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CYCLIC AND NON-CYCLIC ASPECTS OF EROSION[1]

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GEOLOGY has always centered around an effort to decipher records. Until fifty years ago these records were almost exclusively those of the constructional processes, conspicuously those made by sedimentation. Erosion has always been a great destroyer of records. Down to the last half century it was scarcely thought of as offering any compensation by recording a history of its own.

Within that time its records have been analyzed with increasing insight. They are made rapidly and in great abundance but always at the surface. Hence they are much like characters written on wax tablets, always destroyed to make way for newer letters. Only occasionally is a tablet discarded and buried and the writing thus preserved. Such a record is an unconformity. Erosional history mentions relatively few large facts of early geologic date, but of recent events the account is very full, even more so than the parallel account written in the language of deposition. The study and interpretation of the records left by erosion constitute the larger part of the science of geomorphology.

These records begin to have value when erosion is seen as a series of events and not as a continuous process without beginning or end or variation. It is true that erosional events had long been used to chapter, paragraph and punctuate the sedimentary record, but the post-sedimentary record was a kind of unsystematic epilogue, without plot, added merely to tell what became of the characters. From a human standpoint it contained some things of news value, but the whole was rather less orderly than a morning paper. Items of significance were not yet organized into a science.

With Powell's concept of the "base level of erosion"

[1] Address by the retiring president of the Geological Society of America at its New York meeting, December 26, 1935.

came recognition of the fact that even without submergence and renewed deposition, the work of erosion may come to definite stops. This was a great advance. It divided the whole of post-sedimentary erosion into definite undertakings, each task (if completed) beginning with an uplift and ending at base-level. Thus was the foundation laid for Davis's cycle.

In order to be serviceable the cycle concept, like any other, must have definite meaning. It is not enough that a land mass is reduced to base-level, then lifted up and reduced again. If the word "cycle" means no more than that, the word "repetition" would have done as well. The cycle finds its character in a regular round of events and changes, always in the same order. Progress is marked by changing form, and the stage attained is known by appropriate topographic features as truly as though they were hands on a clock. Many topographic features need no other explanation than merely to point out their position in the cycle.

If, on the other hand, a surface be thought of as lowered by erosion, while remaining constantly parallel to its first position, there is no propriety in speaking of a cycle. The consideration of this case may be deferred. It is first necessary to examine the results of a series of cycles.

In a series of complete cycles it is obvious that there would be no record of any but the last. The "complete cycle," however, remains an intellectual ideal, unless the area be restricted by cutting off margins where the work was incomplete. Probably no river ever ran to the sea carrying nothing but water. Anything else would indicate that somewhere in its basin the work was still in progress. The common experience is to find in a single area the partially destroyed, or newly begun, forms of several cycles, none of them carried to completion, unless it be the first. The work of the next cycle stopped somewhat short of the stage reached in the first, the third fell short of the second, and so on. Speaking only of those whose records remain, it follows that the cycles were of decreasing completeness and (presumably) in most cases, of decreasing duration. The prevalence of this observation is sometimes noted as curious, as though implying that the earth's crust is becoming progressively less stable. Reflection shows, however, that the record could never have been otherwise, since only such cycles are recorded as were followed by others less complete. The limitations of the sedimentary record, inherent in the method of its making, have often been remarked, but there is less consciousness of the principle that erosion is limited in this respect, that it can record only a series of diminishing cycles. Another Schooley cycle would wipe out the whole story.

It is unnecessary at this point to explain at length the exceptional effects of local base-levels. It is obvious that the principle here stated is true only when the same area is affected by all the cycles, *i.e.*, in a succession of regional uplifts. There are many local cycles and local peneplains related to local base-levels, each independent of the others. These are not considered in the principle just stated. But the assumption that a series of peneplains reflects changes of base-level throughout the area concerned carries with it the assumption of an accelerated succession of diastrophic events. A very long series would seem to involve serious issues on which the geophysicist may well ask to be heard. If a dozen or a score of base-levels be evidenced, the probability becomes very great that most of them were local. With respect to the Appalachians this same conclusion has been reached by other lines of reasoning.

In considering the probability or improbability of numerous cycles, or a multiplicity of peneplains, another principle must be taken into account. Valleys near the sea reflect very promptly the effects of small ups and downs which remain forever unrecognized in the great interior. Davis saw this nearly 50 years ago and wrote:

> The cutting and filling resulting from comparatively brief and trivial elevations and depressions [near the sea] make a record so complete and so complicated that its details encumber the problem and place its solution out of reach for the present.[2]

With due attention to the two principles here mentioned, the vast proliferation of cycles and peneplains between the Hudson and the Potomac may be in part removed from controversy. Yet not entirely, for geologists are still human beings, and the subjective factor is always present in their conclusions. Even when the objective data are agreed upon, the pattern seen in the mind's eye may be one thing to one man and something quite different to another. A simple and familiar illustration of this is the projected profile, on which one man may see a flight of stairs (say six peneplains) where another sees only a general slope, interrupted by fortuitous ups and downs. If half a dozen hills have approximately the same height, one sees a peneplain, the other mere coincidence. Such subjective differences are perfectly normal, and neither type of mind can afford to scorn the other. Time will bring about a slowly emerging consensus of opinion which will probably be right.

The interpretation of erosion in terms of cycles is based largely on the recognition and identification of peneplains. The concept of the peneplain is by no means so well defined as the offhand and frequent use of the term might seem to indicate. In exact discus-

[2] W. M. Davis, *Bull. Geol. Soc. Amer.*, 2: 577, 1891.

sion the user of that word must still state what he means. Whatever else the term stands for, it certainly designates the ultimate or penultimate stage of the normal cycle, but the amount of residual relief allowed is determined subjectively by every man for himself.

A peneplain is "almost a plain" but there is no single and convenient word that designates "almost a peneplain." Such a word is sorely needed. For lack of it the term "peneplain" has come to be overworked, being used with more and more allowance for imperfections. This is because poor peneplains are more numerous, and perhaps "almost a poor peneplain" is still more abundant. Yet even this last may be very arresting and very significant when it constitutes the surface of a highly complex structure of great magnitude. If successful inquiry could be made into what lies in the back of the minds of those who speak of peneplains, it would probably be found that the only impression always there is that the likeness in altitude among divides is sufficient to suggest a common control and that no other control than a possible base-level is obvious. This is a very vague commitment. In mountain countries like the Sierra Nevada, such a surface, or the imagined generalization of such a surface, becomes a plane of reference above which mountains are seen to rise and into which valleys are cut. It may safely be asserted that any degradational surface so thought of is bound to be called a peneplain, even though its own relief is many hundreds of feet. That is, the term will be so used until some other short and euphonious term takes its place. This was not the intention of the maker of the term and is not here defended as right, but only pointed out as fact, even a regrettable fact.

At all events, no peneplain was ever flat. Even on the Schooley there were local swells, ridges and hills, and probably the major valleys were several hundred feet lower than the major divides. Much criticism and not a little cynicism toward peneplains has been aroused by expecting too much. It is not in their nature to be flat. The outcropping edges of strong formations, like the Tuscarora in Pennsylvania, the Berea in Kentucky, the Burlington in Missouri, the Niagara in Wisconsin, the Winslow in Arkansas and the Chase group in the Flint Hills of Kansas, may continue almost indefinitely to make low swells or subdued cuestas on peneplains. When the plains are lifted up and the escarpments again sharpened, the surfaces on opposite sides of the escarpment are mistakenly assigned to different cycles. Thus the Highland Rim and Lexington peneplains are made two instead of one. The same is true of the Salem and Springfield uplands in the Ozarks. And the Driftless Area of the Upper Mississippi is allotted an undue number of cycles and peneplains.

Both in the original intent and in current usage the term peneplain connotes a mode of origin quite as much as of form. There are other ways of producing plains perfect or approximate. A generation ago it was still necessary to discuss the criteria which distinguish the plain of marine planation from that of subaerial degradation. Whatever the difficulties of practical application, there is in this case no confusion in thinking, hence no need for discussion.

But if confusion of thought is no longer to be feared in the case of marine planation, it certainly is in another, that is, in the case of stream planation. The lateral swing of a meandering stream makes, not a peneplain, but a flat. Such a surface is not made by wearing down but by sawing off. The two processes are wholly and essentially different. So are the resulting forms, despite their superficial resemblance.

It is true that these two types of topography are often associated. It is normally to be expected that when a region is worn down to low gradients, its streams will meander broadly. Lateral planation and alluvial plains will be extensive, but these, taken by themselves, are not peneplain in any exact or technical sense, while the worn-down areas between them are true peneplain in their own right. Speaking geographically and with reference to large areas, the flood-plains may be included, just as peat bogs are included in a ground moraine, but peat bogs are not moraine, and flood-plains are not peneplains. To assume the complete planation of an area by the meandering of its streams and merging of its flood-plains is simply to dispense with the idea of peneplanation altogether and to substitute another process. Yet even in recent literature, and in the work of able geologists who are morphologically minded, there is seen, now and again, an implication of identity of the peneplain and the alluviated planation surface. An expression like the following is typical: "(Streams) began at once to lower their channels in the old peneplain, and, when they had reached the new base-level, to form a new peneplain by lateral corrasion."[3] When it is remembered that planation surfaces are relatively numerous, the consequences of treating each one as a peneplain will readily be seen.

In this connection attention should be given to the association of peneplains with gravel. Gravel is to be expected in the alluvium associated with a peneplain as in any other alluvium on a suitable grade, but within that area that is typical peneplain and not something else, nothing is scarcer or less expectable than gravel. When a surface, suspected on other grounds of being a peneplain, is found to contain patches or strips of gravel at about the general level, the evidence of peneplanation is strengthened. But

[3] C. W. Hayes, *Nat. Geog. Monograph* No. 10, p. 330, 1906.

if the entire area is gravel-covered the assumption is that it is a planation surface, made by a stream which may or may not have been flowing on a peneplain. Generally the gravel-free surface is much the larger part of the area. This is about equivalent to saying that search for gravel must be limited to those parts which are only peneplain by courtesy, *i.e.,* by reason of the company they keep. Gravel may corroborate the suspicion of a peneplain, only when its elevation is approximately that of a more extensive surface. Alluvium, whose nature bespeaks a very low gradient, affords presumptive evidence, not proof, of a peneplain.

This matter is important, for it bears directly on the multiplication of peneplains by confusion with river terraces. The latter have their own value in giving evidence of a changing base-level and thus of interrupted cycles of erosion. They have their uses in the study of diastrophic history, but the time intervals which they record are relatively short, and generally they have nothing to do with completed cycles.

The confusion of flood-plains with peneplains is in many cases a matter of terminology. Rarely would the one be mistaken for the other. It is otherwise with structural plains. So impressive is the cycle, so vastly useful and illuminating in the study of history and topography, so widespread is the evidence that land masses have been reduced to peneplains, that almost any flat horizon is under immediate suspicion of owing its flatness to a former base-level. One can scarcely be surprised if such control is sometimes invoked where the flat horizon is in reality due to some other cause or condition.

Foremost among these other conditions is the presence, immediately beneath the surface, of a strong stratum which may have arrested erosion at that level. Where such a relation is suspected geologists at once line up, those who are cycle-minded opposed to those who are structure-minded. These traits may be congenital or they may have been instilled, but whether one or the other they are permanent factors in determining the judgments of their possessors. Often they fix the judgment in advance and forestall investigation. A man simply belongs to one party or the other, just as he is a Republican or a Democrat, or (if you are a reader of Alphonse Daudet's "Tartarin of the Alps") he belongs to the party that takes prunes for dessert or the party that takes rice. Debates will go on indefinitely between the cycle-minded and the structure minded, just as between individualists and socialists, or between any other groups whose line-up is fixed by sympathies. In the end each group will say that the other needs to study the question in the field, a kind of final thumbing of the nose with which geologists end a hopeless argument.

It seems highly probable that the influence of resistant strata has often been underrated (and the number of base-levels correspondingly overestimated) especially in dissected plateaus. It is difficult to read the literature of the Appalachian plateaus and avoid this impression. The reason for limiting this statement to dissected plateaus and excluding extensive uncut stratum plains will appear later. From the most recent treatment of the Appalachian plateaus in Pennsylvania, it might be concluded that altitudes owe more to two Carboniferous sandstones than to any succession of cycles. Whether or not this statement is too strong, it calls attention to the fact that the attempt to divide up a great area and assign each and every segment to the work of some particular cycle runs into grave difficulties and must be so compromised as virtually to be abandoned. Much of the area, even where the skyline is flat, may be only remotely related to any peneplain.

The simplicity and beauty of the conception of allotting all parts of an area to their respective cycles is alluring. So much so that we are prone to think in terms of diagrams, in which each higher level gives way visibly to a lower and younger surface, a newer peneplain which is constantly enlarging at the expense of the older and constantly losing by the spread of still newer and lower surfaces. The conception embodied in such diagrams is so simple, so illuminating, so useful, in many cases so true, and it burst so suddenly upon the science, hitherto without it, explaining so many things, and introducing order where chance had reigned, that it can not be wondered at if its application was, for a time, made too broad.

The recognition here given to structure as a determining factor in horizontal surfaces is believed to be a just concession. No service can be rendered to the cycle theory by overstraining it. But explanation in terms of structure can also be overstrained. To minds of a certain twist there is something hypnotizing in the presence of a structural surface which happens to parallel the topographic surface. The two seem to be necessarily related, like the unexplained sounds in a strange room and the number 13 over the door. The fact that structural surfaces must have *some* position is apt to be overlooked. More of them are horizontal or nearly so than in any other one attitude. The same is true of topographic surfaces. By the mere law of hazard the surface must often parallel the structure. Yet the discovery of such agreement is often treated as sufficient evidence of cause and effect. Sometimes this is true, as every student of plateaus knows. Often it is not.

There is such a thing as a plain of stripping, quite independent of base-level, but the limitations of stripping at high levels are very severe and are often ignored. To do a clean job of stripping a horizontal bed is just as difficult as to make a perfect peneplain. The process is the same, and its last stages are just as slow in one case as in the other if equal areas be assumed. The time required increases enormously with the extent of the area. Meantime the margins, stripped early in the process, must hold their own against dissection and this dissection is a very speedy process if the relative altitude is considerable. The result is that while rock terraces and mesas of limited extent are common, extensive plains, stripped at considerable altitudes, are in all cases subject to dispute. The stripping is not denied; only the altitude at which it is done. The question at issue pertains to the ability of any hard stratum to maintain a local base-level high above the sea, during the long time required for peneplanation.

A familiar example is the Edwards Plateau in Texas, 1,000 to 3,000 feet above the sea and 400 to 1,200 feet above its surroundings. The underlying limestone formations, collectively known as the Edwards, are relatively resistant in the climate of central Texas. No doubt this is the explanation of the plateau's present height above the lowlands on weaker rock. But it does not follow that the stripping was done at that altitude. The margins of this formation are being raked and shredded in a way to show the precarious position of the entire mass at its present altitude. In view of the present havoc (obviously of recent beginning) one wonders how the margins of the plateau retained their flatness during the long time required to strip the interior. If unstable now, why were they not unstable then? And why is the process of destruction still in its early stage? The evident answer is that the stripping was not accomplished at the present altitude.

This region is described in the classical paper of Hill and Vaughan.[4] In descriptive terms the limestone surface is said to be stripped, but it is also pointed out that the several beds are beveled, the surface being here on one bed and there on another. This is one of the tests, if not *the* test, of a peneplain. To say that the Edwards Plateau has been stripped is a mere statement of fact; but to leave the inference that the strength of the limestone explains the flat surface without regard to the control of base-level during erosion is wholly unwarranted.

An even more striking illustration is seen in the "Great Sage Plain," 6,000 to 7,000 feet high in southeastern Utah and southwestern Colorado. Its agreement with the surface of the Dakota sandstone is noteworthy. It is mentioned as one of the best American examples of stripped plain, and generally with the implication that the strong Dakota sandstone was, in this case, an adequate substitute for base-level. However, it is only necessary to look at the sharp canyons, already branching into the interior, to be convinced that the Great Sage Plain can not last long under present conditions. Its prompt dissection will be a brief episode in comparison with the long, tiresome process of washing away the last stains of Mancos shale from the still flat surface. Obviously this slow stripping process must be allowed a long handicap and must have run most of its course before dissection was allowed to start.

It should be a safe principle that a peneplain can not originate under conditions which make it essentially unstable. So slow a process as peneplaning must not be asked to run a race against so swift a process as dendritic dissection of a high plateau. Applying this principle to the Great Sage Plain, it is safe to conclude that, at the time of its development, the altitude was much less than at present, certainly not high enough above the local base-level to make dissection possible.

Within limits of altitude and of time, a strong stratum may become a substitute for base-level, but these limits are, without doubt, much narrower than are implied by many casual descriptions of stripped plains. The strong stratum may actually raise the local base-level a little for a long time, or may raise it much for a short time, but not much for a long time. It follows from this that, in terms of diastrophic events, the interpretation of an extensively stripped horizontal stratum, now at considerable altitude, differs very little from that of a peneplain. Each was necessarily developed at a lower altitude.

What is said here of the stripped or denuded stratum applies equally to the exhumed or resurrected peneplain. The mere fact of exhumation implies that the rocks below are strong. The discovery that the present surface agrees essentially with one of pre-Cambrian time is sometimes hailed as proof that the present nearly flat surface is not a true peneplain. This may be true if the patches concerned are small. Barring this limitation the more recent plain may be treated much like a peneplain in its own right; that is, it may be so regarded in the interpretation of diastrophic history.

In view of what was said before about structural control, expressing the belief that it has received something less than its due in the Appalachians, this may seem to some like blowing hot and cold with the same breath. Structural control is sometimes found to be adequate and sometimes not. If difficulty is found in stripping ten thousand square miles but not in stripping ten square miles, a single strong stratum may be made to dominate the horizon over a large area by

[4] U. S. Geological Survey, Eighteenth Annual Report, Part 2, 1808.

the simple expedient of dissecting it first and stripping afterward, or allowing the two processes to go on simultaneously. Such an explanation is not necessary for most of the Appalachian plateaus, at least not until some one disputes the fact that they were largely or approximately base-leveled in the Schooley cycle. But it helps in minor ways, as in the avoidance of an older and higher base-level on the Harlan sandstone in the Cumberland Mountains.

The problem of accounting for a multiplicity of surfaces (topographic or mathematical, actual or conceptual) at different levels is a main, if not *the* main, center of controversy in most discussions of cyclic history. The above remarks on the influence of structure bear on this subject. But there is another factor, recognized though not yet prominent in discussions, which may produce surfaces at an indefinite number of levels. This is the slow wasting of a surface without change of characteristic form. This is no new discovery. It was tacitly assumed before the cycle was born. It would scarcely be going too far to affirm that the coming of the cycle, with its more exact concept, its orderly sequences, its concreteness and its exemplification in known forms, has done much to withdraw attention from a vague, universal, unobservable process whose results show poorly in diagrams. Yet such a process is going on, and it is lowering most of the earth's surface, sometimes in association with valleys, sometimes without.

Such erosion, without systematic change of form, is essentially non-cyclic,[5] for the cycle produces and reproduces a series of events and forms. One stage is not like another. There is a beginning, a climax and an end. Cycles have parts, and the parts make wholes, and the wholes may be counted like apples. Non-cyclic erosion can only be measured like cider. There is neither part nor whole but only much or little. The exact altitude of a surface affected by such erosion does not record an event, but only a continuing process which may be fast or slow. Obviously surfaces under such conditions may have almost any altitude. A mere count of altitudes would mean little in terms of events.

It may need emphasizing that the cycle itself is not a physical process but a philosophical conception. It contemplates erosion in one of its aspects, that of changing form. But erosion does not always and everywhere present this aspect. This generalization is not apt to be denied; in any case the exact physical process does not concern us here. It is sufficient to say that the potentialities of erosion without producing valleys will bear far more emphasis than they have received.

So constant is our association of valleys with erosion that it is difficult to think of the straight, horizontal Appalachian crests as being lowered scores or even hundreds of feet and yet looking the same after as before. The fact has long been acknowledged, though not until recently has it been given much significance. Hayes was first to suggest a possible 300 feet (in the south). Recently Ashley[6] has made a minimum estimate of 100 feet in a million years.

Considering first the case of narrow ridges like those of the Ridge and Valley province, it is to be observed that this process of surficial wasting does not destroy the horizontality of a crest but only lowers it. If the amount of such wasting were everywhere the same, the record of cyclic erosion would not be defaced and the count of cycles and peneplains would not be confused. Confusion begins when one ridge has been lowered 30 feet and another 300 feet. Both crests received their flatness at the same time, *i.e.*, when both summits were parts of the same peneplain. Neither one has at any time lost its flatness. Both are lowering now as fast as ever and neither is at base-level. Yet a casual view, and perhaps the present vogue, would assign them to different cycles with the tacit implication that neither summit has been lowered since uplift and that the summit plane of each cuts the mass now just where it did when the peneplain was made.

Theory would indicate that the rate of erosion without valleys should vary with the hardness of the rock and the width of the outcrop, the latter being determined by thickness of stratum and dip. Even a casual examination of the Appalachian ridges is sufficient to indicate that such correlations of altitude with structure exist. Thick strata make higher ridges than thin ones, and the ends of pitching folds where the outcrops are broad are almost invariably high. Much ingenuity has been expended in depicting a series of base-levels so that each mountain crest may fall in one of the assumed planes. When an equal amount of exact study shall have been given to correlating each height with the character of rock and the breadth of outcrop, the time will have come to decide how many base-levels must be assumed. Perhaps three would be enough, or two; the last and extremest suggestion is one. More than three may be needed.

Application of the principle of constant, universal and unequal degradation of crests is based in part on actual observation and correlation. The inherent probabilities in the case are almost equally deserving

[5] The substance of what is said here about the non-cyclic aspect of erosion was presented by the writer before the Chicago Geological Society on March 8, 1933. The same principles were applied to the interpretation of the Allegheny Plateau in a paper read before the Geological Society at its meeting in Chicago in December, 1933, and published in abstract on page 78 of the Proceedings of that year.

[6] Geo. H. Ashley, *Geol. Soc. of Amer. Bull.*, Vol. 46, 1935.

of attention. But such a consideration may also save us from going too far and limiting the number of cycles too severely. Judging from the sedimentary record it is inherently probable that cycles of all grades and degrees of completeness should appear in the post-sedimentary record. Indeed, short aborted cycles, resulting in local peneplains on soft rocks, are much more probable than such a remarkable occurrence as the Schooley cycle. It is not strange that such an exceptional event as that left the most convincing records. Yet logic would scarcely permit us, on that account, to accept the reality of the extraordinary and not to acknowledge the ordinary. A Harrisburg cycle, having some such importance as custom ascribes to it, would be quite in line with the expectable.

A vivid portrayal of the effects of unequal wasting is seen in the slant of some ridge crests (not the axes of plunging anticlines) which can not be assigned to any one peneplain, one end perhaps agreeing in height with an older base-level, and the other with a younger. What we know with absolute certainty about the Ridge and Valley province is that there are ridges or parts of ridges at all possible heights from minimum to maximum; there may well be more than one height that seems to be specially favored. This is presumptive evidence (though not proof) of more than one base-level.

Turning now to plateau surfaces, unexpected reenforcement of this principle of erosion without change of form has recently come from our newborn concern with soil erosion. It has long been known that gullying of fields is one of the major wastes of our civilization. Now it appears that even this widespread and spectacular disaster is a secondary matter compared with the skimming of top-soil with no observable change in topographic form. Bennett affirms[7] that in addition to the 50 million acres of gullied land in the United States, and another 50 million acres "about as bad," there are 125 million acres (nearly 200,000 square miles) all or most of whose top-soil has been carried away in the short period of cultivation. The Soil Conservation Service is undertaking to determine by direct observation the rate of such soil wastage without gullies. As a check on their methods and results it may be noted that the ascertained rate of wastage of moderately steep ungullied grass-covered slopes on loess in Missouri is not very different from the computed rate for the entire Mississippi basin, based on the observations of Humphreys and Abbott on the annual load of the Mississippi River. The rate of erosion is of course excessive on ploughed fields. But the actual rate of such lowering is a secondary matter. No application of this principle to geomorphology assumes more than a minute fraction of the higher rates observed in soil studies. Even at Ashley's rate of 100 feet in a million years the Allegheny Plateau would have lost a bare eighth of an inch since its settlement.

It is unnecessary to say that this erosion without valleys is in part dependent on steepness of slope. After making due allowance for the work of wind and solution, a perfectly flat upland should be narrowed rather than lowered. This means that the altitude of a plateau in its pre-mature stages is not significantly reduced. From maturity on, all crests are melting down concurrently with the wasting of slopes. With a stable base-level, lowering of crests and flattening of slopes would proceed together after the familiar pattern of the plateau cycle. With continuous or intermittent uplift of suitable amount, the steepness of the slopes and the depth of the valleys may continue unchanged while divides are pared down hundreds of feet. There is no theoretical limit to the amount of uplift and erosion that a maturely dissected plateau may undergo without change of characteristic form. Meanwhile, in another portion of the same original plateau, but consisting of harder rock, erosion may have lagged behind uplift. The result will be two adjacent plateaus, differing in elevation and, according to customary interpretation, representing two cycles.

In its application, this principle is related to that of stripped plains, but the two are not identical. The essence of this latter view is that all planes are being reduced at all times, but some faster than others. Indeed, computations or estimates of rate made by geologists have been based almost wholly on the wasting of the more resistant rocks in mountains.

The exact rate at which this general wasting proceeds is not a matter of primary concern. The suggested rate of 100 feet in a million years is somewhat slower than the general wasting of the Mississippi basin. The important consideration is that, if the rate of lowering is any considerable fraction of this amount, no elevated surface older than Pleistocene is properly interpreted without taking this factor into account. If all surfaces were lowered at the same rate the remains of former peneplains would still stand in their true relative positions. But no one will assert that all surfaces waste equally. The result is that all correlations based on altitude are liable to error in proportion (among other things) to the antiquity of the surfaces concerned. Before correlation, all readings must be corrected by an amount dependent (to say the least) on time, slope and resistance. This correction is not a minute Einsteinian matter. For features dating from Miocene time it may well run high into hundreds of feet. To compute such correc-

[7] H. H. Bennett, SCIENCE, 81: 322, 1935.

tions for peneplains older than late Tertiary is an arduous task. To correlate without them should be classed by law as among the dangerous occupations.

It is no reflection on the cycle to point out that its records are subject to complication with those of another process. Rather, it should be apparent that, when the complicating factor has been properly evaluated and allowed for, the record of cycles will be less confused and more trustworthy. It seems appropriate to speak of this other factor as essentially non-cyclic. It matters little whether the terminology here used be liked or accepted. It matters much that the facts be recognized. For want of such recognition the cycle is burdened with so many complexities and inconsistencies as to impair its usefulness or even at times to expose it to unfriendly criticism.

THE NATIONAL MAPPING PLAN OF THE NATIONAL RESOURCES BOARD

By Dr. WILLIAM BOWIE

CHIEF OF THE DIVISION OF GEODESY, U. S. COAST AND GEODETIC SURVEY

FOR many years scientific men, including engineers, have realized the importance to a country of having a complete knowledge of its terrain and all physical data relating to the earth's surface. Without basic facts good plans can not be made, and they surely can not be carried out with any degree of effectiveness.

It is recognized that the topographic map shows graphically essential data regarding the area covered. It would take years of traveling or of reading in order to obtain a knowledge of an extensive region, while with a topographic map much more comprehensive and accurate knowledge can be gotten in a few hours.

Not only is the topographic map of value in planning the industrial and commercial activities of our people, but it is essential in many lines of scientific research. The configuration of an area has a bearing on plant and animal life. Without a knowledge of the terrain geological and geophysical investigations can not be carried on with efficiency and accuracy.

In spite of the need for maps, very little has been done. Only about 47 per cent. of the area of the United States, about three million square miles, has been covered by topographic maps and more than half of those maps are so out of date or so sketchy in character that they do not meet present-day needs. We thus see that only about 25 per cent. of the area of the country is adequately mapped; in fact, many of the otherwise satisfactory maps must be revised to show changed cultural features.

The National Resources Board, an agency set up by President Roosevelt to advise on the conservation and utilization of our resources, requested the Federal Board of Surveys and Maps to prepare a plan for completing the mapping of our country. This was done late in 1934. This plan was endorsed by the National Resources Board and was forwarded to the President in one of its reports. The opening paragraph of this report is significant:

> Most of the land planning and land use agencies of the Federal Government, as well as many other Federal and State organizations whose activities are concerned with land, have asked the Board of Surveys and Maps to prepare a program for the completion at an early date of the mapping of the United States. The Board has made an exhaustive investigation and finds much evidence that the actual loss of money due to lack of adequate maps is greater than the estimated cost of completion of the standard map of the United States. Moreover, most of the land use agencies have testified that the absence of adequate map data makes it almost impossible to carry out any plan of readjustment in land use until the areas affected are adequately mapped.

This report in turn has been commented on favorably and given endorsement by the Science Advisory Board of the National Academy of Sciences.

The national mapping plan was considered of such interest by the American Society of Civil Engineers that it was printed in full in the February, 1935, issue of *Civil Engineering,* a journal of that society.

Many engineers, geologists, biologists and others have expressed great interest in the national mapping plan and have expressed the hope that it might be put into effect immediately and carried on vigorously. It calls for the completion of the topographic mapping of the country within ten years at an estimated cost of $117,531,000 or less than $12,000,000 per year. Congress had already authorized the mapping of the country as a federal project in the so-called Temple Act, which became a law on February 27, 1925. That act authorized the appropriation of funds with which to complete the map of the country within twenty years. Ten years have passed and little topographic mapping has been done during that time. Forty-three per cent. of the country had been mapped before 1921, while to-day only 47 per cent. has been mapped. This is at the rate of 0.3 of one per cent. per year. One can see that the mapping will not be completed within a hundred years at this rate of progress, and besides,