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NOTICE TO AUTHORS

Manuscripts are to be submitted to associate editors of the appropriate section and *may* be submitted directly to the editor in chief. Clear manuscript, preferably typed, with wide margin to the left, with double spacing between lines, is desirable. Please try to follow the typographical style of the most recent issue of the BULLETIN. Line drawings should be submitted on Bristol board, or similar material, and done in India ink. Figure number should be written on this in pencil. Titles for drawings, with figure numbers, should be typed on a separate sheet. Please try to minimize footnotes. Appended references and bibliographies, clearly so marked, should be done in the style of the A.A.A.S. publication, *Science*.

Astronomy

FATHER SECCHI AND SOLAR RESEARCH

VICENTE MARASIGAN, S. J.

Fr. Secchi's extensive work on solar research is recorded in detail in his two volumes of *Le Soleil*. Many modern English textbooks on the sun, for example, Menzel's, Abbot's and Mitchell's, have more references to the written works of Lockyer and Young as their source of material. But Fr. Secchi's work is the earliest major work on the sun listed in Abetti's textbook. The second edition of *Le Soleil* appeared in 1877 while those of Lockyer and Young appeared in 1887 and 1895 respectively. One can see, just by comparing dates and tables of contents, that Fr. Secchi's work may well have been the standard model textbook. If so, one may be justified in saying that Fr. Secchi has, in effect, laid out the general outlines of modern solar research.

Not that he was the originator. Periodical literature, even before Fr. Secchi's time, was noticeably growing in abundance on the subject of solar physics, particularly with the contributions of Janssen, Lockyer, Herschel, Zollner, Huggins, Respighi, Tachini, Lorenzani, Rayet, Young, and many others. But it remains true that *Le Soleil* represents the first systematic ordering of all solar science that man possessed at that particular point of history, Fr. Secchi's lifetime. In other words, Fr. Secchi was a representative cross-section in the progress of astrophysics.

One of the earlier studies of Fr. Secchi was the problem of sunspots. His methods and findings are now merely of historical interest, because we know a great deal more now about sunspots than did Fr. Secchi and his contemporaries. His apparatus for spot observations was as follows: he attached to a small portable telescope a white screen, whose normal lay along the optical axis; the image of the sun was projected on this screen. With this simple arrangement, he observed the presence and number of sunspots, their shapes, their motions and their general behaviour. In *Le Soleil* he devotes about a hundred and fifty pages to a detailed description of his spot observations, which included the movement of spots across the face of the sun, their proper motions, their changing shapes, the annual and secular variation of their movements, their cavity structure, their periodicity and its possible causes. To give an example of the details with which he minutely specifies his observations, we quote from *Le Soleil,* Vol. I, p. 75, which we translate freely:

"As soon as the spot of July 30 . . . had arrived near the limb, I examined it; but the first part disappeared at about five in the afternoon, and the air was so turbulent that I found it impossible to observe it. The next day, the air was calm and the sky perfectly clear; the spot was recognized simultaneously by three observers. M. Tachini of Palermo was staying at our observatory at the time, and his is the design which we reproduce . . . It was nine o'clock; one of the craters was near the limb, and clearly seen was the contour formed by a prominence on the solar disc, curving out from one side to another. Beside it was a huge facula. This part of the contour formed a depression below the general surface of the sun, at a region where, after a short time, a large penumbra became discernible. At 10:20, the crater had receded considerably, and presented more points that were extremely sharp. One such point was noticed by all observers because of its size and hook formation. At 10:32, the interior of the crater showed nothing but a very narrow dark line which disappeared at 11:00, the contour of the sun still preserving its indented formation . . ." And again: "On July 9, 1874, there was a spot traversed by a magnificent bridge; on the 10th, the spot had approached the sun's limb, the bridge appeared off-center with respect to the nucleus; it was projected on the side of the spot away from the center of the solar disc. On the 11th, its eccentricity was more pronounced; it was projected on the penumbra. Undoubtedly, these changes are real, but since there was otherwise a great constancy in the shape of this group, I am greatly inclined to believe that these are effects of perspective; this bridge is a luminous arc at a higher elevation than its vicinity, and is seen projected on different parts of the cavity below it, depending upon the different angles at which it is observed . . ." And so on.

One can readily see from this one excerpt that Fr. Secchi possessed a great devotion to detail, a virtue that is very important in gathering empirical data for teamwork research. He observed many details, understood some of them, but not others; but whether he understood them or not, he faithfully recorded them, with the hope that some day, some other investigator would succeed in piecing them together and work out a correct theory to explain them.

Among the more important details he recorded from his observations of sunspots, are the following: 1) the filament structure of the penumbra, 2) converging currents in the penumbra, 3) the vortex motion of the spot interior, 4) the presence of a *veli rosati* inside the nucleus, 5) some precise measurements of spot depths, 6) an apparent relationship of sunspots with eruptions and faculae, 7) the presence of metallic eruptions and absorbing sodium vapor in sunspots.

He carried on this program of spot observations for seven years, using both the small portable telescope and the large refractor with an attached projection screen. He did this on practically every clear day of these seven years. His main conclusions were: 1) spots are formed as a result of huge eruptions of metallic vapors from the solar interior, 2) most spots are cavities on the solar surface, full of absorbing matter, 3) eruptions are frequent and large during spot maximum, few and weak during spot minimum, 4) the successive relative positions of eruptions and spots indicate a mass movement of solar matter in a vortex motion, carrying everything slowly from equator to poles, a fact which necessitates internal circulation; this also explains the variations of solar diameter (in a law now generally known as the *Secchi-Rosa Law*), 5) this mass movement explains the higher velocity of the equatorial zone in comparison with the polar regions, 6) eighteen years of spot record confirm the theory of Lamont and Sabine that terrestrial magnetism is influenced by sunspots.

The study of sunspots was not the only, nor even the most important field of Fr. Secchi's solar research. Earlier even than this was his study of intensities of solar radiation. He set up a simple attachment to his equatorial telescope, whereby he could get a projected image of the sun one meter in diameter on a black screen. On this screen were two small openings about 20 centimeters apart, allowing two beams to pass through on to a white screen not far behind the black screen. The black screen was movable. In this way, any two points on the solar disc of different radial distances could be compared for relative luminous intensities. This was one method. Another method he employed was by means of a *pila termelettrica* which measured thermal intensities with deviations of a sensitive galvanometer. These deviations determined quantitatively the decrease of radiation intensities with distances from disc center—or *limb darkening*. Moreover, Fr. Secchi observed that the solar equator had a higher temperature than the poles, and that the northern hemisphere had a higher temperature than the southern.

On the subject of solar prominences, Fr. Secchi was at his best. His descriptions of the phenomena that he saw indicate a passionate devotion to detail and a warm admiration of their beauty. Over 220 pages, including about 50 colored drawings and diagrams in Volume II of *Le Soleil* speak eloquently of his enthusiasm over solar prominences.

His method of observation was the usual one of placing a broad slit tangentially to the solar disc a few seconds of arc away, and viewing the monochromatic H-alpha image visually. The dispersing element was a system of three prisms in series. With this arrangement, he observed that the edge of the chromosphere was jagged and in continual agitation, and he called it *prateria infocata* or *prairie in flames*. These flames were directed in the general direction of the poles during periods of maximum activity, and during minimum, they were irregular, brighter and more elongated. The extremities of the prominence flames appeared driven by horizontal currents, suggesting circulation of the solar atmosphere. The prominences were scattered all over the solar surface from pole to pole. Fr. Secchi made regular observations of their number and formations. In 1871, he counted 2767 prominences, and described their various forms as resembling clouds, plumes of smoke, trees, feathers, fountains, jets, spikes, spouts. With his habitual attitude of orderliness, he set down a preliminary basis of classification of prominences into his *quiescent* and *eruptive* types. He noticed that eruptive prominences were more closely associated with sunspots. His analysis disclosed their chemical constitution: iron, titanium, sodium, barium, magnesium. They formed and dissolved very rapidly. It was this fact of the short duration of many interesting solar phenomena that eventually led to the developing of a modern technique called *spectroheliokinematography* or color movies of the sun.

Fr. Secchi's records of his observation of prominences included not merely descriptions and drawings, but also attempts at gathering quantitative data. But the mathematical process involved, though simple enough, is too tedious for this present paper.

Eclipse expedition. For men engaged in solar research, if one has not had at least one eclipse experience, he has not lived. Fr. Secchi had one in 1860. By that time, he had already attracted the attention of the scientific world, and his authority on solar studies was widely respected. His Holiness Pope Pius IX was very much interested in his achievements, and willingly financed his eclipse expedition to Desierto de las Palmas, in Spain. Fr. Secchi undertook it with the specific plan of photographing his favourite specialty, the prominences, then comparing them with those to be taken simultaneously by another eclipse team stationed at Rue a Rivabellosa, and thus to settle once and for all a raging controversy on: 1) whether the prominences were real or illusory, 2) whether they belonged to the sun, 3) whether the corona really existed or not, and how large it was. I translate Fr. Secchi's own description (*Le Soleil*, Vol. I, p. 377):

"Two expeditions were organized to photograph the eclipse . . . M. de la Rue chose Rivabellosa near the Atlantic coast, and my position was at Desierto de las Palmas, near the Mediterranean coast. We had two difficulties to solve; we knew absolutely nothing of the photogenic power of light during an eclipse; we did not know whether it was possible to obtain results working with the speed demanded by the circumstances. De la Rue was using a heliograph from Kew, and since the images formed directly at the aperture of the objective were very small, he preferred to enlarge them with a lens. We, on the other hand, preferred to take the direct image produced by our Caushoix objective. This image 25 mm. in diameter, gave perfect visibility, and anyway, there would always be some means available of enlarging this by any of the usual procedures. We adopted this method for two reasons: 1) the feeble intensity of the light, which, on the assumption that it is equal to that of a full moon, would seem to demand an exposure of one minute if the image is to be enlarged; we were more sure of a direct image; 2) this method allowed a larger number of exposures within a given interval, and consequently a determination of a larger number of phases. . . . Our first plate was given an exposure of six seconds, and during this moment, besides the prominences, there was a perfectly visible trace of the corona. The second plate was exposed for about twenty seconds; but three jolts on the equatorial during the exposure produced three distinct and separate images of the prominences; evidently, with an objective of six inches like ours, a very short time interval suffices for reproducing these appendages. . . . (The figure) represents the first exposure taken just before the start of totality. It contains seven main prominences: 1) A prominence having two summits close to each other and somewhat elevated. In the photograph of de la Rue, this is barely visible; one could just about see the two summits; this as we have said before has a parallactic effect. 2) A large prominence in the shape of a cloud inclined 45°, round at the base, pointed at the summit, with a helicoidal structure, as is shown in the enlarged photograph of de la Rue. 3) Tiny wisps of clouds in a group shaped like a curved horn at an altitude of 2′ 40″ . . ."

And so on goes Fr. Secchi, describing in minute detail all the phenomena he observed: a cross-shaped group of clouds, a huge mass of blinding effulgence, a chain of flaming mountains, gigantic flames of all colors—completely accompanied with quantitative data, the exact time of each occurrence, the exposure time of each plate, the sizes and shapes of objects seen, down to the last minute and second of arc. After presenting all this abundant accumulation of data, he states the following conclusions: "1) Prominences are not mere optical illusions; they are real solar phenomena. Our observations were made from two points very far apart, and it is impossible to suppose that these photographs, so sharp and so identical, could have been produced by a mirage or something like that. 2) Prominences are masses of luminous matter in considerable agitation, with wonderful photogenic activity . . . many of them emitting chemical radiations . . . 3) There are masses of prominence matter suspended and isolated like clouds in the atmosphere . . . 4) Besides the prominences, there is a layer of the same material, completely enveloping the sun. Prominences originate from this layer; it consists of masses which move about throughout the surface but remain detached from it, sometimes resembling smoke ejected from chimneys, at other times resembling volcanic craters . . . 5) There is an incalculable number of prominences . . . 6) Their height is considerable, ranging from ten times the earth's diameter to one minute of arc . . ."

All these findings of Fr. Secchi on the subject of prominences are now standard articles in modern textbooks.

On the subject of solar spectroscopy, Fr. Secchi's studies were more of an exploratory character. Spectrum analysis was then only in its infancy after its first application by Bunsen in 1859. At that time, the dispersing element was usually a prism or a series of prisms, and Fr. Secchi was limited to this method of spectroscopic work. It was not until 1875, or three years before his death, that he got hold of a diffraction grating. It was presented to him personally by Professor L. M. Rutherfurd, the American spectroscopist, as a token of personal esteem. It was speculum metal, ruled finely with 4000 lines to the inch. But it was rather late. Fr. Secchi's eyesight was beginning to fail him. His last recorded writing, his *Memoirs* of 1877, one year before his death, states that he was working on methods of overcoming the difficulty of overlapping orders in the grating spectrum. One method he tried was the use of filters, which is common procedure today. Another method which he found successful was one which, he said, "was different from that of Young". What that second method was, I could not find out. Perhaps no one will ever find out. A few months after writing this, Fr. Secchi died.

He was then sixty years old. Of these, he had spent more than thirty years in scientific research. It was the year 1878.

This period during which Fr. Secchi lived was marked by a widespread intoxication over scientific progress. Scientific discoveries were startling the world. The Industrial Revolution was effecting a shift of values towards materialism. In the biological sciences particularly, as a result of the Theory of Evolution as interpreted by Thomas Huxley, Haeckel, Spencer and others, there was a wide-scale movement towards atheism. In a manner of speaking, this was due to faulty statistics: with insufficient empirical data, over-eager minds made arbitrary interpolations, and the resulting curve went askew.

Fr. Secchi's mind worked differently. He concentrated on empirical data almost exclusively. He seemed to avoid any theorizing. He knew and highly appreciated the value of theory. He realized that much could be accomplished in the progress of knowledge if man continually kept throwing out tentative hypotheses to bridge the gap between the mind and physical reality. But for lasting value, that bridge between mind and reality must stand solidly on stone piers of facts, and not hang precariously from fancy curves and statistical festoons. Fr. Secchi represents a school of thought, alongside of Kirchoff, Janssen, Lockyer, Huggins, Young, and many others, that a sober appraisal of observed facts is necessary to maintain the equilibrium of scientific knowledge. In this sense, he was a symbol of moderate empiricism.

Fr. Secchi was a symbol in a second way, and this more profound. The materialism of his age had so paralyzed and confused the spirit of man, that as far as religion was concerned, man's attitude was either supercillious or superstitious. The attitude had grown so much that man, in his psychotic confusion, decided to build a "wall of separation" between science and religion, and thus avoid interior conflict. Fr. Secchi's solution went deeper. With a strength of mind born of a firm faith that truth is one and indivisible, he succeeded in integrating his life of prayer with his scientific research, because as a matter of fact, mysterious as this may seem, the two are one.

Geology

THE GEOLOGY OF NEW ENGLAND

James W. Skehan, S. J.

In our travels from house to house within the New England Province, each one of us has noticed that this part of the country is a land of great topographic variety. The Taconic Mountains occupy eastern New York, western Vermont and Massachusetts. The Green Mountains of Vermont, the Berkshire Hills of Massachusetts and the highlands of western Connecticut are separated from the Taconics by the steep walls of the Rutland and Berkshire Valleys. The lowlands of the Connecticut Valley are separated from those of Rhode Island, eastern Massachusetts and Maine by the mountains of western Maine, the White Mountains of New Hampshire and the Hills of central Massachusetts and eastern Connecticut.

We are all familiar with the geology of New England to the extent that we refer to New Hampshire as the "granite state" and to Vermont as the "marble state". Most of us realize that granite is quarried also in many places in central and southeastern New England, as for example in Quincy. In addition many of us have noted the red rocks of the Connecticut Valley which stand in sharp contrast to most of the drab colored rocks elsewhere in New England. Perhaps we have even heard that dinosaurs once lived in the Valley. Everyone, moreover, knows that the gravel and boulder blanket now covering all of New England is a souvenir of the last glacier.

Perhaps we have wondered whether the mountains of New England ever rose to the spectacular heights of the Rockies of western United States, or of the Alps and Himalayas of Eurasia of today. Did Mount Greylock, for example, ever rise to 16,000 feet as does Mont Blanc today? Did Mount Monadnock, a natural rock pyramid on the lowlands of southern New Hampshire, ever rise to the elevation of the present-day "fiend of the Alps", the Matterhorn?

The answer to these questions is that 200 million years ago New England undoubtedly displayed mountains as spectacular as those of the present-day Alps. At that time the Appalachians, of which New England is a very important and interesting part, had just risen out of the ocean in which they were formed.

How were these New England Alps formed? The process by which the Appalachians of New England probably were formed consists of four major steps. This hypothesis, developed by the writer, is based in part on geo-physical data from various parts of the world, and in part on personal field studies and a comprehensive survey of the geologic literature of New England. The scope of this discussion

GEOLOGICAL TIME SCALE

Millions Of Years B. C.	Era	Epoch	Events	
		Recent		
1	Cenozoic			
		Pleistocene	4 Glacial Stages	
		Pleiocene		
		Miocene		
		Oligocene	Alps Formed	
		Eocene		
		Paleocene	Rocky Mts. Formed	
70				
		Cretaceous		
		Jurassic		
	Mesozoic		Conn. Valley Formed	
		Triassic	Stage 4	
195				
		Permian		in the
		Pennsylvanian		formation
		Mississippian	Stage 3	of the
	Paleozoic	Devonian		Appalachians
		Silurian	Stage 2	(according
		Ordovician		to
		Cambrian	Stage 1	the Skehan
500				Hypothesis
				1952)
		Proterozoic		
	Precambrian			
		Archeozoic		
3000-5000				

is such that it is necessarily a vastly oversimplified picture of the process as it occurred. The writer proposes, however, to give a unified picture of the major events in the geological history of New England in such a manner that the most significant geophysical and geological data are satisfactorily explained without at the same time entering into the details of the process.

The first step in the formation of the Appalachians was inaugurated in early Cambrian time, about 500 million years ago. (Cf. geological time scale; also Skehan, 1951) At this time a long trough-like depression was formed by the downbuckling of the crust of the Earth. This trough formed a narrow arm of the sea and was located along the general line of the present Appalachian Mountains. It extended from Newfoundland and Nova Scotia southwestward to Alabama.

Not only was a seaway trough formed but a corresponding upwarp of the crust developed on the Atlantic Ocean side of the trough (Fig.1). This upwarp is known to geologists as Appalachia. Figure 1 is a vertical section through New England as it looked at that time and shows the downbuckled crust resting on the basaltic substratum. Appalachia on the east with its volcanoes is the source area from which the gravels, sands and shales were carried by rivers and deposited in the seaway trough. During the whole of the 300 million years from the beginning of this process to the final episode in the formation of the Appalachians, millions of tons of rock fragments were washed into the seaway and piled up to a thickness of 50,000 feet. Throughout the whole process most of the strata were laid down just below sea level. Therefore it is inferred that the trough buckled downward slowly but steadily. At this time also lava-flows poured out intermittently on the seaway floor and volcanic ash filtered down through the ocean waters and were deposited together with the gravels, sands and clays contributed by the rivers of Appalachia.

Are there any mountains of the future which today are in the fundamental stage of formation shown in Figure 1? It seems that the Gulf of Mexico as it is today is a seaway in which strata have accumulated to great thicknesses. These will undoubtedly be folded some day and will be lifted up to form a new generation of American Alps.

The second stage in the process of the formation of the Appalachians (Fig. 2) was completed when the granite crust was compressed to such an extent that the part below the seaway was depressed deep into the basaltic substratum and the strata of the seaway were gradually crumpled and folded. Temperatures in this part of the basaltic substratum were greater than the melting temperature of the granite crust. Once depressed into the zone where the critical temperature of granite was exceeded, the rocks of the trough began to melt.

Gravity data in the East and West Indies (Vening Meinesz, 1940; Vening Meinesz et al., 1934; Hess, 1938) indicates that the granite crust is there depressed to a depth of about 37 miles. Thus it would seem that at the present time the seaway trough between the Asiatic continent and the island arcs of the western and southwestern Pacific area are in this second stage of mountain formation. Thus Japan would seem to correspond to Appalachia; the Asiatic mainland, to the Adirondacks; and the sea of Japan to the Appalachian seaway.

Stage 3 (Fig. 3) in this process of the formation of the Appalachians was reached after the central part of the trough had been

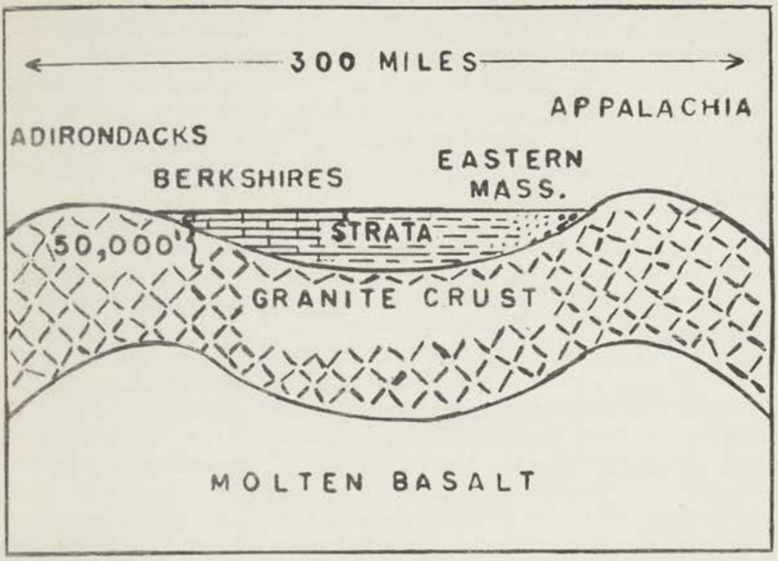

Fig. 1. The initial stage in the formation of the Appalachian Mountains of New England.

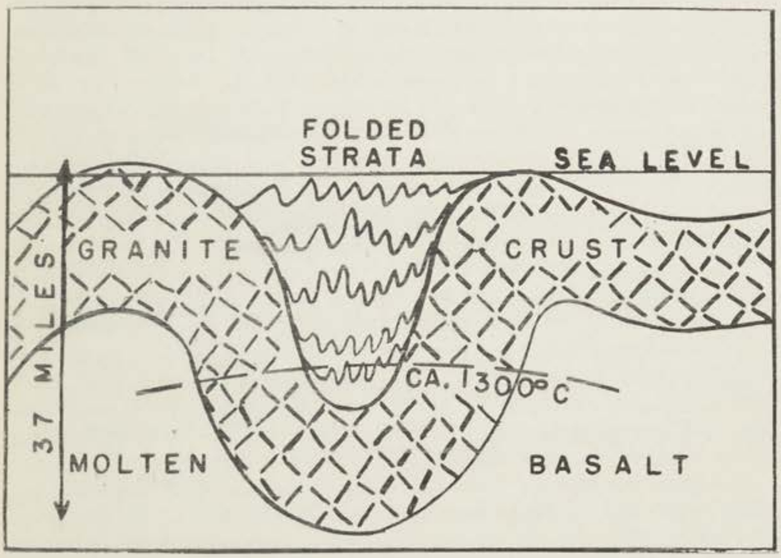

Fig. 2. Second stage. The granite crust was downbuckled into the basaltic substratum.

depressed into the basaltic substratum and after the crust and lowermost gravels and sand deposits had melted. These molten granite bodies rose toward the surface of the Earth and in doing so effectively buoyed up the whole mass of the adjacent unmelted crust and strata. The average specific gravity of the basaltic substratum is about 3.0, whereas that of the granite is about 2.7. Thus the granite tends to rise toward the surface of the Earth much as a basketball when submerged in water rises to the surface when released.

Once the downbuckled granite root had melted and begun to rise, the compressive forces that had depressed this mass into the substratum began to relax. With this relaxation and the concomitant rise of the granite, the whole mass of strata and crust were lifted high above sea level. Thus 200 million years ago the formation of the New England Alps was completed. Once these mountains began to rise above sea level, rivers and streams began their relentless task of carrying them piecemeal back to the nearest ocean basin.

As the New England Alps were whittled down by rivers, they came to look first like the modern European Alps, then like the Rocky Mountains of western North America. About 30 million years after the Appalachians were completed, the Connecticut Valley region was subjected to great earthquake activity, probably as a result of the extreme relaxation of the former compressive forces that had folded the Appalachians. Thus the eastern side of the Valley was cut by a fault zone (Skehan, 1948, p. 94, Fig. 5). At this time the central part of the Valley began to sink along this fault zone relative to the rocks to the east. As it sank, rivers and streams from the highlands to the east and west poured gravels and sands into the lowlands. From time to time lava flows spread out over the Valley floor. One such lava bed is now represented by Mount Tom and the ridges to its north and south. These rocks accumulated to thicknesses of several miles. Throughout all this time dinosaurs and other Mesozoic vertebrates, flora and freshwater invertebrates flourished in the tropical, humid climate of the Valley. The fossils and rock types are the clues that tell us the nature of the climate at that time.

In Jurassic time, about 150 million years ago, numerous earthquake zones in addition to the eastern border fault were produced (Skehan, 1948, p. 94, Fig. 6). Volcanoes in the adjacent mountains cast out volcanic ash over parts of the lowlands, as for example at Northampton, Massachusetts. By the movements of these fault blocks, a highly mountainous topography was regenerated. Since that time this second generation of Appalachian Alps has been worn down to the relatively smooth surface of present-day New England.

About one million years ago the climate of North America became colder and ice began to pile up on Labrador and in the area west of Hudson Bay. As the ice accumulated it began to spread out until at last it covered the northern half of North America and all of New England. Four times the glacier advanced southward and four times the advent of milder climates melted it back toward its place

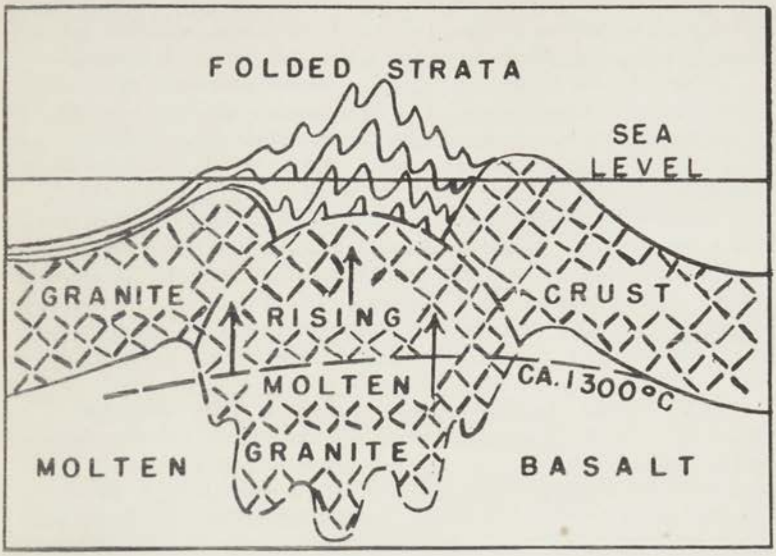

Fig. 3. Third stage. Downbuckled crust and lowermost strata melted and began to rise.

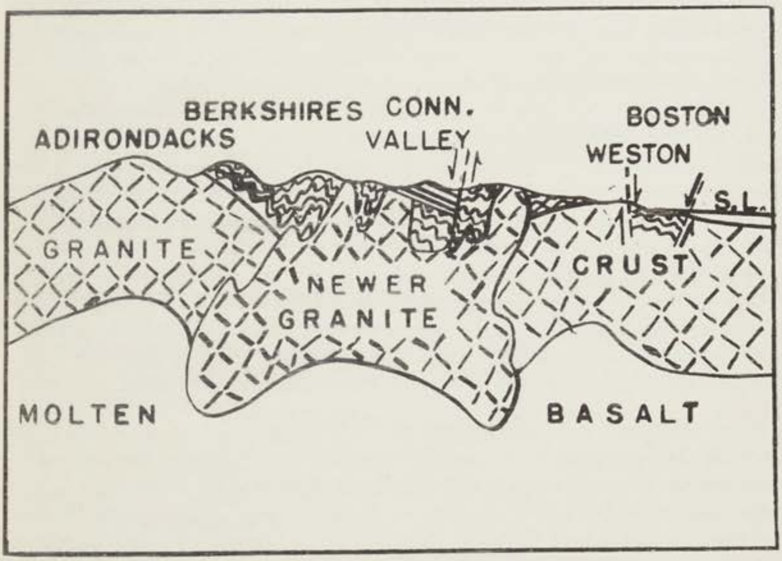

Fig. 4. Diagrammatic cross-section of the present day Appalachians of New England.

of origin. As the ice pushed its way southward over New England like a giant bulldozer, it carried all the loose gravel and sand it found or could grind and pluck from the rocks over which it moved.

When the ice melted during the warmer periods it dropped the rocks that it had picked up. The finer sands and clays were carried by the glacial rivers and were deposited in glacial deltas, riverbeds and lakes. These deposits are represented today by the boulder-strewn lands, gravel, sand and clay banks so abundant in New England.

Figure 4 is a cross section of New England today. Many of the strata deposited and folded in preceding geological ages have by now been either wholly or partially carried away by rivers, as for example, by the main artery of New England, the Connecticut River. The glacier has changed the surface appearance of New England very little. So relatively thin are the glacial deposits that they cannot be shown on the scale of this cross section.

What is the probable geologic future of New England? The rivers of New England will go on cutting down the mountainous uplands until the whole area is much like that part of Canada north-west of New England. The distant future will probably be one of relative geologic peace and quiet. New England will probably never again have much to fear from severe earthquakes in spite of recent talk in certain quarters to the contrary. In the relatively near future it is quite possible that a glacier may again cover New England. If the glacial cycle of the past million years repeats itself, we may expect the fifth of the glaciers to appear on the northern horizon in about 240,000 years. Glaciers do not form suddenly and once formed move only a few feet per year. Therefore we have the consolation of knowing that we will not have to live in igloos in New England during our lifetime as a result of a sudden glacial period.

Lest from the hypothesis presented here one should infer that the process of mountain formation is well known, the writer hastens to add that this is far from the truth. Actually this branch of structural geology is still in its infancy. Great hope, however, for obtaining more detailed information on the processes of mountain building has sprung up principally from the many advances in recent years in the application of various geophysical sciences to geological problems. More specifically, detailed geologic mapping complemented by data from earthquake seismology, reflection type seismic surveys, gravity and magnetic investigations will provide much needed data for controlled speculation on the processes of mountain formation.

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Chemistry

THE HISTORY OF CHEMICAL EQUILIBRIUM

BERNARD A. FIEKERS, S.J.

This is an attempt to derive the equilibrium constant historically and give the teachers, who have to derive it mathematically year after year for their classes, some respite from the gruelling routine. For it might be interesting to see how the concept was developed through the primitive chemical affinity stages into the reversible reaction stage, refined by dynamic considerations and finally brought into its present general form.

Most of us are familiar, I am sure, with the interest in atomism shown by the Greek and Roman thinkers. For we have often heard the names of Leucippus, Democritus and Lucretius as the earliest speculators on the structure of matter. A second speculation which intrigued the Greek philosophers, and none the less important, dealt with the question why does one substance change into another; why do chemical changes occur. Hippocratus of Coz (ca. 460 B.C.) stated that substances which are akin or similar to each other have mutual affinity, and accordingly react with each other.

This concept of affinity seems to have survived even to the time of St. Albertus Magnus (1193-1282) and beyond. On account of their pre-occupation with the transmutation of baser metals into gold, many alchemists were perfectly familiar with the so-called affinity between mercury and gold. Indeed this idea persists today as a thumb rule for physical solubility: similia similibus solvuntur. But chemical affinity as we know it was still to be developed.

In 1661 Robert Boyle on the other hand was probably the first to point out an elective character in affinity. He observed that particles tend to unite at times with those of other denominations more closely than among themselves. One such chemical reaction, that was clear cut and actually driven to completion, was discovered by John Mayow in 1674. It is the action of sulfuric acid on potassium nitrate to yield nitric acid: in Mayow's terminology, vitriol on nitre to produce spirits of nitre. Complete reaction was possible because nitric acid could be separated by distillation. He gave numerous other examples of this kind and paved the way for Glauber's Salt.[1]

About the year 1700, Isaac Newton emphasized that such affinity might be electrical in character. He studied the precipitation of metals out of solution by means of baser metals. Affinity would not only be elective in character; priorities could be assigned and tables of affinity would be constructed. Such a tabulated series was probably first proposed by Etienne Geoffroy in 1718. At any rate George Ernst Stahl, the Father of Phlogiston, proposed his displacement series in 1720.

By 1760 Antoine Beaume reversed Mayow's reaction, already cited, by producing nitre and vitriolic acid from spirit of nitre and vitriolated tartar (K_2SO_4). We could almost write the modern equation:

$$K_2SO_4 + 2\ HNO_3 = 2\ KNO_3 + H_2SO_4 \qquad (1)$$

but for the fact that different conditions attended the opposing reactions. This led to tables of affinities under dry and under wet (solution) conditions. In the hands of Bergman (1775), if some exaggeration be allowed, as many tables of affinity could be constructed as there were conditions to prevail. But Bergman did recognize that in a given reaction, an excess of one of the reagents would enhance the completion of the reaction.

About this time (1774) Priestley decomposed mercuric oxide into its elements and his discovery was communicated to Lavoisier. Lavoisier made the oxide from its elements and then decomposed it, not reversibly of course, because the conditions for the opposing reactions differed. Out of this development of chemical affinity and the possibility of reversing reactions under different experimental conditions, was to arise a more distinct idea of the part played by the mass of the reagent in determining the course of a given reaction.

Claude Louis Berthollet, out on an expedition with Napoleon in 1799, observed large quantities of soda ash formed by the Dead Sea. Ordinarily, it would precipitate calcium, according to the modern equation:

$$CaCl_2 + Na_2CO_3 = CaCO_3 + 2\ NaCl \qquad (2)$$

Berthollet ascribed the anomaly to the large salt concentration; called for the reversal of equation (2); took exception to Bergman in postulating the absolute completion of reactions because of the quantities of reagents; and showed that the division between reagents and products depends in part upon the affinities and in part upon the masses present.[2] This is really the law of mass action in which the dynamic aspects are still somewhat obscure.

At this point (1803), Berthollet ran afoul of Proust in a prolonged controversy over the apparent incompatibility of his position, with the law of constant proportions. Gay Lussac supported Berthollet. So did Heinrich Rose, later in 1842 and also many of the early texts. It was Berzelius who finally reconciled the law of constant proportions with the law of mass action.

There was still a great amount of work to be done in bringing the law into its final general form. In 1850 Wilhelmy was probably the first to stress reaction rates, by his study of the inversion of sucrose. This naturally emphasized the quantitative aspects of the amount of sugar present. Then Williamson[3] discussed the dynamic nature of equilibrium reactions, showing that the state of equilibrium results from two opposing reactions proceeding at the same rate. In 1855 Gladstone confirmed these ideas through his study of the ferric thiocyanate reaction. In 1862-3, Berthelot and St. Gilles studied esterification reactions from both directions; showed that the same final concentrations could be realized; and formulated mathematical rate expressions in concentration terms; but left the combining of the rate expressions of the forward and the reverse reactions for Guldberg and Waage to accomplish.

Finally in 1862-3, Guldberg and Waage gave us the final generalized rate expression of the equilibrium constant substantially as we use it today. They proposed the postulate of "active masses," which we still have to come to grips with today.

All the while physical dynamical equilibrium had undergone parallel development, and probably lent giant support to the progress of chemical equilibrium. As we know, J. Willard Gibbs developed his phase rule, shortly thereafter. Equilibrium is its central theme. His system of thermodynamics also is a precursor of our modern concept of the relation between free energy and equilibrium. This relation is due to van't Hoff. Free energy in turn is our quantitative estimate of chemical affinity.[4]

But the physical aspects of equilibrium provide matter for a second paper, which should close the circle of affinity, ancient to modern, through the equilibrium concept.

The author is deeply indebted to Prof. James J. Tansey of this Department for most of the historical data offered.

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MULTIPLE EQUILIBRIUM IN CHEMISTRY

BERNARD A. FIEKERS, S. J.

The numerical values of equilibrium constants for many ionic reactions are not to be found in the chemical literature; some can be calculated, however, from more fundamental and available constants. A familiar instance is the hydrolysis constant, K_h, for the reaction:

$$Ac^- + HOH = HAc + OH^- \quad (1)$$

which is written:

$$K_h = \frac{(HAc)(OH^-)}{(HOH)(Ac^-)} \quad (2)$$

From one point of view of this reaction, the two Brönsted bases, OH^- and Ac^-, compete for available proton. If then this hydrogen ion concentration is put into both the numerator and the denominator,

$$K_h = \frac{(HAc)(H^+)(OH^-)}{(H^+)(Ac^-)(HOH)} = \frac{K_w}{K_a} \quad (3)$$

the hydrolysis constant can be evaluated by equation (3) in terms of other well known constants: the water constant, K_w, and the ionization constant for acetic acid, K_a. The values of these are generally available and the great utility of such derived constants is at once apparent to the instructor.

R. N. Boyd,[1] in an article appearing recently in the *Journal of Chemical Education,* generalizes this technique beyond the familiar evaluation of hydrolysis constants. Some of his problems and illustrations depend on the following equations and their corresponding equilibrium constants.

$$AgCl + 2\,NH_3 = Ag(NH_3)_2^+ + Cl^- \quad (4)$$
$$Mg^{++} + 2\,NH_4OH = Mg(OH)_2 + 2\,NH_4^+ \quad (5)$$
$$Br^- + AgCl = AgBr + Cl^- \quad (6)$$
$$Cd^{++} + H_2S = CdS + 2\,H^+ \quad (7)$$
$$Zn^{++} + H_2S = ZnS + 2\,H^+ \quad (8)$$
$$Zn(OH)_2 + 2\,OH^- = ZnO_2^- + 2\,HOH \quad (9)$$

A close inspection of all of these equations shows two significant facts:

1) in each of these equations, there are at least two species that yield ions only sparingly in solution. These are either substances like water, hydrogen sulfide and ammonium hydroxide or they are complex ions or precipitates; 2) in each of the equations there are always at least two species that compete for a third species, and that third species does not appear explicitly in any of the equations. In equation (4), ammonia and chloride ion compete for the silver ion; in equation (5) ammonia and the

magnesium ion compete for the hydroxyl ion; in equation (6) bromide and chloride ions compete for the silver ion; in equation (7) hydrogen and cadmium ions compete for the sulfide ion; in equation (8) hydrogen and zinc ions compete for the sulfide ion; and in equation (9) zincate and hydroxyl ions compete for a proton. These observations do not appear in Boyd's article.

The method proposed by Boyd is not applicable to all problems on equilibrium, but only to those in which there occur at least two species which yield ions in solution to only a slight extent and in which corresponding competition occurs. These observations have then diagnostic value in this respect.

Further, when one attempts to apply the method it is often extremely difficult to choose the proper factor which is to appear in both the numerator and denominator. Here the second of the observations made above is of further diagnostic value. One chooses that species (or those species in more complicated cases) for which two or more species in the equation compete, even though it may not appear explicitly in the equation. These species have been listed for each equation under observation no. 2 above.

[1]Boyd, R. N., A Procedure for Solving Equilibrium Problems, J. Chem. Educ., **29**, 198 (1952).

GRAHAM'S LAW OF GASEOUS DIFFUSION IN THE CHEMISTRY LABORATORY

Bernard A. Fiekers, S.J.

There seems to be a genuine difficulty in supplementing lecture work in gas kinetics with suitable experimentation in the chemistry laboratory. Many manuals have suitable procedures for the law of Charles; one is often deterred from experimenting with Boyle's law in large classes on account of the bother and expense of the mercury involved and in the hope that the subject will be treated in physics laboratory anyway; one might be discouraged from presenting Graham's law of gaseous diffusion, when he finds that so many of the laboratory manuals require the use of clocks or stopwatches for determining these rates.

But the use of timing devices is utterly unnecessary, as the following experiment will show. In principle it simply involves the fundamental dimensions of mass and length; the third one, time, might be added, except that it seems to complicate this simplest and academically most satisfying of the traditional experiments in gas kinetics. On the debit side, this experiment does not yield the most accurate results; nor is it long enough to take up a whole laboratory period; the period has to be filled out with some other experiment. The directions which the author uses follow.

MOLECULAR WEIGHT OF A GAS. RELATIVE METHOD. If hydrogen chloride (HCl) and ammonia (NH_3) are suitably introduced at the extremities of a length of glass tubing, each diffuses through the tube at its own rate, and on meeting both react to form ammonium chloride (NH_4Cl). This appears as a cloud within the tube or as a sharply defined deposit on its walls, thus serving as an *indicator* for the distance traversed by each of the gases.

This phenomenon is an application of Graham's Law for the Diffusion of Gases. The law states that the rates (u) of diffusion of gases are inversely proportional to the square roots of the densities (d) of the gases at a given temperature; or, since these densities are directly proportional to the molecular weights (M, m) of the gases, then the rates of diffusion are inversely proportional to the square roots of the molecular weights of the gases:

$$U M^{1/2} = K \quad (1)$$

and $$u_1 m_1^{1/2} = u_2 m_2^{1/2} = u_3 m_3^{1/2} = \text{etc.} \quad (2)$$

or $$u_1^2 m_1 = u_2^2 m_2 = u_3^2 m_3 = \text{etc.} \quad (3)$$

Since, however, the distance traversed by the gas (L), divided by the time (t) constitutes the rate of diffusion

$$U = L/t \quad (4)$$

on substituting these values into equation (3), the time values cancel, since the time is the same for both gases and, under these experimental conditions

$$l_1^2 m_1 = l_2^2 m_2 \quad (5)$$

Or knowing therefore the molecular weight of one of the gases, and measuring the distances traversed by both, one can then calculate the molecular weight of the second gas by this relative method.

EXPERIMENTAL PROCEDURE. Select clean glass tubing about 2 ft. long by 8 or 9 mm. internal diameter. Clamp it in a horizontal position to a ring stand over the desk. Then use two cotton tipped medical applicators, dipping one of them into freshly opened concentrated ammonia (NH_3) and the other one similarly into concentrated hydrochloric acid (HCl). At exactly the same time insert the applicators into opposite ends of the tube to a depth of a few centimeters each. Observe the tube carefully for some minutes for the formation of the indicator cloud. Mark its first appearance with ink or crayon and later check it by the proximity of the deposit which will form. With a meter stick measure to the nearest millimeter the distance traversed by each of the gases from their origins (the tips of the cotton) to this mark. Record also the approximate time required for the diffusion.

Wash out the tube with water. Then rinse it once or twice with a few ml. of acetone in order to remove the water. Gently pass a flame over its length until the tube is visibly dry, and allow it to cool to room temperature again. It is essential to have the apparatus dry.

Repeat the experiment using new applicators each time. If time allows, repeat washing, drying and diffusion for a third determination. Calculate each determination separately; using m value 17 for ammonia, find m for HCl. Unless data are to be rejected (see instructor) report the mean for the three determinations, the deviation of each from the mean and the average of these deviations. Compare your results with the known molecular weight of hydrogen chloride.

It was found that this procedure could be used with the pairs: ammonia/hydrogen chloride, methyl amine/hydrogen chloride and dimethyl amine/hydrogen chloride. In the case of the two amines mentioned, and in the case of using "stale" ammonia, a few drops of each of these compounds had previously to be mixed with concentrated NaOH (12 M) in order to release the gases and get reasonable results. Otherwise the vapor pressure of the gas out of its solution would have been the rate controlling step, being slower than the kinetic diffusion. Eastman Kodak, no. 527, 25% solution of methyl amine in water and no. 601, 25% solution of dimethyl amine in water were used for the supplementary amine experiments. Any number of other pairs come to mind, which can be released by some such chemical device and should be suitable to form an indicator zone. The experiment can well be kept in mind for organic chemistry, qualitative analysis, both organic and inorganic, not to mention physical chemistry. For, it takes but ten or fifteen minutes to run it. At times it can give very striking information.

REMOVING FLUORINE ETCHING FROM GLASS

Bernard A. Fiekers, S.J.

During some research on silicon tetrafluoride, the glass windows of our hood in the research laboratory at Holy Cross became etched and lost their transparency. A number of expedients were attempted to remove this etching and most of them failed. Finally, after experimentation on some broken pieces which were removed and replaced, the following solution was found.

Pumice powder was mixed with Bon Ami powder in the dry, in a ratio of three or four pumice to one of the Bon Ami. This was applied in the standard way for cleaning windows with Bon Ami. Some extra "elbow grease" had to be applied both in the first (wet) application of the mixture, and in the final cleaning of the dry mixture from the panes. It is surprising what little added effort was needed over and above the usual window cleaning technique. Of the ten windows thus damaged, very few needed a second treatment. This note was suggested by the efforts of Mr. John L. Reardon of this department.

Varia

UNDERGRADUATE RESEARCH

JOSEPH F. MULLIGAN, S.J.

A recent article in *Scientific American* points to the importance of undergraduate research in turning students towards careers in science and stimulating them to continue their scientific studies after graduation (A. Roe, "A Psychologist Examines 64 Eminent Scientists," *Scientific American*, 187, no 5, p. 21 [Nov., 1952]). Of the 20 biologists, 22 physical scientists and 22 social scientists (psychologists and anthropologists) studied, most did not decide on their careers till their junior or senior year in college. Then what decided them was a college project in which they had occasion to do some independent research. Once they discovered their liking for this type of work, they never turned back.

This fact is of considerable importance for our Jesuit schools. A number of recent studies indicate that Catholic colleges in general, and our Jesuit colleges in particular, have not turned out a proportionate number of graduates who later went on to the Ph.D. level in science. (See, for example, *Baccalaureate Origin of the Science Doctorates Awarded in the United States 1936-1945* [Washington, D.C.: National Research Council, 1948]; Knapp and Goodrich, "Origins of American Scientists," *Science*, 113, 543 [1951]; J. N. Cooper, "The Undergraduate Origins of American Physicists," *American Journal of Physics*, 20, 200 [1952].) As a result the Catholics among the eminent scientists of the country are regrettably few.

For this reason it is imperative that students in our colleges with a bent for scientific research find this out before it is too late. As Professor Karl F. Herzfeld wrote in 1939: "The specialized mind that will be interested and will excel in research is so rare that every deflection of such a one is a grave loss to the cause of Catholic higher education. Unfortunately it happens sometimes that students go into engineering and then discover that they really wanted to do research in physics, go into medicine and discover that they really wanted to do research in biology. Too late they discover that they have chosen the wrong track because nobody told them that pure research is possible." (K. F. Herzfeld, "Catholic Education and Science," in *Vital Problems of Catholic Education in the United States*, edited by Roy J. Defarrari [Washington, D. C.: The Catholic University of America Press, 1939], p. 180.)

These facts should serve as a stimulus to programs of undergraduate research along the lines of those reported in the chemistry section at the 1952 meeting of the American Association of Jesuit Scientists at Fordham. It is to be hoped that such programs of undergraduate research will encourage some of our students to go on to become the eminent Catholic scientists that the Church needs so badly today.

Westinghouse Fellowships for Teachers of Science at M.I.T.

Bernard Scully, S. J.

In the summer of 1948 the Westinghouse Electric Company initiated a generous plan of 50 fellowships for high school science teachers at the Summer Session of Massachusetts Institute of Technology. During the summer of 1952 I was awarded one of these fellowships and I derived so much benefit from the course that I would like to tell other Jesuits about them.

The Westinghouse Fellowships are open to any high school science teacher in the country. Information and application blanks may be obtained from Professor Francis W. Sears, Summer Program for Science Teachers, Room 4-356, Massachusetts Institute of Technology, Cambridge 39, Massachusetts. There is no entrance examination given. The application blank contains spaces for education and experience. A letter of recommendation from the high school principal is required.

In the group of fifty last summer there were included four religious. There were two Jesuit scholastics, a Benedictine priest, and a Xavierian brother.

In general, the aim of this course is to acquaint teachers with two things. The latest advances in chemistry, physics, biology and engineering are presented by experts of the Institute's staff to bring the teacher up to date in his field. (The extent that these may be taught to high school students and the most feasible methods are largely developed by after-class discussions among the particular teachers interested in each field.) Great emphasis is placed on the type of science discipline that the Institute finds to have been most helpful for high school students. Demonstrations and lectures similar to those given in some of the M.I.T. freshman and sophomore classes are given as examples of the way that the Institute teachers educe the concepts that have already been given in a rudimentary way in the student's high school class. Here a very important notice is made of the great necessity for the high school teacher to give clear, solid, fundamental ideas of the basic *principles* of chemistry and physics. For example, several high school teachers who had taught their pupils a rather pragmatic and somewhat non-realistic approach to mechanics (the D'Alembert system) came to see the great advantage of a simple yet thorough disciplining in the simple Newtonian Laws of Mechanics. Through experience, the Institute has found that, although D'Alembert's methods may be used to solve some simpler problems, the Newtonian concepts are much more intelligently understood and used because they are based on a more realistic observation.

Another example to illustrate the high school preparation that the Institute has found to be the most solid for incoming students is found in the use of the so-called M K S system of units in physics. Teachers at the Institute have found that this is the practical and intelligent system to use in their freshman classes. They have found that freshmen who have been instructed in the fundamentals of this system are not prey to the inevitable confusion that has arisen in the mind of the student who has had to rack his brain over the difference between a pound of force and a pound of mass. After a very telling lecture-demonstration on this point one of my fellow students told me "Now I can see why my students found units so hard to use. This year I'm going to teach that Meter-Kilogram-Second system if I have to write my own text-book."

In chemistry, too, the need and benefit of stressing fundamentals was insisted upon. Professor Arthur Davis showed himself to be not only a master teacher but also a penetrating diagnostician of the cerebral aches of first year chemistry students. From his own inimitable way of teaching equilibrium concepts the Westinghouse Fellowship students grew to appreciate the feasibility of his recommendation to insist on teaching the fundamentals of equilibrium in high school chemistry. It was very apparent to all of us that our high school students can receive a better *chemical* way of looking at reactions if we can teach them that most reactions are really reversible reactions.

These examples have been given merely to illustrate some of the ways that Professor Francis W. Sears, a veteran freshman physics teacher at the Institute, and Professor Arthur Davis, freshman chemistry teacher, impress on the teacher-students the type of science education that the Institute has found to best prepare students for their course.

In the six weeks of instruction there were many interesting lectures and demonstrations that stand out in memory. Mention has already been made of the basic physics lectures by Professor Sears. In the first three weeks there was one daily lecture and demonstration by Professor Sears in some of the important branches of Physics. Clear demonstrations showed that color is really made up of different wave lengths. An interesting experiment to determine wave length by diffraction was performed before the class. The theory came to life when the class was taken through the huge underground spectroscopy laboratory. Professor Sears taught the fundamentals of wave theory as a background for interesting trips to the Radar and Acoustics laboratories. Other lectures on electronics increased our interest and understanding of the apparatus and demonstrations in the Van der Graff laboratory and at the Cyclotron and the Synchrotron. There were other fields of physics covered and sixteen laboratory tours concretized the concepts of the lectures.

In the last three weeks there was one daily lecture and demonstration in chemistry by Professor Arthur Davis. In addition to the explanations of equilibrium *per se*, Professor Davis illustrated how many types of chemical reactions may be explained by equilibrium ideas. Hydrolysis, Neutralization, Use of Indicators, Electrolytic reactions were explained and illustrated by equilibrium concepts. Professor Davis' innumerable asides and *obiter dicta* enlivened his classes for everyone. His ready mind seemed to have a card index of the mistakes that freshmen will be expected to make at any demonstration. Great was his delight when he could trap the teachers of our group into the same pitfall as I for one can blushingly recall. Laboratory tours showed exemplifications of chemical theory in industries such as mining and rubber.

These are only a few of the very interesting highlights that memory recalls of the actual classes. Outside of class there were many discussions of individual teaching problems, syllabi, text-books, aims, standards, techniques, and salaries. Despite the fact that members of this class numbered fifty-one teachers from almost every state in the Union, there was a very close degree of understanding and friendship among all. It was an education in itself to learn first-hand the varying standards and attitudes represented by teachers from all over the country. The very obvious interest of the Technology staff in the members of the group brought about a most friendly and co-operative atmosphere. The kindness and respect shown by the Institute staff and the science teachers to the four religious in the group was heartwarming. From the very beginning the other religious and I knew that we were among friends who wanted to help us and to receive whatever we could contribute to the class. We were given a typewritten list of the names and addresses of the group and I am sure that many of the teachers will strengthen the friendships that they made in this most helpful summer course.

Graduate credit of four semester hours may be obtained either in chemistry or physics. To receive this the student may choose a subject in his field on which he writes an original paper. For example, I wrote on "The Chemistry of the Carbon Cycle in Nature". The professor in charge hands out a set of problems in chemistry or physics which are to be turned in by the end of the program. The process of solving these problems, in chemistry I know, and in physics, I was told, is a real help to understanding the matter of the lectures and demonstrations.

The amount of the grant from Westinghouse is two hundred and fifty dollars of which fifty dollars is given for the cost of tuition. Students may live in the Institute's dormitories for seventy-two dollars for the six weeks or they may live in any other place of their own choosing. No text-books are required for the classes since the professor's notes are specially given only to members of this Westinghouse group. It is a great advantage to have the use of the copious Institute libraries. Science teachers will find the members of the library staff most courteous and helpful.

The keynote of courtesy and interest in the members of the group was given to us on the very first night at a pleasant banquet in the Tech faculty club where we were addressed by President James R. Killian and by Mr. Louis M. Stark, Director of the Westinghouse Educational Foundation.

A great debt of gratitude is owed to Westinghouse Corporation and to Massachusetts Institute of Technology for their generous spirit of civic interest and their sincere desire to help in the education of America's science teachers and future scientists.

I most heartily recommend this Westinghouse Summer Fellowship to any Jesuit High School Science Teacher. He will find that he can gain benefits from it to help him in his work of teaching the children whom God has entrusted to him.

THE ATOM BOMB ON HIROSHIMA

REV. JOHN B. SIEMES, S. J.

For a long time the inhabitants of Hiroshima had been wondering why they alone were not undergoing heavy bombing by the Americans. Observers were flying over the town daily and were off again. Occasionally bombs would fall. There was little damage. Certainly it could not compare with the damage that other Japanese cities had to suffer. Wild and fantastic rumors started to circulate that America had something special for us, but no one could have dreamt that it would turn out like this.

The sixth of August, 1945, turned out to be a clear bright day. Air raid warnings at seven o'clock! Some planes appeared over the city, but nobody bothered any more than usual. About eight o'clock, special air raid warning! I was sitting in my room in the Jesuit Novitiate of Nagatsuka, which was about two and a half miles from the centre of Hiroshima. The Novitiate is built half way up a mountain. It has a beautiful view of the wide valley that stretches to the sea. It was at about 8.14 that the whole valley was suddenly

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*NOTE. This translation is offered at this time because of the active part played by many of "ours" in civilian defense. Those who have seen the U. S. Army film, "The Tale of Two Cities", will recognize the author, Father Siemes, who appears in this film to give substantially the same testimony which appears here. This paper appeared originally in German in the "Mitteilungen aus den deutschen Ordensprovinzen," S.J., no 110, pp. 32-35, 1946. This translation was made by the Rev. Bernard A. Fiekers, S.J., Chairman of the Chemistry Department at Holy Cross College. Other versions have already been publicized in the American Press. Permission to translate and publish "this first detailed account of an eye witness" has been graciously granted by Superiors of the Province of Lower Germany. *Ed.*

filled with a glaring light as if a giant photoflash had gone off. At about the same moment I felt a heat wave, but I could then only see a yellow flashing sort of light. As I rushed to the door—it was probably only ten seconds after the first flash—I heard a moderately loud explosion which likely had its source above our house. With it all windows in the house were wrecked. I was covered with splinters of glass. My hands and my head were cut and bleeding. Around me all was confusion: windows smashed, doors broken through and book shelves askew. My Jesuit confreres were wounded from the flying glass; some bled; but no one was seriously wounded.

At a distance of about a half mile down the valley, some of the farm houses started burning. Smoke clouds started to rise over the city and I heard some rather indistinct explosions. A long line of confused mankind started to stream up the valley from the city about a half hour later. Some came to our house, heavily dragging their steps, with blackened faces, all either bleeding or burned, many with horrible wounds on their limbs or backs. We brought them into our chapel, laid them on the straw carpets and gave them all possible help. But our small supply of fat was soon used up. Father Arrupe, our Rector, who fortunately had studied medicine before entering the Society, took care of the wounded everywhere as long as medicaments and bandages lasted. In the long run we had to be satisfied merely to clean the wounds because of the large number wounded who came streaming in to us.

Towards noon the large chapel and the library were filled. The procession of refugees from the city did not halt. Father Kopp came with it and he was wounded in the neck and head and had a burn on the inside of his left hand. He had been standing in front of the Convent of the Helper of Poor Souls, intending to walk home, when of a sudden he noticed the flash of light, felt the heat wave, and felt a big blister developing on the inner part of his hand. He believed that a bomb had fallen in his immediate vicinity. Immediately things started to burn all around him, so that he only had time to save a few things for the convent before the whole district was consumed in flames. He and the sisters had to fight their way along the river's edge and through the burning street to get to our house.

Soon the report came that the whole town was in flames and ruin. Outside the town, the streets were jammed with burned, bleeding and bewildered people. There were, however, among them many who were unharmed. Completely dazed by the catastrophe, they took off with little thought of organizing help for the others. Later on we realized to the full that the Japanese were able to develop little preparedness, organization or effectiveness in the face of this catastrophe. Where by cooperative effort something could be saved, they despaired of the rescue work and in their fatalism allowed the catastrophe free rein. If we put pressure on them to join in the rescue, they would indeed do all as directed, but on their own initiative they did but little.

We realized that our Mission Superior, Father LaSalle and three of our fathers were down in the middle of the city. At about four o'clock we heard that our church, the rectory and the neighboring houses had been burned to the ground; that Father LaSalle and Father Schiffer had been severely wounded and could not get away under their own power. The nearer we came to town, the greater the damage and the harder it was to make any progress. Indeed we had to go down to the river twice in order to get around the flames.

A mass of humanity had taken refuge in the park since all streets and bridges were blocked by fallen trees. Wells of fires still flared up in the distance and spread an eery light. Finally at the outer end of the park at the shore of the river we found Father Schiffer lying deathly white on the ground. He had a deep wound behind his ear and had lost so much blood that we feared for his life. Father Superior suffered a deep wound on his leg. Fathers Cieslik and Kleinsorge had two minor wounds. They were completely exhausted.

Gradually they narrated their experiences. At about 8.15 they had seen the glaring light and immediately heard the windows, walls and furniture literally flying apart. They were showered with glass splinters and rubble. Father Schiffer was buried when a wall collapsed. It was here he got his head wound. Father Superior was wounded by numerous fragments in the back and legs thus losing a great deal of blood. At this part of the city too, the impression was strong that the bomb had burst in the immediate vicinity. All surrounding buildings had collapsed immediately. Horrible cries for help arose everywhere from the surrounding ruins. Fathers LaSalle and Schiffer helped despite their wounds as much as they could and lost much blood in the effort. As the fire crept up on them, they had to leave in order to save their own lives.

Mr. Fukai, the secretary of the Mission, practically lost his mind. He could not be moved from the spot until Father Kleinsorge carried him out of the house on his back and took him away by force. Beneath the ruins of the houses upstreet and downstreet many bewildered people lay and cried for help. But there could be little hope. The flames would reach them before the wreckage could be cleared away. Mr. Fukai refused to go further and nothing was ever heard of him again.

Luckily a saving angel came to our rescue in the form of a Protestant Japanese minister who was passing us on the stream in a boat. He insisted that he would bring our wounded upstream to safety. Father Schiffer, our heaviest casualty was the first to be taken. Many children, whom we gathered from the shore along the way, died quickly. They had bad burns. Father Cieslik agreed to walk home in order to give his place on the boat to others.

At midnight we were still at work, taking care of the wounded and attempting to get our own people back to Nagatsuka. Fallen wire, beams, ruins and rubble blocked every street and passage. The night was pitch black. We were always stumbling to the ground and our human burden fell with us. Father Schiffer lost consciousness. At every fall Father LaSalle had a humorous remark to make, painful ordeal for him though it was, when we consider all the glass that was still stuck in his back. The enterprise had taken twelve hours. In the early morning I got two hours of sleep. Then I celebrated a Mass of Thanksgiving, for it was then the seventh of August, the Feast of the Restoration of the Society of Jesus.

The following day we tried to rescue victims in the streets. Rescue teams had made no appearance in the city. The people whom we had brought to safety on the previous day, sat and lay in the exact positions where we had laid them. More than thirty hours passed before the regular rescue squads came in.

It was dark by the time we returned to Nagatsuka the second night. We had fifty refugees to care for; most of them wounded; many with dangerous burns; all of them, even those with only slight burns, were weak and helpless. In the eyes of the people the help that we gave them meant much more for the cause of Christianity than all of the work we had done in the previous long years. A few of those whom we cared for did not survive. At the official rescue stations a good third or probably half of those who were brought in failed to survive. They merely lay there, practically without care. Supplies, surgeons, assistants, bandages and drugs, all were lacking.

The magnitude of the catastrophe that fell on Hiroshima on August 6th, 1945, I can only all too slowly comprehend. Now that I can review the whole picture more easily I would like to sum it up in the following way: the explosion of the bomb at 8.15 o'clock ruined the whole city in a single blow. Only small outer districts to the south and east of the city escaped the total ruin. The bomb exploded over the middle of the city. The little Japanese houses, which comprise 99% of all buildings in the city were at once collapsed or blown away by the air gust. The occupants were buried in the ruins. Whoever was outside suffered burns either from a substance or from rays that emanated from the bomb. Fire broke out wherever this substance alighted in sufficient amount. It spread rapidly. The heat that arose from the ground was intense enough to cause eddies that drove the fire the length and breadth of the city. Those caught in the ruins could not be freed quickly enough to escape. Houses were damaged up to three miles away from the centre. Many collapsed and were consumed in flame. Windows were broken even at a distance of seven miles.

How many people were victims of this one bomb? Hiroshima had a population of 400,000. Official reports give the following figures up to the first of September: 70,000 dead, 130,000 wounded, of which 43,500 were seriously wounded and many thousands are missing.

Thousands of the wounded who later died could have been saved if they had received the proper treatment. But there could be no help in proportion to the size of this calamity. Many of the wounded died of weakness due to undernourishment. Those who had average strength and good care recovered slowly from their atom burns. But there were cases where the wounded temporarily recovered and then succumbed suddenly. Some who had only slight external burns died within a week after symptons of inflammation in the throat and mouth. I know personally many cases where people died without any external burns. Fathers Kleinsorge and Cieslik, who were near the explosion, suffered ordinary wounds due to cutting, but had no burns. Two weeks after the explosion the simple wounds were normally healed, but the others became worse and were only imperfectly healed in October. Without doubt, the rays or whatever it was, have some effect on the blood. I am still of the opinion that the obvious general undernourishment and weakened condition of so many people is responsible for the many cases of death. The rumor went out that the ruined city would still emit death rays for a while. I doubt it. For I myself, and many others who worked hours on end after the explosion in the ruined area, have not suffered even the slightest detriment to our health.

It was an undescribable catastrophe. But the most remarkable thing of all is this, that the Japanese people have not been embittered against America. Much good can still arise out of this tragedy. Of all nations in the world today, America is the first to be in the position to help us to bring this people to the knowledge, the love and the service of the one true God.

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