only to aim at producing certain sensations, without regard to harmony or beauty. Many scientists have considered it their task simply to analyze certain phenomena, functions or sensations, without any regard to the relationships of these phenomena to others. Other eminent scientists, however, are questioning the value of isolated observations made without regard to relationships. It is necessary to keep constantly in mind that analysis and synthesis should not be mutually exclusive, but rather mutually cooperative.

The worship of organization in this country has many dangers. To do big things, to encourage economic mass production, we form big machines, which work more or less automatically. A man at certain places in the machine performs certain motions which it can not be made to perform. But in doing this he becomes the slave of the machine; his actions depend upon those of the machine and hence become merely mechanical. In organizing hospitals, group clinics and educational institutions, there is a comparable danger. Each individual has a set task and a rate of motion imposed upon him by demands of the rest of the organization; this consumes so much of his energy that there is little left to apply to independent thought. Machines and organizations should do man's work and not his thinking; they should release his energy so that he may think. When they consume his energy and inhibit his thinking they are a menace rather than an aid to progress. Because applied science has made possible these wonderful and fearful machines with their consequent demands, many persons are asking to-day whether science, which has made them possible, has not failed in its object to better man's condition. They recognize that materially he may be more comfortable, and physically more healthy, but ask whether mentally he is superior to his predecessors and whether he has retained his freedom. We as scientists must face these questions and honestly try to determine wherein lies the fault. Have we in our zeal for analysis been trying to make science do too much? Have we allowed it to exclude certain elements present in the world and in man and as yet beyond its domain? Have we forgotten that its chief function is to answer the immediate *how* rather than the ultimate *why?* Is it not necessary to try to be artists in syntheses as well as scientists in analyses?

A strong feeling has also developed that science is not a part of general culture, but that art and the humanities represent the cultural side of man. In this attitude it is often forgotten that science in the past has played an important rôle in providing material for the artist. Knowledge of man's body, of his environment or of the universe is no less a part of real culture than is knowledge of man's past efforts. But is this exclusive attitude the fault of the humanities or of science? I venture to suggest that it is because the representatives in each field refuse to allow their own discipline to articulate or to come into contact with the other. In other words, there must be more working together with what all hold in common, and each must allow an influence to be exerted by what the other possesses as peculiarly his own. We must try to be artists in our syntheses and attempt to develop harmony from our mutual efforts. A few lines and pigments properly applied may produce a more effective picture than thousands of lines and much paint; on one page a poet may express a truer relationship than a scientist does in a monograph. The great requisites are the proper selection of material and imaginative synthesis to express what the artist sees. All human activities must mutually influence one another by expressing truth as we best can know it. This idea has doubtless been made articulate many times, but probably never better than by Plato, who defines science as the discovery of things as they really are, and further states, "Now when all of these studies reach the point of intercommunication and connection with one another and come to be considered in their mutual affinities, then I think and not till then will the pursuit of them have a value."

And so in the field of effort called clinical investigation we should constantly keep in mind the relationships of its various elements. The science of medicine should furnish us with knowledge and a technique for acquiring more knowledge. Although the art of medicine may indicate the manner in which that knowledge may be applied it should also assist in the technique for acquiring new knowledge. Because science can give us only a partial description of our universe, art must be ever at hand to supply the deficiency. Not all the art of medicine is at the bedside, nor all the science of medicine in the laboratory. In our respective activities the skill with which we mingle the two will determine our success.

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QUANTITATIVE VS. QUALITATIVE STUDIES IN GEOLOGY[1]

IT has been quite habitual among geologists to record relative or qualitative accomplishment in geologic processes. We say "this topography is older than that," "resistant rocks weather more slowly than soft ones," "this volcano has erupted more than that," but we are not able to say how old in years or geologic periods either piece of topography is, how

[1] Presented to the Geology Section of the Ohio Academy of Science, Cincinnati, April 6, 1928.

fast either rock weathers or how long any volcano has worked to build its cone.

Qualitative studies must needs come first, and no doubt we shall do qualitative work for many years yet; but I want to show in this paper some of the places in which much more accurate studies can be carried on. Our qualitative work has been excellent and large in amount and has laid the lines for valuable deductions and stimulating interpretations, but it is time for us to turn over a new leaf in the nature of our investigations. Stimulated by chemists and physicists who have been measuring quantitatively for many years, let us greatly expand our activities in making accurate determinations of our geologic processes, of their rates of accomplishment and of their degree of continuity and variation in rate of performance.

Geologists have already done several things quantitatively. Crystals have been carefully measured and their unknowns calculated to the fourth decimal place or the seconds of arc. Chemical analyses of minerals and rocks have been made in great numbers. Mineral analyses of rocks have been done with considerable exactness. Stream measurements have been made revealing the actual velocities and volumes and changes in both elements, also showing the amount of matter carried in suspension and in solution.

For a number of years very careful quantitative observations have been made in the field of volcanism at the crater of Kilauea in Hawaii. The printed sheets issued each week may become a little monotonous, but they are telling how many earthquakes occur each day or week, how they are distributed in time, how intense or feeble they are and their probable distance from the observatory. They record the slides in the pit, and the "working" of the floor and walls of the pit, the number of millimeters cracks have widened in a week, the colors and change of colors on the sides and floor of the pit. They tell of the displacement of bench marks as the volcano heaves and writhes in its development; they state how much the surface tilts day by day both in direction and in amount down to small percentages of seconds of arc. The studies endeavor to correlate the various volcanic phenomena with each other and with non-volcanic phenomena, as with tides and tide variations, with changes in atmospheric pressure, with earth disturbances in other places. Of course this sort of study and record will catch all the spectacular phenomena, but it is the infinite detail and accuracy of these routine items that in the long run will give us a new and better understanding of volcanism at Hawaii.

We have measured the rate of recession of falls in a few instances. Every one knows of the careful work covering a hundred years that has been done on Niagara. But here the object was as much or more to develop a measuring stick for geologic time as it was to get at the rate of progress. Two or three other falls have been similarly studied.

Twice have valleys of small streams been studied. Measurements of the actual erosion occurring in a given time in a limited part of the stream and valley were made; then careful measurements of a larger section of the whole valley were made and the two results set down in ratio. Again the study was made in each case much more to acquire a measuring stick for postglacial time than to disclose just what the stream is doing in the nineteenth or twentieth century.

Nearly thirty years ago a considerable series of observations were made on artificial structures in and around greater Boston. The observer was able to find the dates of making the structures and the condition of the structure when made. He then took photos and conducted measurements of the structures and estimated what nature had been able to do in known periods of years. Walls had been heaved or bulged, slopes had crept, flagstones had been worn, monuments weathered almost to the effacement of inscriptions and scores of other changes had been made. These were accurate measurements of actual processes in geology.

But the very fact that we can and do single these little programs out and thus exalt them is ample evidence that we are not in the habit of thus seeing quantitatively.

Why shouldn't we measure scores of falls in many kinds of rocks and under different climatic conditions and leave our records for some successor? Then he could repeat the measurements and from the two series deduce conclusions. Why can't we measure many postglacial, as well as other, streams with their valleys in drift or in coastal plain rocks or even in crystallines and file our results with some permanent institution, and let some one else repeat the measurements in fifty years and check up on rates of work? This study need not be done solely to get a time measurer but to learn rates of work.

Nearly twenty-five years ago a member of the United States Geological Survey spent some time with surveyors in the Great Lakes region tying in a series of accurate bench marks with the lake levels. He has gone on. His records are in the survey archives. I believe the plan is to have the bench marks and lake levels again surveyed in some twenty-five or twenty-six years more and the results checked against the earlier survey. It is said that if there is a tilt of an inch in one hundred miles in any direction these observations will detect it. Then we shall know if the Great Lakes area is in motion in the first half of the twentieth century; the direction and amount of motion if detectable will also be made manifest. Now why not do this sort of thing in many places? Shore-lines of long bays, the Baltic, Adriatic, Gulf of California and Chesapeake Bay would be serviceable.

Dr. Karl Sapper said not long ago, "The most important goal for the future of volcanology is to obtain new collections of facts and make them available, for only by such procedure can a substantial basis be made for more satisfactory interpretations than have hitherto been invoked." Why not extend the careful quantitative study of volcanoes now under way at Hawaii to at least a score of volcanoes in different types of regions, with diverse character of eruption, other kinds of rock magma and other relations to land and sea? Such a series of observations over a period of fifty years would help to reach Dr. Sapper's suggested goal. How can we expect to make much further progress in volcanology on qualitative observations and these limited to a half dozen volcanoes? We are not cultivating the field anything like as intensively as many of our agriculturists work theirs.

What do we know about viscosity of lava? Several statements from recent works are quoted below. "Lavas low in silica may flow like water, lavas high in silica are more viscous." "As cooling progressed viscosity or stickiness of the substratum kept increasing." "The increasing pressure (toward the earth's center) could not fail to develop high viscosity."

The author gives no reference, no evidence and no measurements, nor are there given in any of the above references a suggestion even of a ratio, to say nothing of the actual rates and values. Some of these items may be subjected to experimentation. Rate of flow of different lavas could be measured, at determined temperatures, and with calculated cross-sections and gradients; then the values could easily be checked against the calculated or measured rate of water flow under similar conditions. The viscosity of water is known. Then we should have some specific ratios, with actual values in them.

Possibly we could study the viscosity of lavas made up with specific compositions and heated to desired temperatures. The following statement is taken from a well-known text-book. "Where the saturation (of the water) is but little above that of average ocean, animal life becomes scarcer and Molluscs make thicker and rougher shells." It seems simple to test the matter out and see how much the saltiness has to be increased to make certain appreciable reductions in the numbers of animals, and to ascertain how much thicker and rougher molluscan shells become with definite increases in the saltiness of the water.

We have a considerable number of meandering streams of various sizes and on various grades with measurable velocities, loads and volumes. It would be instructive to make quantitative studies of say twenty or thirty streams over a period of years to learn the rate of growth of meanders under the several conditions, the rate of migration of meanders, the size attained by meanders in the different streams before they cut themselves off, the frequency of meanders and other items on the behavior of meanders. We shall never be able to meet successfully the problem of Mississippi River control until some one knows quantitatively much more of the physiography of the river.

The mechanics of faults have been worked out pretty largely on an empirical basis. Let us now measure the rate of movement, direction of movement, frequency and continuity of movement along several active faults and thus add somewhat to our knowledge of faults and possibly as much to our ability to interpret them.

Rocks weather at vastly different rates. What is the rate of solution of limestone? Of dolomite? What is the rate of weathering of feldspars? Of olivine, hornblende or enstatite? By controlling conditions and making regular accurate measurements we can add to our knowledge of weathering.

We have often made estimates of the rate of sedimentation of sands for sandstones, clays for shales, and calcium carbonate for limestones. It is now possible to make accurate measurements of the depth of water in lakes, marshes and shallow seas, then resurvey after a period of years, and thereby to discover the actual rate of sedimentation under these conditions. One such study would be valuable, but a score of them would remove some of our guesswork and crude estimates. Some of the Finger Lakes in New York State, western Lake Erie or Ontario in parts might be thus studied. Lakes in Europe would also be admirable for such studies. Chesapeake Bay would seem to be a promising sea to investigate. Lake Pontchartrain has an interesting story to tell. Reefs off the coasts of Florida, Cuba, Yucatan and the Bahamas might give good results on the rate of reef building.

Shore-lines can be carefully surveyed or measured both where cliffs occur and where beaches are building, as well as along delta fronts. New surveys fifty or one hundred years after would give worth while checks on rates of progress. Most of our knowledge of such matters is couched in such terms as "rapid erosion," "slow building forward" and "intermittent cutting and filling along the beach."

Of course no one man could complete any of these studies, but institutions could initiate them and carry them on. Individuals could make first surveys and will their programs and data to some institution able to take the work over. Then some other man could later continue the work. Some of these problems should be carried forward at intervals for hundreds of years. It is a long look ahead, but geology, which gazes far back, should also have courage to plan far ahead.

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GENTLE SOUTHWEST WINDS

THERE had been a chill easterly breeze all day, which to a person perhaps abnormally apprehensive had been a source of fear that his plans for to-morrow's outing might be upset, but the evening paper carried the reassuring forecast: "Fair and warmer, gentle southwest winds." In the morning waking up by the light rather than by the clock one finds it already late with a driving rain against his windows and a northeaster blowing forty to fifty miles an hour in the stead of that five- to eight-mile soothing southwest zephyr. True, the mathematician will tell us that -50 is much less than $+8$, but is it gentler? The plans are indeed upset with only a cynic's humor to relieve the disappointment as he reads in the breakfast newspaper the forecast for to-day: "Fair and warmer, gentle southwest winds."

There are two things to observe about this forecast. First, however it may have been from the viewpoint of the meteorologist who made it, from the viewpoint of the reader it is not a forecast but a plain contradiction of the actual condition. Does not the forecaster unnecessarily expose his reputation by permitting this observation to be made? Should he not take a leaf from the notebook of the long-range weather expert and talk only of the future, that which ordinarily will be future to the reader of the forecast, so that his errors will not strike so many so obviously? And, second, is the forecast properly to be called erroneous; may it not be that, superposed upon the general flux of meteorologic conditions, which is subject to such law as may let forecasting aspire to be scientific, there is an essentially hazardous element of more or less local instability which will forever in part cheat the aspiration of its fulfilment?

Those who watch the typical local thundershowers of the summer play hide and seek with the sun know that the precipitation is brief in time and very spotty in place. Many do not so well realize that even in a general wide-spread rainstorm the precipitation comes often in spurts which last but a short time and are therefore to be presumed to be distinctly local. This phenomenon shows itself on the records even of the annual rainfall.[1] Consider the table of six stations in the city of Providence,[2] R. I., for the years 1921–1925.

The first five rows in the table give the records under municipal auspices at the Hope, Fruit Hill and Sockanosset reservoirs, the Pettaconsett pumping station and the sewage precipitation works. That record which in each year is highest or lowest is marked H or L; that year which at each station is highest or lowest is marked h or l. The yearly means for the five stations and the difference between high and low are next given. This difference varies from 2.7 to 9.4 inches, with a mean of 5.0. Yet if the records for the five years be averaged the difference H-L is only 1.9. The range of the yearly means is 10.5 inches. Furthermore, the highs and lows (H and L) distribute themselves haphazardly among the stations. These facts show that, so far as this brief record of these five stations goes, we may well consider that the variations between the stations are fortuitous each year, that as precipitations are measured to .01 inch the variations between the stations must be regarded as real and of about one half the extent of the variations from year to year, that the driest year was 1924, according to Hope and Fruit Hill, but 1925

[1] See the discussion by A. McAdie, "Dry and Wet Seasons," Blue Hill Annual for 1923.

[2] The figures for precipitation are given in the exhaustive tables by X. H. Goodnough, "Rainfall in New England," J. N. E. Water Works Assoc., 29, 239–432, 1915; 35, 228–293, 1921; 40, 178–247, 1926.

PRECIPITATION IN PROVIDENCE, R. I.

Station	Elevation	1921	1922	1923	1924	1925	Avg.	h–l
Hope Reservoir	162 Ft.	46.5H	49.0h	27.7	37.8Ll	39.0	44.0	11.2
Fruit Hill	275	44.2	53.7Hh	44.7	38.4l	40.0	44.2	15.3
Sockanosset	182	44.6	48.8L	49.3h	42.9H	41.5Hl	45.2H	7.8
Pettaconsett	25	45.1	49.1h	40.4L	42.5	39.2l	43.3L	9.9
Sewage Works	25	43.6L	50.2h	49.8H	42.1	38.8Ll	44.9	11.4
Mean		44.8	50.2	46.4	40.7	39.7	44.3	
High–Low		2.9	4.9	9.4	5.1	2.7	5.0 or 1.9	
U. S. Weather Bur.	182	36.8	44.9	40.8	33.5	33.6	37.9	
Mean—U. S. W. B.		8.0	5.4	5.6	7.2	6.1	6.4	