandstones and conglomerates because the porosity of the rocks caused shells to be dissolved. Such solution doubtless explains the absence of carbonate shells from weathered outcrops of such rocks but field experience teaches that the impressions of shells as well as tracks may often be found in them. Complete absence of organic

remains is more probably explicable by original unfavorable conditions for life than by subsequent destruction. As a matter of fact the spaces between bowlders on a beach were favorite shelters for some kinds of aea creatures.

Faulting, jointing and folding of unconsolidated sands. It seems to have been thought by many persons that faults, joints, and folds originate only ~~or at least mainly~~ in firm rocks. Field experience with glacial formations leaves no doubt whatever that such features can and so form in unconsolidated sands. This fact is also shown by experiments described by Mead.



Crags in relation to glaciation. There is a widespread idea that crags are a certain sign of the non-glaciation of a region. This would be essentially correct were we only to consider the young or Wisconsin glaciation. The old drift areas of Missouri, Iowa, and Illinois are for the most part devoid of conspicious crags not because of glaciation but because the bed rocks of these regions are not of such a character as to form crags. If we study northern Wisconsin just adjacent to the north border of the Driftless Area we find many crags in such positions that they cannot be explained as nunataks around which the ice closed without overtopping them. A well-known crag of this sort is "The Monument" northeast of Kkx River Falls. There has been ample time sice the older glaciations of the Pleistocene for the erosion of deep valleys with numerous crags in rocks which can form such features.

Unconformities. Many geologists describe umconformities between perfectly parallel sedimentary formations and point out irregularities of a few inches as erosion valleys. The magnitude of the time lapse during which the kxxx region is assumed to have been land is then measured in terms of thickness of beds supposed to have been deposited at other places. To the writer this method appears to hve two fundamental weaknesses: first the rate of deposition even of the same material must certainly have differed vastly in various parts of the same sea as in different seas, second, non-deposition does not necessarily imply land conditions but may occur wherever the sea bottom is raised above wave base. It seems reasonable to suppose that the many breaks in the stratigraphic column were in large part due to this simple cause which does not involve so many land areas of such low elevation that the valleys were measured in inches!