cause of inaccurate usage or because of insufficiency in the light of growing conceptions, or for both reasons. This phenomenon is exemplified in the history and present needs of geomorphic nomenclature.

When the conception and the word, peneplain, were simultaneously introduced by the American founder of geomorphic science, Professor William M. Davis,[1] the conception, which this word was manifestly intended to cover, was the land form produced in the penultimate stage of the erosion cycle, the approximate completion of a cycle of erosion over great areas and on large land masses.

Further study of land forms long ago revealed the truth that cycles of erosion are not all of like duration; they may be terminated at any point of progress toward ultimate completion. Where traces of the forms produced in these earlier stages have been preserved, geomorphologists have been content to call them partial peneplains, and more immature forms of still earlier stages have been called terraces or, more recently, straths.

Terrace is too useful a term in its unrestricted meaning to be withdrawn for such a limited and technical use. Strath is defined in the Standard Dictionary as follows: "(Scot.) A wide open valley, usually a river course; distinguished from a glen." This has been its usage by Geikie in "Scenery of Scotland" (p. 156). It is scarcely admissible to use with a new significance a geomorphic term which has a prior and different usage in Great Britain, namely, to designate the broad valley floor unrejuvenated. Several years ago the writer, feeling keenly the need of a term that should not put limitations on so useful a word as terrace (or as bench) and that might be given a restricted technical significance, approached M. R. Campbell and Laurence LaForge of the U. S. Geological Survey for suggestions. The discussion that ensued resulted in the selection of the word *berm*.

Berm is defined in the Standard Dictionary as follows: "*Civ. Eng.* A horizontal ledge part way up a slope; bench. *Fort.* A narrow level space at the outside foot of a parapet, to retain material which might otherwise fall from the slope into the ditch." It is suggested that this term be given a geomorphic usage; it should be used to distinguish those terraces which originate from the interruption of an erosion cycle with rejuvenation of a stream in the mature stage of its development. Dissection, following upon elevation of the land, will leave remnants of the earlier broad valley floor of the rejuvenated stream as a terrace, or berm, and remnants of the uplifted abrasion platform as a seaward-facing terrace, or berm. In different localities every gradation between relatively narrow berms and widely developed peneplains may occur. Considerable latitude should be given therefore in the use of this term, so that it may include berm-like forms as well as typical berms; while those forms more nearly approaching the peneplain might be called partial peneplains.

[1] "The Physical Geography of Southern New England," *Nat. Geog. Monograph*, 1: 276, 1895.

Such a distinction as the following between berms, partial peneplains and peneplains might be considered: berms, paralleling streams and sea-coast, only cross divides on weak formations; partial peneplains cross divides on rocks of median resistance or on decayed resistant rocks; peneplains are wide-spread on resistant as well as non-resistant rocks.

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LORD KELVIN'S "MORTAL SPRING"

WHEN I began the study of the calculus, using Church's "Elements of the Differential and Integral Calculus," I was greatly bewildered by the reasoning leading to the central principle of the subject, the differential coefficient, latterly the derivative. How a quantity, *dx*, for example, could be something in the first member of the equation and nothing in the second was a great mystery, and the statement that such was the fact came as something of a shock to one who had begun to associate clarity and rigor with all mathematical processes. It looked, to a novice, suspiciously like smuggling approximation methods into a territory where exactitude alone is permissible.

Since Newton was one of the founders of the calculus I turned to him for light, and examined with care his method of passing, using present-day symbols, from $\frac{\Delta y}{\Delta x}$ to $\frac{dy}{dx}$. After considerable labor I was forced to the conclusion that the passage was made by a flash of intuition, and not by so-called logical rigor. His mind had been prepared, of course, for this great insight by deep and long-continued reflection upon the behavior of variable magnitude. This helped me out, for what was good enough for Newton should, surely, be good enough for me. I proceeded at once to apply the new instrument to the solution of interesting and important geometrical and physical problems, with gratifying results. Confidence in the validity of the processes was quickly and firmly established. My satisfaction with this method was strengthened by the discovery that Comte in his later years veered to the view that there is a transcendental element in the calculus which renders all attempted demonstrations alike irrational and futile. The laws of the calculus, like Newton's laws of motion, are to be accepted because in all their applications they are always found to agree with the facts of experience. Even quite recently the late Sir Oliver Heaviside, a most original mathematician, committed himself to the opinion that "within the last twenty-five years we have erred in attempting to lead the student to a working knowledge of the calculus by first convincing him that the reasoning is sound. It is quite possible that the fundamental principle of the calculus does not admit of deductive demonstration." I gained an added appreciation of the sage suggestion of the Autocrat at the Breakfast Table that a mathematical demonstration is often a *pons asinorum* over chasms which shrewd folk can bestride without the aid of such a structure. I also found this an advantageous starting point for an examination of the devices for bridging the chasm separating the quotient of increments from the quotient of differentials proposed by D'Alembert, La Grange and Weierstrass and his school.

These early experiences were brought back into the memory with vividness and force when, the other day, I stumbled upon an interesting passage in an article in the *Contemporary Review* for June, 1918. The author of the article is the Rev. Dr. D. S. Cairns, an eminent Scottish divine and professor of dogmatics in the United Free College, Aberdeen. Principal Lindsay, to whom reference is made, was for many years at the head of the United Free College, Glasgow. The quotation follows:

> The great and dramatic moments in the progress of science are when its pioneers, after long brooding over the data which set their problems for them, leap far ahead of all verified knowledge and divine the solution, when Newton goes ''voyaging through strange seas of thought, alone,'' when Darwin sees his unifying truth in a south country lane, and Wallace, ill with fever in the southern island, is ''stung by the splendour of a sudden thought.'' The story of the last century is full of such records, and it is not too much to say that the whole fabric of modern science and industry rests upon the truths discovered in such inspired moments. Let me add another not generally known to these histories. My friend and colleague, the late Principal Lindsay, once told me that Lord Kelvin told him that he never thought his way quite up to any one of his great discoveries. He said that he brooded over the facts, which set him his problem, until there came a moment when his mind made a mortal spring out beyond any thing that he, or any man, could demonstrate, and that he knew then in the very marrow of his mind that the solution lay in a certain fact or set of facts. He said further, and this, I think, is of peculiar interest, that he was never able himself to supply the intervening steps, and that before he announced his discoveries he always got Tait or Clerk Maxwell to work out these intervening steps for him. I repeated this story once to two or three distinguished biologists, one Scottish and the other Continental (both of them, by the way, Gifford Lecturers), and they said at once that that was how the great discoveries of science were always made, that the end was seen before the means.

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ON "THE NEW CYTOLOGY"

IN the March 20th number of SCIENCE Dr. Alexis Carrel has summarized in a general statement of its problems the principles and methods of the new cytology. In particular he emphasizes the point that "structure and function are two aspects of the same thing" that "must be considered simultaneously." As methods to such an end he describes in some detail his use of tissue culture and also states: "There are two ways of preventing the death of tissues and organs removed from the organism. One was originated by Ludwig and the other by Harrison. Ludwig supplied the blood vessels of an excised organ with artificial circulation of a proper fluid." And after describing the difficulties of this technique, adds, "the old method of the physiologists of the nineteenth century is being rejuvenated, and may become one of the most useful tools of the new cytology."

The purpose of the present communication is to call attention to just such an application we have made of Ludwig's method to cytological problems. Our specific problem was that of experimental nephritis, and in its investigation the method of perfusion was applied to the frog's kidney. In the first article of two which will appear shortly in the *Journal of Experimental Medicine,* we describe the functional disturbances of the kidney lesions following the administration of renal poisons not, as is usually the case, to the living animal, but directly to the isolated organs by way of the modified Locke's solution with which they were perfused. In the second paper the structural changes in the perfused tissues are considered and, to quote from our conclusions, "the two aspects of damage (functional abnormalities and structural change) can be correlated to a reasonable degree."

The structural changes in these perfused kidneys were found to be identical with the anatomical lesions which follow the injection of the same poison into the living frog; in fact, an experimental nephritis was produced in the isolated organs. The finer cytological changes involving the nucleus, such as pyknosis, karyorhexis and karyolysis were observed as well as the protoplasmic changes which accompany cell death. Pathological alterations in the granular and mitochondrial elements of cells were particularly well reproduced in the isolated organ, as for example, "cloudy swelling," mitochondrial clumping and other elements of the classical picture of cell damage. Such changes can not be considered mere artifacts, due to the artificial character of the perfused tissue's environment, for the kidneys functioned in a perfectly