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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*.

THE REVEREND MICHAEL J. AHERN, S.J.

(1877-1951)

R.I.P.

The first president of the American Association of Jesuit Scientists, and a charter member; through whose initiative and enthusiasm was due, in large measure, the foundation of this association. To his sacred and abiding memory we most respectfully dedicate this issue.

THE REVEREND MICHAEL J. AHERN, S.J.

An Appreciation

In the truest sense of the term, Fr. Michael J. Ahern, S.J., was one of the Founding Fathers of the American Association of Jesuit Scientists, (Eastern States Division). It may come somewhat as a surprise to realize that he was the first President of the Association. In late years, his contact with us has not been close; many public activities have had claim on his time. However, it should not be forgotten that our Jesuit Science Association owes its foundation, in large measure, to the inspiration and scientific enthusiasm of Fr. Michael J. Ahern, S.J. With much justice did the first Editors of the Jesuit Science Bulletin write: "Whatever success the future may bring to the Association, too much credit cannot be given to Fr. Ahern who initiated the project." In his death we have lost a Founder, our First President and a Charter Member.

He was a scientist whose interest in matters scientific never waned. His main field of study was Chemistry and Geology, but he sought knowledge in all the branches. Anthropology also was one of his favorites. He seemed to lean more toward the factual side of science than to the theoretical. His excellent memory stored up enumerable facts which were invaluable to him in his public-lecture work, especially when treating of the "Church and Science." This topic was to his mind of major importance. For, in his earlier days as a Priest, he realized that many bitter attacks were launched against the Church for her attitude toward the natural sciences. This supposed hostility of the Church was usually illustrated by the Galileo case which was considered proof of the reactionary position of the Catholic Church. On numerous occasions did Fr. Ahern lecture to disprove these false accusations.

Today, that extreme antagonism has been moderated and men of science are in many instances coming to see the truth of the question. Perhaps it would not be an exaggeration to say that it was the presence of a Priest-scientist like Fr. Ahern, in scientific circles and lecturing on scientific topics on public platforms or perhaps his membership in and active cooperation with men of science, which have contributed much to the changed outlook of scientists toward the Church, and her approval of real science and scientific investigation. Certainly, the high respect with which he was regarded and the official positions which he held in science associations are an open tribute to his scientific talents and ability.

Fr. Ahern's scientific concern was not merely a personal one. He had an inclusive love of the Society of Jesus and labored much to promote her reputation in science. He was eager to assist his fellow Jesuits, especially the younger men, in their science studies. To cite one instance: as Rector of Canisius College, Buffalo, N. Y., his encouraging attitude and generosity were a notable factor in building up the various departments of science and elevating the academic standards in these subjects. He gave the college a prominence and position which has never been forgotten. In 1940 Canisius College awarded Fr. Ahern the degree of Doctor of Laws, honoris causa, in recognition of his talents and his success in promoting the progress of that institution.

Public speaking and lecturing have always been an outstanding feature of his priestly life. Many of us will remember his famous illustrated lecture: "Thank God for a Garden", with the rich colorful slides of flowers, trees and shrubs, many of them taken from Weston College gardens. These slides were made by himself, for he was an excellent photographer. Again, his lectures on his pilgrimage to Fatima and his moving pictures delighted many audiences. I suppose that he was best known for his radio talks on the Catholic Radio Question Box, over station WNAC in Boston. Under the patronage of his late Eminence, Cardinal O'Connell, Fr. Ahern was the founder of this Catholic Truth Radio Hour and carried it on for more than twenty years. It is not too much to say that through this radio work, he was the most widely known Jesuit in New England. "Everybody knew Fr. Ahern." Of the immense good which he did through his radio lectures, both to the Church in general and to individuals, we cannot speak. That is known to God alone. A brilliant jewel in his crown of glory will be his reward for all eternity.

Another salient feature of his public life was his participation in the Interfaith Movement. He strove well to try to reconcile dissident elements of this group. For this, he traveled far and wide and delivered uncounted speeches. He was much sought after for this work. His strong voice, his clear presentation of the doctrines of the Catholic Church, his kind and charitable interpretation of the actions and words of others, made him a favorite speaker among these sects. His friends among them were legion and he would often boast of how easily friendships would spring up as the result of these talks with others outside the faith. For this pulic activity, Fr. Ahern had this rare gift: he was able to take an abstruse point or doctrine and popularize it, so as to fit it to the mentality of an ordinary audience. It was invaluable to him and he used it to the best of his ability. This is one of the reasons why he was in demand for radio programs and public addresses. He was extremely popular among the Rotarians of Buffalo for his dinner-talks.

Fr. Ahern was a very kindly person. He had a hearty manner and easily made friends in every walk of life. He had a host of them. He was generous with his time and talents. Charity was a precious virtue to him; he cultivated it diligently in his speech and his dealings with others. He related to me some instances where a charitable word or gesture avoided unpleasant incidents in public halls. Sometimes a belligerent inquirer might stir up trouble, in a question period after a lecture. But Fr. Ahern's kindly manner or his habit of giving another the benefit of the doubt would preserve peace. His famous lecture on "Tolerance," which he delivered so frequently, was to him an interpretation of the virtue of charity towards people he met in civil life.

In 1946, Fr. Ahern celebrated his golden jubilee as a Jesuit. For the community at Weston College, where the jubilee was held, and for his many friends among the Jesuits and elsewhere, it was a very joyous occasion. He was extremely happy about and much gratified at the heartfelt congratulations and good wishes received that day. Not long after this his health began to fail. While giving a triduum at Shadowbrook last year he experienced a fall which caused internal injuries. This, combined with a diabetic condition of long standing, brought with it other complications which ultimately resulted in his death at St. Elizabeth's Hospital, Boston, Mass., on June 5, 1951.

Others who knew Fr. Ahern more intimately might eulogize him in more glowing phrases and at greater length than these few lines. They might tell of notable events and achievements in his long life as a Jesuit. They could speak of many more virtues than are here mentioned. Yet, to the sacred memory of an outstanding scientist, a worthy Priest and an esteemed member of the Society of Jesus which he loved so well, we are pleased to offer this small tribute. May he rest in peace. *Rev. Joseph P. Kelly, S.J.*

THE REVEREND MICHAEL J. AHERN, S.J.[1]

A Biography

Michael Joseph Ahern was born in New York City on May 25, 1877. In 1896 he received the A.B. degree from St. Francis Xavier College in New York. On September 7th of that year he entered the Society of Jesus at Frederick, Maryland. After a few years of the novitiate he went to the College of the Society at Woodstock, Md., and obtained his A.M. degree there in 1902.

The years 1902-1906 saw Mr. Ahern as Instructor of Chemistry and of Geology at Boston College. During part of this time, from 1904-1906, as a Graduate Student of Geology at the Massachusetts Institute of Technology, through his studies and acquaintances, he seems practically to have laid the broader foundations for his future work in the New England area.

He then returned to Woodstock College for the seminary work of his priestly vocation, where he incidentally instructed in geology, but he remained there for only one year, 1906-1907. It may have been his geological avocation, on the other hand, that prompted a change of seminary to the University of Innsbruck in the Austrian Alps. At any rate Ahern the geologist must have made many a "field day" out of his holidays from seminary life in this earth-science paradise of the Alps. Ahern the scientist achieved here his mastery of the German

[1]From an article to be published in the *Hormone,* Holy Cross College.

language, so necessary for the scientist of his day. Ahern the priest, or man of God, living abroad, must here have acquired his sympathetic tolerance for men of other creeds and nations; and here deepened the channels of his manly religious devotion which clearly marked the course of his life to come. At Innsbruck he was ordained to the Holy Priesthood in 1910, and in 1911 he received the degree of S.T.D. from the University.

His first appointment on return to this country was to Professor- and Directorship of the Department of Science of Canisius College in Buffalo: 1911-1915. From 1915-1919 he was Professor of Chemistry and of Geology at Boston College and Head of the Department of Chemistry. From 1919-1923, Father Ahern served as President of Canisius College and Director of the Canisius Laboratories. The College underwent remarkable growth during his presidency, both in its divisions and departments as well as in its enrolment.

It was in 1923 that Father Ahern was called to the College of the Holy Cross. He was to replace the Reverend George L. Coyle, S.J., who was leaving Holy Cross to found the Chemo-Medical Institute of Georgetown University. Father Coyle had served here since 1906. The College needed his capable successor to tide the Department of Chemistry over to the time of the late Reverend George F. Strohaver, S.J., who in turn succeeded Father Ahern in 1925. It was incidentally Father Ahern, who penned the obituary of his friend, Father Coyle, in 1932 (1).

The year 1925-1926 saw Father Ahern again on pioneer service at St. Joseph's College in Philadelphia where he contributed much of his unflagging energy to building up the laboratories there. Finally he returned to New England in 1926 as Professor of Chemistry and Geology at Weston College. Until his retirement from this task in 1939, due to the pressure of other duties he had acquired, he inspired many of the younger Jesuits in their scientific pursuits. He was to labor at Weston, however, for the last quarter century of his life. Here he seems to have made his greatest contribution.

For forty-seven years, 1904-1951, Father Ahern was a member of the American Chemical Society. In 1937-1938 he was Chairman of its Northeastern Section and for any number of years held otherwise high office in the councils of the Section. His vast experience in public speaking, especially his Catholic Truth Broadcasts which he had started in Boston in 1929, lent tremendous impetus to the chemical broadcasts of the Northeastern Section (2), entitled Chemistry of Today. He was the first chairman of its radio program committee, and in fact delivered the first broadcast for the section on February 19, 1931. He contributed in an original way to the popular broadcasting of technical subjects; he was ever alert to the recruitment and training of speakers; and was ever ready to keep the program on the air, as often as he had to, whenever the vast unseen audience provided too much of a psychological barrier for some tyro speaker.

Some of these broadcasts featured "The Chemical Question Box" in which answers from the experts were provided for any chemical questions sent in by the listeners. Over one hundred weekly broadcasts were devoted to chemistry. But Father Ahern stayed with the Catholic Truth Hour until his retirement in 1950.

Throughout his active life Father Ahern was very popular as a special lecturer before many organizations and clubs, and at Communion Breakfasts. Always a superb photographer, he used the medium of lantern slides whenever they would be of help in expounding his themes. He was one of the earliest devotees of color film (Höchet's Pinatype, 1906. Exhibit no. 39, Holy Cross Chemistry Department). He used photographic and projection equipment all his life and kept abreast of their latest improvements. For years he was chaplain of the motion picture operator's local union in Boston. His popular lectures on geology in connection with the Teapot Dome Investigations in 1923; his lectures on religious and racial tolerance; and his lectures with colored slides: "Thank God for a Garden"; all give some idea of the versatility of the man. Similarly in his science lectures, he was always interested in the acquisition of good equipment, and he became master in the art of lecture demonstration.

Not on visual techniques alone did the man depend for his success. His voice was a real asset too. He used it with great efficiency. When his voice was heard on the sound track of the English edition of the French film, Cloitures, or when he gave the radio commentary for public Masses in the Boston Cathedral, or for Archdiocesan gatherings at the Ball Park, the voice of Father Ahern was expected and simply taken for granted. We can then understand how he would be anxious to pass on to others the fruits of such experience. At Weston College he was ever interested in, and supported the use of public address systems, acoustical devices, wire and tape recorders and the like.

It is natural then to find Father Ahern representing his shepherd and the flock at various interfaith conferences. He seemed to have success in this from the earliest days of the movement. Invited to speak for the first time to a group of non-Catholic clergymen, he opened his discourse with the words: My dear Brothers in Christ. Most of his hearers never forgot their introduction to Father Ahern. Many still speak of it. Naturally too, he was a favorite in the Catholic pulpit. He was further most generous in his efforts in giving Spiritual Exercises to Jesuits themselves.

This apostolate was recognized. The Gregorian University in Rome conferred her Ph.D. on him in 1931; Canisius College, her honorary LL.D. in 1940; and Tufts College, her honorary ScD. in 1942.

When Father Ahern was President of Canisius College, he inaugurated in 1920 a summer school in science for Jesuit Scholastics. A Jesuit Science Association was to grow out of it. In 1922, the Eastern Division of the American Association of Jesuit Scientists held, on his invitation, its first meeting at Canisius College. Within a year the Bulletin of the Association, now known as the *Jesuit Science Bulletin*, was founded. It has appeared as a quarterly ever since, and is now in its twenty-ninth volume. Father Ahern was President of this Association from 1922-1925. In the early days of the Bulletin, he was a frequent contributor (3).

In 1935, "friends of the Reverend M. J. Ahern, S.J., on the occasion of the twenty-fifth anniversary of his ordination, presented him with a fund to be employed in the modernizing of the equipment" at the Weston Seismological Observatory (4). Indeed Father Ahern's interest in the scientific activity of Jesuit Schools, the Association, the Bulletin and the Weston Seismological Observatory was largely responsible for the advancement of science among the Jesuit colleges in the East. This interest in scientific advance was ever prominent in his life. Again, during the days of World War II, when travel restrictions threatened to suspend operations of the Association, and probably its very life and that of the Bulletin, it was Father Ahern who was to take the leadership in organizing for New England conventions of Jesuit Scientists "in miniature". He showed similar interest in the meetings of the Catholic Round Table of Science.

Father Ahern was a member of the American Chemical Society, a Fellow of the American Association for the Advancement of Science, a member of the American Academy of Arts and Sciences and a host of other organizations.

Father Ahern died at St. Elizabeth's Hospital, Boston, Mass., on June 5th, 1951. In death, his was not the customary low Mass for Jesuits. He was given a singular recognition of a solemn high requiem Mass at the Church of the Immaculate Conception in Boston, Mass. This was attended by more friends, including dignitaries of Church and State, than ever he might during his lifetime surmise. His remains rest in the cemetery of Weston College. May he rest in peace.

Bernard A. Fiekers, S.J.

REFERENCES

1. Nucleus, 9, 110-111 (1932)
2. Chemistry of Today, 1931-1932, Broadcast Supplement of the Nucleus (Northeastern Sec., A.C.S.), nos. 1-6, Broadcast nos. 1-56 (Holy Cross College Library: QD 1. N 9s, 1931-1932).
3. The Length of Geological Time, Proc. Am. Assoc. Jes. Sci., II., 11 (1924); The Recent Evolution Controversy, ibid., III., 6 (1925); Recent Science Controversies, ibid., IV., 4 (1926).
4. Jes. Sci. Bull., *14*, 144 (1937).
5. Who's Who in America, passim, vols. 19, 21, 24, 26.
6. American Men of Science, 1933 on.
7. Obituary. The Nucleus, *28*, 269 & 274 (1951).

Anthropology

THE SYNTHETIC THEORY OF EVOLUTION[1]

J. Franklin Ewing, S.J.

The most important movement in evolutionary studies since the publication of The Origin of the Species has been going on for the last few years. It is now not only clarifying its methodology, but presenting the first fruits of its varied activity. Under the leadership of genetics, the collaboration of paleontologists, geneticists and anthropologists is resulting in the synthetic theory of evolution.

These three disciplines were long so independent, that the ideas of one did not seem applicable to the problem of the other. The geneticist's fruit flies lived in a different world from the paleontologist's fossils; the measurements and morphological comparisons of the anthropologist were literally and figuratively superficial, and ignored by other scientists.

In addition, very naive concepts of the nature of species hindered progress. The old idea of a species was that of a morphological type; there was a lamentably large element of subjectivity in taxonomy. There still is; but its harmful effects are pretty much neutralized by a truer appreciation of the species in nature, and by the attention given to more fundamental problems of mechanism.

The new idea of the species recognizes that the creatures to be classified as belonging to a species compose a group, which is in turn made up of populations, more or less differentiated—often very much so. Pioneering in this field, Goldschmidt[2] showed the differences in various populations of the Gypsy Moth, and also showed how these differences have selective value. Later workers have extended and deepened our knowledge. One of the best studied genera is that of our old friend, *Rana;* Moore's work on this is summarized in one of the volumes recommended in the bibliography.[3]

Added to this knowledge of adaptation to and selection for and by the environment, is another important source of insight. Haldane, Fisher and Wright have subjected the knowledge gained in the field and in the genetics laboratory to mathematical scrutiny. They have supplied the key to that we see in the paleontological picture.

In this article, I intend to present in brief outline what the paleontological picture is, particularly with respect to Man, and then review the genetical conclusions which are pertinent to this picture. At the end, I shall make a remark or two about the influence of genetics on current anthropology.

I.

In Figure 1 I have a very simplified chart of the evolution of the Primates.[4] I can hardly claim that the chart will satisfy everyone; in fact, it does not satisfy me. It does not indicate clearly my present opinion that the tarsioids, cercopithecoids and anthropoids (primitive ones, of course) are in that order belonging to what was to become the human line. The chart would hardly please Straus, who believes[5] that the human line sprang directly from a catarrhine stem, without any anthropoid intermediary; I have placed a dotted line to indicate his view, but it does not seem satisfactory. In this opinion, by the bye, Straus disagrees with Gregory, Weidenreich, Le Gros Clark, and others.[6]

It should be clear that my chart is merely constructed to depict a few, very important trends. These trends are emphasized, because they are the ones found whenever the history of a group of animals is analyzed. These trends I shall summarize under five headings.

(1) All new groups appear with what is, for the fossil records, suddenness. The term is relative, geological time being what it was. This appearance is quite usually concomitant with a disturbed period in the career of the earth's crust. In other words, there were large-scale changes in the environment. The new group appears first in relatively small numbers.

(2) The new "type" (phylum, order, etc.—the same phenomena are noted in connection with races) then proceeds to differentiate rather rapidly into a number of genera or other smaller groups. This is *adaptive radiation.* The animals seek out all possible ecological niches and adapt themselves to these ways of life and environments (arboreal, cursorial; desertic, aquatic, for example).

(3) After this, we see a period of stabilization. Some of the new genera die out; as a general rule, they are replaced by near relatives (competing for the same niche). The successful groups grow greater in population. We have, then a greater number of individuals, and a smaller number of genera, races, etc.

(4) Secondary and tertiary radiations may occur, with succeeding periods of stabilization. A popular period for secondary radiations among the Primates was the Miocene.

(5) The analysis of relationships between forms springing from the same stem may well be complicated by convergence. This, the result of adaptation to the same environment and way of life, may be difficult to distinguish, especially in cases where the fossil record is still scanty. It is particularly difficult to distinguish from differential retention of ancestral characteristics. Comparative anatomy must be checked by paleontology.

Now let me remark on the human data, in the light of these general trends.

It seems clear that a rather rapid change, and a fundamental one,

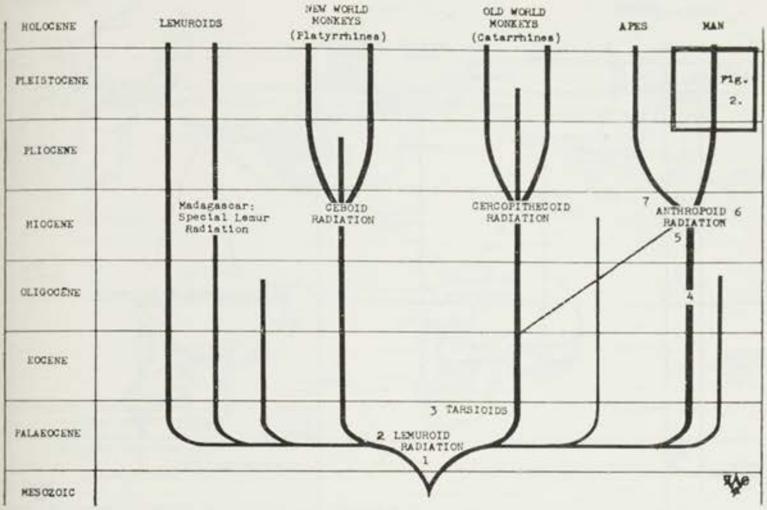

FIGURE 1

Extremely diagrammatic representation of the course of evolution among the Primates

The numbers refer to the following specimens:

1. Notharctinae.
2. Adapinae.
3. Necrolemur, Microchoerus.
4. Parapithecus, Propliopithecus.
5. Pliopithecus.
6. Proconsul, Xenopithecus, Limnopithecus.
7. Dryopithecinae.

occurred in the late Pliocene. This was the acquisition of erect posture. The new stance allowed the hands to be freed from acting as feet, and it gave the brain liberty to expand. The brain-case of even the semi-erect apes is bound in by the necessarily gross neck musculature (compare the gorilla and the modern human skulls in Figure 4).

To demonstrate how even one piece of bone can be evidence of such a change, let us consider the innominate bone (two of which, together with the sacrum, form the pelvic region). In Figure 3 I have crudely drawn the pelvic regions of the gibbon, the gorilla and of Man. In essentially quadrupedal animals, the innominate bone is long and thin (the ilium is not expanded), and parallel to the axis of the vertebral column. In Man, the ilium is flaring, not only because it helps hold up the viscera ("pelvis" means "basin"), but because the bone is the locus of insertion of the gluteal muscles, so diagnostic of erect posture. We have buttocks because of these muscles; no other Primate has anything like human buttocks. The gorilla, being semi-erect, shows an intermediate development of the ilium.

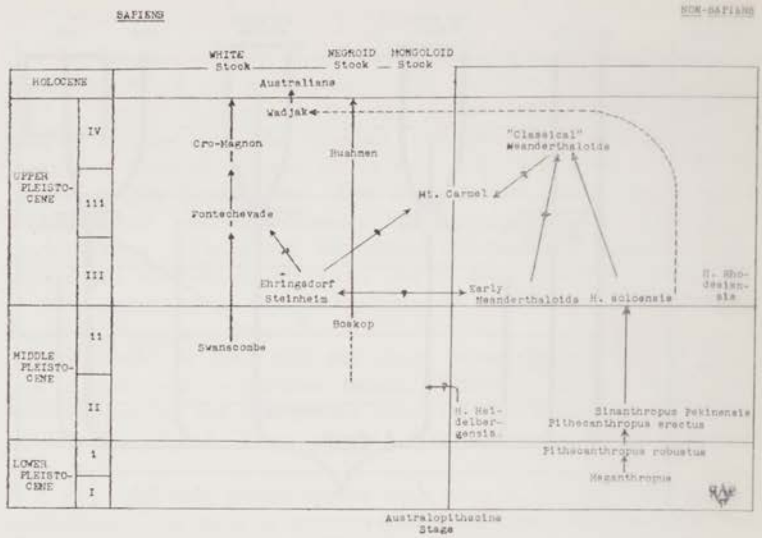

FIGURE 2

Some of the chief characters in human evolution. The large Roman numerals refer to glaciations, the small to interglacials. The spacing of the three parts of the Pleistocene is not representative of temporal duration (quite the reverse: the earlier portions were longer).

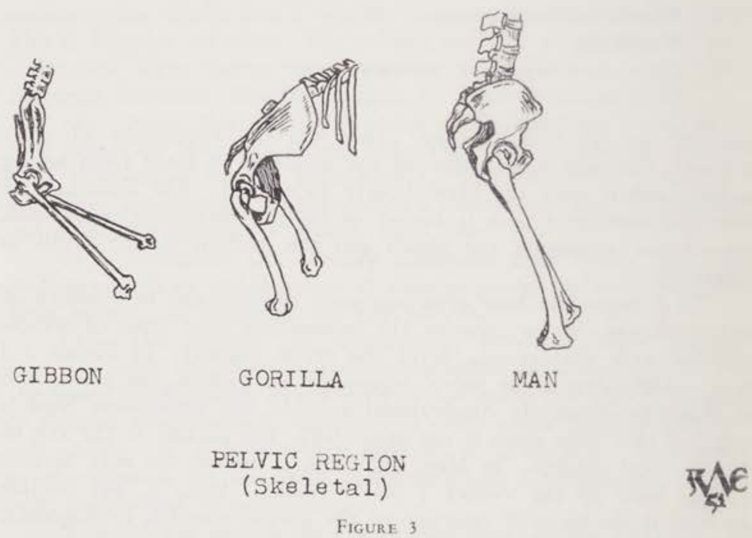

FIGURE 3

Sketches of the pelvic regions to show (from left to right) the essentially quadrupedal type of ilium, the semi-erect, and the human.

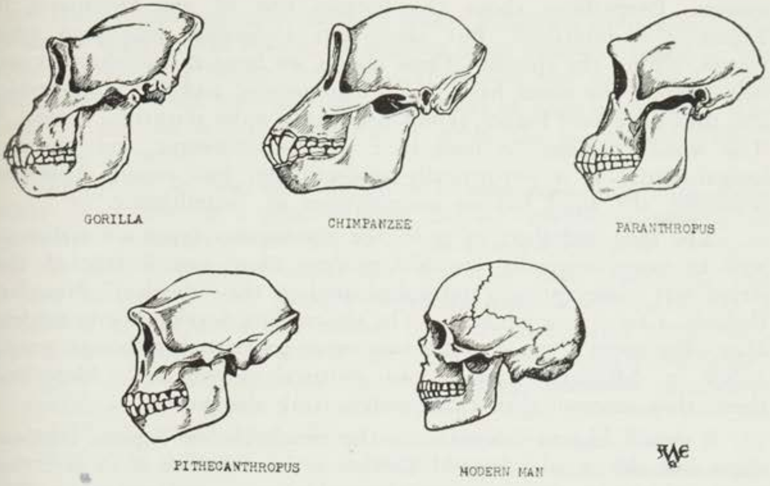

FIGURE 4
Left profiles of the skulls of two modern apes, a pre-hominid, and two types of Man.

An innominate bone, for all intents and purposes hominid in the ilium (the other two parts, ischium and pubis, being somewhat different), is found associated with the Australo-pithecinae remains.[7] These creatures had upright posture. This conclusion is strengthened by the evidence of the skulls and of the limb-bones.[8] The brain has not yet expanded, but the endocranial cast shows more similarity to that of Pithecanthropus than you might expect of a brain about the size of that of the chimpanzee.[9]

One of the great trends in human development has been the expansion of the brain-cortex, and notably of the frontal and parietal areas. Another trend has been the reduction of the masticatory process. The face has been, so to speak, retracted under the forepart of the brain case. In studying this trend, we have discerned "fields" in the teeth and jaws. One field is the anterior teeth, the incisors and the canines. In Figure 4 you note that the gorilla and the chimpanzee retain the large, functional canine. *Paranthropus* (one of the Australo-pithecinae) has a greatly reduced canine, as well as incisors. In this, he was foreshadowed by *Proconsul* and *Limnopithecus*. And these front teeth link *Paranthropus* with *Meganthropus*. Following the study of ancient Man is like watching the negative to emerge from the film, in the darkroom.

At this stage, we are ready for a minor adaptive radiation. Not, this time, of genera, but of sub-species. Almost all the scientists gathered at the Cold Spring Harbor Symposium in June, 1950, were ready to agree that Man was a single, polymorphic and polytypic species. Everything above the bottom line of the Pleistocene in Figure 2 is human.[10] But there was a division into two great classes, within the species. These classes we have called: Sapiens and non-Sapiens (we could have said: Paidomorphic and Gerontomorphic, and thus have used longer, if not necessarily more scientific, words).[11] The word "Sapiens" is used in a purely taxonomic and morphological sense; it is semantically unfortunate, but among American scientists, the word has no connotations of "intelligence."[12]

The long and short of it is: the non-sapiens types are earlier, as well as morphologically less like modern Man; one lineage of this strain was conservative (and culminated in the "classical" Neanderthaloids, a form now extinct). The sapiens line developed into modern Man. For quite a while, these two types co-existed, although pretty much in different physical and cultural provinces.[13] Here and there, they intermingled, thus complicating the picture.

I should like to discourse on the Neaderthaloid-Sapiens relationships, but this would demand another article. Suffice it to say, that much of the controversies on this subject is now irrelevant. The whole question of the origin and development of modern races is also too complicated for this short article. Many careful distinctions, made at great length, would be necessary.

Before I go on to the pertinent concepts derived from genetics, I should like to say a word about new methods of dating, for these have a great bearing on the position of certain forms on the prehistoric chart of Mankind.

You notice, if you are familiar with this sort of study, that I have omitted mention of Piltdown Man. I do this, because the newest advances in fluorine-dating have put this character in the Upper Pleistocene. If this be so, Piltdown remains an interesting enigma, but off the main line of our simple discussion.

Oakley[14] shows that bone absorbs fluorine; the hydroxyapatite which is the hard part of the bone, seizes on fluorine atoms in ground water, turning itself into fluorapatite. The percentage of fluorine in this compound can be determined. However, no absolute scale of dating can be elicited from this evidence, since local conditions vary. Unfortunately, the tropics are practically useless, since bones in these areas quickly absorb the maximum of fluorine. However, the fluorine method is extremely useful, when one compares the amounts of the element in various bones from the same site. Thus, the animal remains of the Piltdown material had much more fluorine than did the human—therefore, the human remains were more recent. This method has been employed on the famous Galley Hill skeleton. This, representing very modern man, was found in a very ancient layer. Controversy raged around it for years. Oakley found that the Galley Hill skeleton had very little fluorine, the animal bones a great deal. I consider it certain that Galley Hill was an intrusion.

Actually, after settling the claims of a whole row of modern-type skeletal remains, this method makes Piltdown even more of a headache than it was before. A modern brain case was found in the same gravels as a simian mandible. If recent, the melange is even less understandable, especially from the point of view of the jaw. The best we can do is put Piltdown in what the accountant calls a "suspense account."

Something of the same difficulty is now connected with Rhodesian Man, but to a greatly lesser degree. Rhodesian Man is so "primitive", that he should not be in the Upper Pleistocene. Oakley[15] has adduced chemical evidence for this later date. However, this is not certain as the fluorine method; also, South Africa is a peripheral area (forms can remain on there later than elsewhere). This is one of the reasons why I mention an "Australopithecine Stage" in Figure 2, without putting these interesting specimens themselves in anything like a direct line with Homo. Here, however, we have a *stage*,[16] very definitely fitting in to Man's history. Even if the actual animals involved are ones which lingered on in South Africa, there are more than enough reasons for saying that this was the sort of thing that must have happened. At the very least, here are a set of facts that must be explained by a positive theory!

I shall say nothing more about dating methods (although the new Carbon 14 method is fascinating, for more recent events), except to mention the fact that the correlations in Figure 2 are based on the work of Movius and others.[17]

II.

It goes without saying that mutation is one of the great factors in evolution. I presume some knowledge of this in my readers.[18] Selection is another great factor. Sometimes we can isolate a particular part of the environment which has obviously selective force.[19] In many more cases we know that the environment as a whole has selective action. Ecological and geographical studies give ample demonstration of this. We are also aware of the importance of genic shufflings and hybridization as additional factors.

But the mathematical study of populations throws the greatest light on how mutation and selection can bring about the formation of new species. And this is the critical process now being investigated. Among the adherents of the synthetic theory, there is not one who holds for sudden great phyletic changes.[20]

Mutation alone supplies the new material for evolution; mutation alone can hardly establish the new character involved. This can not be expected of selection, particularly when mutation supplies what are at the beginning more or less neutral alleles. A process called "genetic drift" here comes into play.

It is well known that the classical Mendelian 1:2:1 ratio (genotypically) or 3:1 (phenotypically) is hardly ever observed in single families or in very small populations. The Hardy-Weinberg equilibrium law holds true only for large populations. If we were to put a male and a female on each of a number of islands, all the couples being endowed with identical genic traits but denied access to any other island, we should soon have quite different populations on the sundry islands. Genetic drift would have occurred.[21]

This drift is most rapid in small populations; it can even negate selection (unless the mutation is lethal, naturally). In our hypothetical example, we have united two powerful forces for speciation: drift and isolation. In addition to these, and in addition to mutation and selection, we should have minor factors, especially true for humans. Such would be assortative mating, which is for the most part culturally determined.

Now, early Man occurred in small population. We know this because of many indications.[22] Scattered as his clans were over the face of the earth and difficult as communications were early Man offered precisely the small isolates favorable for differentiation. Disease and other factors would often provide "bottleneck generations", when the small number would be made even smaller (no doubt, in some case, a single pair). Early Man was also a traveller, and migrant groups would from time to time fuse with indigenous groups. Migrant groups are random, but not necessarily representative, samples of the original population.[23] At all events, there was plenty of time and room for the development, fixation and resorting of traits. In this process, genetic drift played a major role. Drift is the main element in the explanation of Man's polytypy and much of his polymorphy.

I hardly need belabor the fact that all this throws great light on the paleontological record. The great groups (and the lesser ones, too) seem to arise suddenly and to have possessed few individuals. Genetic change is greatest in small populations. Selection played a great part in subsequent change during adaptive radiation.[24] The same is true, but in a different way, during the period of stabilization, when populations eventually became greater. Once established, a species would be less subject to mutation pressure and selection, than to the ordinary processes of equilibrium.

III.

A few words about anthropology and genetics, and I am finished. Physical anthropology was firmly founded, in pre-Mendelian times. Its libraries are full of reports of measurements and observations made with detail and with astounding patience on large numbers of victims. Older anthropologists acted as if they thought that enough measurements would inevitably and eventually furnish the answers.

Almost without exception, current anthropologists have abandoned this attitude. The field of racial anthropology, particularly, has become less and less popular. For the measurements have not yielded any racial classifications on which all could agree, nor enough insight to warrant the further expense of time and energy. What we have been measuring—facial breadth, head length, stature, etc., etc.—has always been genotypically complex. Hence we have not been able to isolate the underlying mechanism. The laborious use of statistics has not obviated this difficulty.

A number of workers have enthusiastically adopted the genetical approach. Thus, for example, Spuhler[25] studies several characteristics in Man which seem to be unit-factor controlled; Birdsell[26] uses genetical constructs to analyse the Australians; and there are many others. The heredity of human diseases and malformations continues to be studied. Most of the work on human heredity to date has been on the abnormal or the pathological.[27]

The most flourishing sphere of interest in this connection is that of blood groups. Boyd's recent book amplifies the attitude of a serologist towards racial anthropology.[28] He overstates his case occasionally, and underestimates the proper contribution of morphology, but in general he has a new, stimulating and eminently useful approach.

All this does not mean that measurement is completely to be abandoned, if, indeed, this is not a truism for a scientist. It does mean that measurement must fit the problem. Obviously, in applied anthropology, when there is question of fitting clothing, machines or prosthetic devices to the human body, measurement is vital. It also has its part to play in another approach, the experimental one. This is the special province of Washburn;[29] he argues that the biologist is not interested in problems of form that can be applied to human form, hence the anthropologist must do the work himself. I think he is right; I wish I could find a biologist student or two who would work with me. I would give him enough problems for the rest of his life! A simple sense of problem helped me in some of my own recent work,[30] even though the initial data were collected in a traditional way. I have since passed on to more serious problems.

Some anthropologists have also tried to focus the human data in the field of selection and adaptation, applying their conclusions to race[31] and even to constitutional anthropology (although with patchy success, so far).

Some of this summary concerning anthropology is biased. There is still a lot of racial study going on, even if I think little of it. And there is a great deal in the knowledge amassed by anthropology which is solid and useful in the consideration of evolution.

IV.

There are many obscurities in our knowledge of evolution. This is inevitable, and it is good. Otherwise, we would not have the stimulating challenge of further work.

There are still great gaps in the fossil record, although the way these gaps have been closing—and, I may mention, in precisely the way theory would demand—is amazing. There are still difficulties with material: you cannot have breeding experiments with fossils! The transfer of results from one animal to another is still beset with cautions.

However, I maintain that the fact and the mechanisms of evolution are known as they were never known before; and, what is more consoling, the various approaches are vibrant with the life of current research and with the hope of greater discoveries right around the corner.

References

[1]These are approximately the remarks made before the Biology Section, at the meeting of the Jesuit Science Association, Eastern States Division, on August 27th, 1951. To the remarks I have appended an annotated bibliography of the recent books on evolution that I have found useful. In the present article, I presuppose what is usually called the "fact" of evolution, i.e., the respectable scientific status of the overall theory. For a very general presentation, cf. my Precis on Evolution, *Thought*, vol. 25 (1950), pp. 53-78.

[2]Goldschmidt, R. 1934. Lymantria. *Bibl. Genet.*, vol. 11, pp. 1-180. This publication summarizes Goldschmidt's previous work on the Gypsy Moth.

[3]Moore, J. A. Patterns of Evolution in the Genus *Rana*, in: Jepsen, Mayr, Simpson-Genetics, Paleontology, and Evolution (cf. bibliography), pp. 315-338. There is a brief discussion of speciation in: Mayr, E. 1949. Speciation and Selection. *Proc. Amer. Phil. Soc.*, vol. 6, pp. 514-519. Mayr's chief work still remains: Systematics and the Origin of Species (cf. bibliography). More recently, see his: The bearing of the new systematics on genetical problems. The nature of species. *Adv. in Genetics*, vol. 2 (1948), pp. 205-237.

[4]Modified from Simpson, 1949 (cf. bibliography), p. 91.

[5]E.g., Straus, W. L., Jr. 1949. The riddle of man's ancestry. *Quart. Rev. Biol.*, vol. 24, pp. 200-233.

[6]Gregory, Evolution Emerging (cf. bibliography).

[7]Broom, R., and J. T. Robinson. 1950. Notes on the pelves of the fossil apemen. *Amer. J. Physical Anthrop.*, n. s., vol. 8, pp. 489-494.

[8]Le Gros Clark, W. E. 1949. New paleontological evidence bearing on the evolution of the Hominoidea. *Quart. J. Geol. Soc.*, vol. CV, pp. 225-264. Also, *id.*, History of the Primates (cf. bibliography).

[9]Dart, R. A. 1948. The Makapansgat proto-human Australopithecus prometheus. *Amer. J. Physical Anthrop.*, n. s., vol. 6 pp. 259-281. Cf. especially: Fig. 5.

[10]The confusion in terminology of ancient Man is extreme, especially when first encountered. Much of this has resulted from people rushing into print with new generic and specific names, without knowing the real relationships of the forms involved. Anthropologists feel that new finds should be named after the place of discovery, until our taxonomic framework is established. Biologists should realize that traditional names are used (as here) with no pretense at proper generic and specific connotations.

[11]The use of "paidomorphic" and "gerontomorphic" recalls Bolk's foetalization theory—that modern Man is as he is, largely because he retains infantile anthropoid characteristics. This theory I consider crude. The use of the terms here simply indicates that the paidomorphic forms are not so emphatic in heaviness of bone and in facial protrusion as are the gerontomorphic forms. The excess of bone found in older skeletons among modern Man (certain tendons having ossified, prostrusions of bone called exostoses having been built up with age), inspires the second term.

A glance at Figure 4 will acquaint the reader with some of the contrasting characteristics of the two groups. The gorilla particularly, among the apes, is a heavy animal, and his semi-erect posture involves massive neck musculature. This is reflected in the great bony crests running along the back and top of the skull. All the apes have great forward protrusion of the face, and a large masticatory apparatus. This apparatus correlates with bony buttresses around the eyes, mechanically necessary for the stresses and strains of the masticatory muscles (especially the masseter). Pithecanthropus, while human, shows a considerable amount of this phenomenon. Paranthropus (one of the Australopithecinae), with a brain-size approximately that of the chimpanzee, clearly has a facial reduction of the prehominid type. The reduction of prostrusion begins with the anterior teeth; the molar size is reduced later on. A real chin develops late. Not seen in the Figure, but indicated by the planes of the neck area of the skull, is another important diagnostic: the position and plane of the foramen magnum (the aperture through which the spinal cord reaches the brain). In the case of the Australopithecinae, this position and plane is practically human.

[12]This is not always true of European writers. In reading French authors, confusion will be avoided by remembering this fact. There are other, and even more difficult, semantic by-ways in the literature on ancient Man.

[13]Movius, Hallam L., Jr. 1944. Early Man and Pleistocene Stratigraphy in Southern and Eastern Asia. *Peabody Museum of Harvard U.*, Papers, vol. 19, no. 3, p. 125.

————, 1949. The Lower Palaeolithic Cultures of Southern and Eastern Asia. *Trans. Amer. Phil. Soc.*, n. s. vol. 38, pp. 329-420.

[14]Oakley, Kenneth P. 1950. The fluorine-dating method. *Yearbook of Phsyical Anthropology*, vol. 5, 1949, The Viking Fund, N. Y., pp. 44-52.

[15]Oakley, Kenneth P. 1950. New evidence regarding Rhodesian (Broken Hill) man. *Yearbook of Physical Anthropology*, vol. 5, 1949, The Viking Fund, N. Y., pp. 53-54.

[16]Le Gros Clark, W. E., *op. cit.*

[17]Movius, Hallam L., Jr., *op. cit.*

[18]Useful texts are mentioned in the bibliography (Stern; Sinnott, Dunn, Dobzhansky).

[19]Some examples of specific studies would be: Heuts, M. J. 1947. Experimental studies on adaptive evolution in *Gasterosteus aculeatus* L. *Evolution*, vol. 1, 89-102.

Basinger, A. J., and H. S. Smith. 1946. Notes on the time of emergence, longevity, and oviposition of codling moths from walnuts, apples, and pears. *Bull. Dept. Agric. Calif.*, vol. 35, pp. 37-38.

Albonico, R. Die Farbvarietäten der grossen Wegschnecke, Arion empiricorum Fer., und deren Abhängigkeit von den Umweltbedingungen. *Rev. Suisse Zool.*, vol 55, pp. 347-425.

Scores of other examples will be found in the bibliographies, especially of Huxley, The New Systematics, and Evolution, the Modern Synthesis.

[20]Indeed, only Goldschmidt, certainly among prominent workers in the field of evolution, believes in "macroevolution," of great jumps of change. However, as his theoretical statement was published some time ago (The Material Basis of Evolution, Yale University Press, 1940), he may have changed his position more recently.

[21]A simple description of drift is given by Stern, pp. 593 ff.; cf. S. Wright, 1949 (cf. bibliography).

[22]It was only with the Agricultural (Neolithic) Revolution, that the overall human population began to expand. This process has also been greatly accelerated since the beginning of the Industrial Revolution.

[23]In fact, several modern studies have demonstrated that, in our times, the emigrant group always differs from the average of those who stay at home.

[24]Lamarckian mechanisms, old-style or new-style, are not necessary for an explanation of the facts. Whatever philosophy may demand, with regard to ontogenetic or phylogenetic principles of continuity, is quite another question, and obviously outside the scope of a purely scientific paper.

[25]Spuhler, J. N. 1950. Genetics of three normal morphological variations: patterns of superficial veins of the anterior thorax, peroneus tertius muscle, and number of vallate pepillae. *Cold Spring Harbor Symposium* (cf. bibliography), pp. 175-189.

[26]Birdsell, J. B. 1950. Some implications of the genetical concept of race in terms of spatial analysis. *Cold Spring Harbor Symposium* (cf. bibliography), pp. 259-314.

[27]Gates, R. 1946. Human Genetics. 2 vols. N. Y., Macmillan. If I remember aright, something like 65 pages of this book of 1518 pages are devoted to the genetics of normal attributes.

[28]Cf. bibliography.

[29]Washburn, S. L. 1951. The new physical anthropology. *Trans. N. Y. Acad. Sci.*, ser. II, vol. 13, pp. 298-304. Washburn's previous experimental reports will be found listed in several bibliographies of the books recommended.

[30]Ewing, J. F. 1950. Hyperbrachycephaly as Influenced by Cultural Conditioning. *Peabody Museum of Harvard U., Papers,* vol. 23, no. 2, pp. 100.

[31]Coon, C. S., S. M. Garn, J. B. Birdsell. 1950. Races . . . A Study of the Problems of Race Formation in Man. Springfield, Ill., Thomas.

(The Bibliography will be published in the next issue)

Science and Philosophy

PHILOSOPHICAL IMPLICATIONS OF PHYSICAL STATISTICS

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By "physical statistics" I mean (1) the classical Maxwell-Boltzmann statistics,[1] (2) the statistical mechanics of Gibbs,[2] (3) the statistical mechanics of Darwin and Fowler,[3] (4) the Bose-Einstein[4] and (5) the Fermi-Dirac[5] statistics.

It is characteristic of each of these physical methods that they are designed to deliver probability predictions concerning observables that are verifiable by measurements, conformable to the statistical theory of errors.

This study will therefore (1) review briefly the statistical theory of errors, (2) expound and examine the conventional calculus of probability, (3) discuss the state of the question concerning the disputed definition of probability, (4) depict and compare the relevant content of the representative systems of statistical mechanics, (5) compare and contrast the role of probability in pre-quantum and post-quantum systems, and (6) close with some provisional remarks concerning the philosophical implications of items (1) to (5).

1. The Statistical Theory of Errors

The basic motive behind all improvements in precision of instrumental techniques of research is to secure maximum validity of the results by reducing to a minimum all extraneous perturbations of the experimental apparatus. But no matter how many feasible precautions have been taken, it is a palpable fact of experience that the apparently precise repetition of a particular measurement operation under apparently identical conditions will rarely deliver the same numerical result. Given then successive measurements that do not exactly coincide, it is of crucial importance to decide which numerical value may best be chosen to represent the quantity in process of measurement. It is the role of the statistical theory of errors to provide the best possible answer to this fundamental question in quantitative research.

Let the quantity in question be indicated by q, and let a set of measurements of q under presumably carefully controlled experimental conditions be q_1, q_2, q_3 . . . q_n. From the set of n values it it required to produce one which shall be acceptable as the satisfactorily correct one under the circumstances. This will obviously be some sort of average of the q's. The simplest type of average is the arithmetical mean. Given this mean value, it is a simple matter to calculate the deviations from the mean of the various measured values.

It is moreover an observed fact that if one plots as ordinate the number of values as a function of the deviation from the mean, a frequency curve is generated which, although it differs in detail for different experiments, nevertheless always possesses certain general but definite characteristics. The deviation from the mean is of course not a continuous set of values. But if one divides the total range of deviation into a set of equal intervals, and in the center of each interval plots the number of measurements for which the deviation falls within it, then as n is made sufficiently large, a smooth curve constructed through the resultant points generally resembles the Gauss probability curve in the following respects: (1) there are many more values for which the magnitude of the deviation is small than there are values for which the magnitude of the deviation is large; (2) the number of values for any particular positive deviation tends to approximate the number of values for the corresponding negative deviation interval. Thus there always tends to be a maximum in the curve.

If there were no accidental errors involved in processes of measurement, one would expect the same value q to result from each and every observation. The known differences therefore among the q's may be called errors, and the frequency curve described above may be termed an error-curve in which the ordinate gives the number of ascertained cases in which the error lies within a particular interval. By division by n the ordinate may further represent the probability of an error lying within the given interval.

Now it is demonstrable that if one takes the most probable value of a measured quantity as the arithmetical mean, the law for the Gaussian error-curve can be derived. But it is important to know whether one can give an estimate of the error involved in the arithmetical mean itself. And it can likewise be shown that the probability that the arithmetical mean shall represent the 'correct' value for a quantity grows with the square root of the number of observational measurements of the quantity. The arithmetical mean therefore is $\sqrt{n}$ times as accurate as any one of the measured values.[6] The theory of errors is indeed fundamental for estimating the validity of all physical measurements, but it is especially relevant to verification tests of the probability predictions of statistical methods in mechanics.

2. The Calculus of Probability

Closely connected with probability predictions are probability calculations [*Wahrscheinlichkeitsrechnung*]. And the conventional calculus of probability is in fact that set of functional schemata in which the notion of probability is accepted as an undefined primitive, and in which theorems are demonstrated for the *transformation* of assumed probabilities into other significant and related ones. In this way the calculus of probability renders feasible a more adequate testing of probability statements by making explicit the predictions that they imply.

From an elaborately developed calculus of probability four such theorems may be adduced:

(1) The General Product Theorem:

$$P(AB, R) = P(A, R) \times P(B, AR)$$

i.e., the probability of both characters A and B in R (the chosen reference set or class) is equal to the product of the probability of A in R, multiplied by the probability of B in A and R.

(2) The Special Product Theorem:

$$P(AB, R) = P(A, R) \times P(B, R)$$

i.e., the probability of both A and B in R is equal to the probability of A in R, multiplied by the probability of B in R, *if and only if* it is also given that A and B are independent of each other with respect to a definite R.

(3) The Special Addition Theorem:

$$P(A \vee B, R) = P(A, R) + P(B, R)$$

i.e., the probability of A or B in R is equal to the probability of A in R, plus the probability of B in R, *if and only if* A and B are exclusive with respect to R.

(4) The Division Theorem:

Since it is obvious that

$$P(AB, R) = P(A, R) \times P(B, AR) = P(B, R) \times P(A, BR)$$

i.e., that the probability of both A and B in R is the same as the probability of both B and A in R, then

$$P(B, AR) = \frac{P(B, R) \times P(A, BR)}{P(B, R) \times P(A, BR) + P(\bar{B}, R) \times P(A, \bar{B}R)}$$

i.e., the probability of B in A and R is equal to the quotient of the the product of the probability of B in R and of A in B and R, divided by the sum of the products (1) of the probability of B in R and of A in B and R, and (2) of the probability of not-B in R and A in not-B and R.

Such then is the general structure of the abstract calculus of probability which supplies demonstrated theorems to guide specific operations in the probability calculations of statistical physics.[7]

3. The Definition of Probability

There is indeed universal acceptance of the central issues in the calculus of probability. But there is as yet no generally acceptable definition of this most important concept. There is no alternative therefore to a survey of representative opinions.

Poincaré[8] in fact declared it impossible to formulate a satisfactory definition of probability without committing the fallacy of a vicious circle. And in contemporary times Ville[9] despairs of achieving agreement in the welter of modern controversies. And the edge of the controversy in fact cuts deep into fundamental issues. It is conventional, for example, to speak casually about the probability of an 'event'. But Keynes[10] maintains with some vigor and competence that probability is not a character of events at all, but a logical property that pertains exclusively to propositions or to statements about events.

Some clarity however may be introduced into the confused picture if one notes that the various proposed definitions of probability really do not pretend to explicate the concept, but rather to show how probability is open to processes of measurement. Jacques Bernoulli,[11] for example, characterizes probability as the *measure* [*gradus*] of the strength of our expectation of a future event. And it is von Kries'[12] opinion that probability statements express a more or less *degree* of justification for an expectation. Careful examination of the modern literature shows that probability definitions are for the most part only thinly disguised rules for calculating respective weights. In the words of Czuber[13] such definitions do not describe what the term 'probability' means, but rather suggest how the implied quantitative measure and correlated mathematical manipilations are to be handled. From this point of view the case is systematically similar to the routine definition of 'speed' in physics as the ratio of distance to time [$v = d/t$], where one rather proposes a method for its measurement than a definition of its meaning.

But there is in fact a 'classical' definition of probability and it is still held in high respect in many quarters. It is simple and intuitive in character. By the probability of an event is here understood the ratio of favorable cases to the sum total of all equally possible cases. The origin of this definition is Jacques Bernoulli.[14] But Gini[15] has rightly shown that Bernoulli meant his formula, not as a definition of probability, but rather as a measure of probability, previously assumed as intuitively clear and sufficiently understood.

The point of the 'classical' formulation cannot be appreciated unless one recalls the fact that modern concerns with probability calculations began in the field or games of chance where measureable amounts of cash were wagered and either won or lost. And it was the thrust of Jacques Bernoulli's treatment to try to show that probability calculations were not limited to the area of such gambling wagers. But in this expansion of the field of application of probability calculations theorists continued to operate with the conventional classical formulation. It became however painfully obvious in time that this definition breeds paradoxes and difficulties so soon as one uses it in fields other than games of chance. Poincaré,[16] Borel,[17] Lévy,[18] each takes the classical definition as a formal point of departure in analysis. So does Czuber.[19] But the latter prefixes the following assumptions: (1) one must prescind from all casuality involved in the events and erect an hypothesis of a pure and absolute chance; (2) one must also presuppose a complete independence of one event from any other; (3) one must presuppose that the cases concerned are all equally possible.

These are indeed ample assumptions. And the crux of the classical definition has always been the precise meaning of the notion of 'equipossibility'. It is apparent that the classical definition itself supplies information whatever concerning which cases are to be regarded equally possible. In the philosophical controversies centered upon s issue two opposite points of view have been defended: (1) the principle of *insufficient reason* by which two cases are held to be equally possible if there is no known or conceivable reason to expect one alternative more than the other, and (2) the principle of *cogent reason*, which is presumably applicable as a distinct principle when some sort of symmetry may be urged as conclusive, as in games of oin-tossing where the counters have but one head and one tail.

There is one main criticism, however, that has ceaselessly been harged against the classical definition. It is claimed that the definition commits a vicious circle or a *petitio principii*. For it is urged that its 'equipossible' cases are equivalent to 'equiprobable' cases, and thus a definition of *idem per idem* results. It may however just be true that this standard objection is itself an *ignoratio elenchi*. For the alleged 'definition' really seems to propose a determination of the *measurement* of a probability. For a probability ratio is only a special case of a relation between two quantities. And it is Finsler's[20] pointed contention that probability can have no absolute and isolated sense at all. Nor is this circumstance to be construed as a logical defect. For similarly in geometry the length of a line is measureable only if some standard is imported and some procedure established for the comparison of relative lengths. And Gini[21] reminds us that on this paramount point Jacques Bernoulli possessed a more valid insight than the majority of modern theorists.

The classical definition of probability also occurs under the tag of *a priori* probability. It is however a recognized fact that in practical calculations the cases are relatively rare in which boundary conditions permit an *a priori* determination of probabilities. But it is this failure in practical matters that apparently gives birth to the hope that probabilities, unknown *a priori*, may however suitably be established by observation of relative frequencies in large enough series of trials.

This important division of probability into *a priori* and *a posteriori* also derives from Bernoulli. And it would seem that Bernoulli believed that his theorem not only equipped him to predict observable frequencies on the basis of a known *a priori* probability, but also to determine empirically to any desired degree of approximation an unknown probability. This is perhaps the reason why Castelnuovo[22] insists on a sharp distinction between both types of probability as the sole remedy against pernicious theoretical errors fatuous controversies. And Gini[23] dubs the transition from direct to inverse probability, explicable by a failure to observe this distinction, the 'original sin' of probability calculations.

Besides the classical definition then there continued to grow an empirical formulation that terminated within modern times in the interpretation of probability in terms of relative frequency. In this evolution there is apparent the steady urge to bring the objective and pragmatic significance of probability calculations to the fore. The development of this powerful idea can be traced from Cournot[24] through Ellis,[25] Mill,[26] Venn,[27] von Kries,[28] Bruns,[29] Marbe,[30] Fisher,[31] up to von Mises[32] and beyond. But the exposition of von Mises remains still standard. His contribution was to give to the theory an axiomatic formulation in terms of a collection, probability aggregate, or *Kollektiv*. The core of von Mises' theory is contained in the four following items: (1) the concept of *Kollektiv* is prior to that of probability; (2) probability itself is defined and identified as the limit value of relative frequency; (3) an axiom of irregularity or randomness [*Regellosigkeitsaxiom*] is necessary; (4) the task of probability calculations is thus for the first time rendered mathematically rigorous and precise.

'Probability' thus becomes for von Mises just another word for 'the limiting value of a relative frequency in a *Kollektiv*', where a *Kollektiv* is a non-finite series of events, possessing a random character [*Regellosigkeitsaxiom*] and the property of tending toward a limit [*Grenzwertaxiom*]. This formulation has met with a wide and respectable acceptance. Coolidge,[33] Baptist,[34] and Steffensen,[35] among many others, have given it support. But there was also considerable resistance. Popper[36] objected to the union of the *Grenzwertaxiom* and the *Regellosigkeitsaxiom* on the grounds that it was illegitimate to apply the mathematical notion of limit to a series that was by definition not amenable to any law of constructibility. Ville[37] explores the same territory and Vietoris[38] concludes in general that any probability theory that includes the *Grenzwertaxiom* is inherently and irremediably contradictory.

Under the impact of these and similar critiques many authors, such as Copeland,[39] Kamke,[40] Dörge,[41] Tornier,[42] Reichenbach,[43] and Wald[44] undertook a revision of the theory in the sense that only the *Grenzwertaxiom* be maintained while the *Regellosigkeitsaxiom* be dropped or severely weakened. This procedure toward reform was considerably energized by the conviction that it was impossible to provide an existence proof for a *Kollektiv*, defined as by von Mises in terms of the *Regellosigkeitsaxiom*. Popper[45] however argues that the improvement of the *Regellosigkeitsaxiom* is a strictly mathematical and hence professional affair, whereas the *Grenzwertaxiom* is an affront to any sound epistemology. For the endless series and its postulated limit value defy all empirical verification tests, not only in point of fact but also in principle.

Many more recent writers have detoured from these avenues of revision and directed their attention more precisely to the statistical point of view. Anderson,[46] for example, substitutes for the infinite *Kollektiv* the statistical *Kollektiv*, a rather random finite collection, and defines probability within the statistical *Kollektiv* as its frequency in another collection of higher order whence the given one may be derived. The definition which Fréchet[47] gives is typical of this trend: the frequencies of a random event in an extensive series of trials are empirically ascertained values of a physical constant that is characteristic of the event and of the order of the trial series. This physical constant is called the probability of the event. In other words, *the* probability of an event is *measured* by its frequency in a series of tries. And the precision of the *measurement* is in general a function of the length of the series of tries. But it is not necessarily true that the margin of error decreases as the series increases, and in any case the *identification* of probability with the limiting frequency is false. A similar conception appears in Dubourdieu.[48]

At the present time much energy is being devoted to an axiomatic foundation of the probability calculus. This enterprise is not exactly new. For in its own way it is a return to the classical point of view. The book of Kolmogoroff[49] is representative of this movement. The central purpose is to integrate the basic ideas of probability calculus into the group of constructive concepts of modern mathematics. One notes, for example, the abstract structure of Markoff,[50] the set-theoretical foundations of Urban,[51] the work of Finsler,[52] and Cramér.[53]

The foregoing survey therefore suggests a triple classification of theories concerning the definition of probability: (1) the classical, (2) the frequency interpretation, and (3) the axiomatic.[54] From an epistemological point of view the entire controversy seems to stem from an unresolved conflict between 'rationalism' and 'empiricism'. This is clear from the character of the disputes in which rationalist is pitted against empiricist, *a priori* vs. *a posteriori*, subjective against objective, mathematical vs. statistical, orthodox against modern. Harmony here must then await the resolution of the central issues of a responsible philosophy of science.

But in the interim one must note that a probability calculus, as a mathematical creation, is a system of abstract statements. If consistent and free from derivative contradictions, its mathematical passport is assured. But more is necessary to naturalize its citizenship in the world of physical science. No theorems in an axiomatically structured calculus directly refer to empirical events. The issue of concrete interpretation of the calculus is therefore of paramount importance. A validity test can only be supplied by experience. It is Cramér's contention that a conjunction between system and experiment can best be provided by the frequency interpretation of probability. For it is the role of probability in science to function as a physico-mathematical tool. Under the circumstances, and they are ambiguous indeed, I am disposed to agree with Cramér and to follow along with the instinctive assurance of most that despite the many divergent interpretations of probability, neither the calculus of probability itself nor the validity of its applications to experience have been or will be weakened. The situation is indeed bad. It can only get better.

4. Systems of Statistical Mechanics

But until it improves there is no alternative but to do the best that one can with the notion of probability as it functions in the techniques of statistical mechanics. For it is the task of statistical mechanics to reinterpret, for example, thermodynamic observables, such as temperature, pressure, and entropy, and to effect a reduction of all thermodynamic equations. But neither of these tasks can be performed with the conventional tools of mechanics. For other and distinctly different instrumental constructs are required. These new tools are of a statistical sort and involve the notion of probability in an essential way. Statistical mechanics is thus a discipline in its own right, related indeed to mechanics, but operating in terms of certain theoretical constructs uniquely its own. And it is important to discern just why and at what points probability enters into these techniques.

For purposes of illustration let us select one thermodynamic observable, the pressure, and see what is implied in its statistical reinterpretation. As a thermodynamic observable, the pressure of a gas is the force exerted by the gas upon unit area of the walls of its container. In the native concepts of mechanics and in terms of molecular theory this pressure becomes the amount of momentum delivered per unit of time by the molecules impinging upon a unit area.

But out of this picture a perplexity instantly arises as soon as one observes that whereas the thermodynamic pressure is constant, its mechanical counterpart varies erratically in time and from point to point on the surface area of impact. No single measured value could be assumed as representative of the thermodynamic pressure. The task then is to eliminate in some way the rapid fluctuations in the rate at which momentum is delivered to the walls of the vessel by the impinging molecules. The obvious solution is to try some averaging process. One could indeed take the mean value of the momentum per second over a sufficiently large time interval at the point of pressure measurement. Or one could take the average of the quantity at any one time over the entire surface of the enclosing vessel. Both of these and other arbitrary procedures are available and legitimate. For the principles of mechanics leave the choice open and free.

What is really needed, however, is the introduction of some *Kollektiv*, the assignment of probabilities to its properties, and then the derived calculation of the mean value of the dynamical quantity in question. The events which form the *Kollektiv* may be the successive impacts of one definite molecule, or of some specified group of molecules, or they may be the simultaneous collisions of many molecules with the wall at some fixed time, or any other feasible choice. No principle of mechanics prescribes a unique procedure. Physics may choose any probability aggregate [*Kollektiv*] which works, one namely which leads to satisfactory agreement with the thermodynamic experimental measurements (conformable to the statistical theory of errors). It so happens that several alternative choices all work more or less well. Hence there are several kinds of statistical mechanics.

4a. The Maxwell-Boltzmann Statistics

In the initial investigations of Maxwell and Boltzmann applications of statistical methods were not of a rigorously systematic character. Rather vague and somewhat hesitant probability arguments were used. But they do not pretend to be fundamental to the method and are treated on approximately the same level as strictly mechanical considerations. Two main features characterize this period: (1) quite extensive hypotheses are proposed concerning the structure and the laws of interaction between the particles. For example, these are usually represented as elastic spheres, and their laws of collision are incorporated in an essential way within the structure of the theory. (2) The notions of probability do not appear in any precise form and are infected with a certain amount of confusion that often dis-
the mathematical arguments when it does not invalidate them al-
credits the mathematical arguments when it does not invalidate them altogether. In general the mathematical level is here very low and barren. For the most important mathematical problems, incidental to statistical mechanics, do not even appear in an exact and recognizable form.

4b. The Theory of Gibbs[55]

The first systematic exposition of the foundation of statistical mechanics, with rather widely developed applications to thermodynamics, was given by Gibbs. Gibbs improved on the theoretical structure of his predecessors by declining to make any specialized hypotheses about the nature of the particles involved. For a thermodynamic body may be regarded as a mechanical system which has a dynamic state, specifiable in terms of a certain number of coordinates and an equal number of momenta. If the system is regarded as composed of M molecules, this number is 3M. For each molecule's position (whatever be its internal nature) is described by its x, y, and z component. One may further assume that the dynamic system possesses a certain number of degrees of freedom. This is the minimum number of coordinates necessary for the description of the mechanical configuration of the body, and will here be called n. For a gas consisting of M molecules, $n = 3M$. The complete *state* of the system is thus represented by 2n numbers: n coordinates and n momenta. But 2n is an extraordinarily large number, easily of the order of 10^{24}.

The tabulation and computation of these numbers throughout changes of state of the system is impossible. What is therefore required is a more elegant and manageable conception of the changing state of the system.

To illustrate Gibbs' method, forget the swarm of particles and concentrate on the simplest type of mechanical system: a mass-point, perhaps a single molecule, moving along the x-axis. It has but one degree of freedom and its complete state is specified by a pair of numbers: x and p. If this pair of numbers is plotted on a plane graph with x as abscissa and p as ordinate, they are represented by a dot on a plane, or more precisely, by a point in a space of two dimensions. As the molecule moves about, its surrogate point describes a curve in the two-dimensional plane. But if we now replace the point by the molecule, it now moves in two dimensions, and four numbers are required for its complete description: x, y, p_x, and p_y, which correspond to a point in four dimensions. It is patent that this four-dimentional space cannot be imaginatively visualized. But Gibbs appropriated the device of multi-dimensional spaces and exploited their properties for his own needs.

A system thus with n degrees of freedom and hence with 2n variables of state is represented by a point in a space of 2n dimensions, and changes in the state of the system are recorded by motions of the surrogate point along a curve in a 2n-dimensional space. Such hyperspace thus supplies a useful formalism for accurate thought and easy analysis. For such space possesses very simple mathematical properties. This multi-dimensional space Gibbs called the *phase-space* of the thermodynamical system. In this phase-space the dynamic career of the system is represented by a curve with interesting and important properties. For the principles of dynamics prove that it can never cross itself, and unless it be periodic, it must ultimately pervade the entire volume of its space. And since every dynamic observable (energy, for example, total momentum, angular momentum, etc.) of the system is a function of the 2n variables composing the dimensions of the phase-space, *a definite value of each observable is determinately associated with every point of phase-space.* The path of the moving representative point therefore marks a determined succession of values for every observable. Given the location in phase-space of the point at a specified time, one knows what is the energy of the body, its momentum, etc., at *that* time. But it is a disconcerting fact that these values fluctuate very rapidly from instant to instant and therefore convey little useful information about thermodynamic observables, such as pressure.

Precisely at this critical juncture Gibbs introduces an original *Kollektiv* for the purpose of taking mean values. He constructs an *ensemble* of systems, that is, he imagines a great number of thermodynamical systems, all replicas of the given one. He envisions millions of similar vessels, all filled with the same quantity of the same gas.

He then assumes easily that the dynamical *states* of these cognate systems will, however, not be identical. Thus each member of the ensemble will have its destiny represented by a point moving in phase-space, and the hole ensemble will appear in phase-space like a dust cloud in which each dust particle pursues its own path. But the *density* of this cloud will differ from place to place, and it may vary in time at any given place. Yet every particle avoids collision with its neighbor and visits every part of its world.

Among all the *possible* states of motion of the cloud, however, one is especially important, namely, that one in which the density at every point of phase-space remains constant in time. This state Gibbs correlates with the equilibrium condition of the thermodynamical system. And since thermodynamic laws apply to equilibrium conditions only, attention may be centered uniquely on this state of motion. Gibbs calls this constant-density condition the *canonical distribution* of the representative points in phase-space, corresponding to the ensemble. Note that in its canonical distribution the dust cloud is not motionless. For the points move, but in a manner which leaves the density of all points unchanged.

There is no doubt that Gibbs here introduces a very special hypothesis of a statistical character without a serious attempt to establish its relevance or to interpret its physical significance. But the beauty of Gibbs theory lies in its success. For if one averages the rate of momentum loss per unit area of wall over the entire steady dust cloud (the canonical distribution), the correct value for the pressure of the actual thermodynamical system is obtained. And if one averages the energy over the canonical distribution, the correct value for the internal energy of the system is found. And a similar success is achieved for all of the other thermodynamic variables. It just happens that the canonical distribution prescribes the probability distributions (densities of the cloud at different places in the phase-space) which the various possible values of an observable must have in order to yield the correct mean value. In this way *statistical analogues* are produced for all of the thermodynamic variables.

The meaning of Gibbs' construction is clear. The task was to find a certain mean value for rapidly fluctuating observables. But to find a mean, one must know the weights, that is, the *probabilities*, of individual events. Dynamics does not supply these. Hence it was necessary to postulate a probability aggregate (*Kollektiv*) in which the values of dynamic observables are the elements, and Gibbs' theory is precisely that postulate. It says in effect: the probability of a value r belonging to a dynamic observable is proportional to the density D of the canonical cloud at the place in phase-space where the observable has the value r. Knowing D as a function of r is the sole requirement for computing the mean. And Gibbs' theory is a successful set of rules for obtaining D, that is, for finding the probabilities.

The statistical mechanics of Gibbs is elegant and impressive. But it has its faults. The mathematical level is not high, and although the arguments are reasoned adroitly from the logical point of view, they lack analytical rigor. Moreover Gibbs bestows small attention to the molecular structure of thermodynamical systems and thus fails to provide fruitful correlations with known facts in other scientific areas. And while the introduction of probabilities by way of ensembles achieves success, it does not illuminate the depths of the physical processes involved. Hence it has been found difficult to harmonize Gibbs' statistical method with quantum mechanics. For the discontinuous character of the quantum phenomena does not jibe easily with canonical distributions of ensembles in phase-space.

4c. The Method of Darwin and Fowler[56]

Instead of considering the number of degrees of freedom of the thermodynamic body Darwin and Fowler concentrate on the molecules that compose it. Assume that each molecule bears a unique number from 1 to N, and that each molecule can acquire any value of an observable. Its speed, for instance, can be either 1.5 cm/sec or 1000 cm/sec, etc., its energy can be either 1 erg or 10^{-16} erg, or any other value. It will be convenient here to focus attention on the energy. Now on purely mechanical grounds it is not plausible that a single molecule whose motion remains altogether unspecified, will possess with equal probability an energy of 1 erg and 10^{-16} erg. The former value is far more probable, according to the principles of dynamics. For there are many more possible states of motion in which a single molecule, enclosed within a vessel, can have an energy of 1 erg rather than 10^{-16} erg. And at this level of consideration no significant issues of statistics are involved. Assume too that the energy scale is calibrated into small areas of different lengths, the lengths being so adjusted as to make it equally probable on mechanical grounds for a molecule to possess the energy of one segment as of another. Let these segments also be tagged with numbers.

When attention is now transferred to the assemblage of N molecules, strictly proper statistical assumptions are introduced. And here one distinguishes two types of state with respect to this assemblage: (1) a *microscopic* state in which single molecules are assigned to specific energy segments, such as molecules number 5, 691, 1959 fall into segment number 1 (or have energy within that specific range), whereas molecules number 12, 751 fall into segment 2, and so on, until all the molecules are allotted to their proper segments. If and when such a microscopic state is known, the total internal energy of the assemblage can be simply computed by summation of the energies of the individual molecules. (2) A *macroscopic* state which may be defined as follows: there are three molecules in segment 1, two molecules in segment 2, and so forth, until the *number* (but not the identity) of the occupants of each segment is specified. One therefore requires much less information to describe a macroscopic state than a microscopic one. Nevertheless the total energy of the assemblage is fixed and computable when the macroscopic state is known. The central point to be grasped here is that a single macroscopic state comprises or is compatible with a considerable number of different microscopic ones. In fact the number of microscopic states contained in a macroscopic one is equal to the number of permutations of the various molecules between the different energy segments. Hence so far as the thermodynamic body is concerned, interest is centered exclusively upon the macroscopic states. For an observable property has the same value for all the microscopic states (or configurations) that compose a macroscopic one. To find then the mean value of an observable, it is sufficient to calculate its average over all of the macroscopic states.

But different macroscopic states are *possible*. And how is one to distribute probabilities among them? Darwin and Fowler reply by way of a special postulate that thereby distinguishes their system of statistical mechanics: *the probability of a macroscopic state is directly proportional to the number of microscopic states which it contains.* This assumption seems eminently reasonable. But it is not prescribed by the laws of mechanics. The interesting thing is that it works. For it leads to satisfactory agreement between the computed mean values and the measured quantities of thermodynamics. The statistical analogues of Darwin and Fowler are irreducibly different from those of Gibbs, but they satisfy equally well the correct thermodynamic relations. Moreover the system is enriched by providing a systematic computation of the average values. Previously a more or less convincing determination of 'most probable' values was assumed without rigorous justification to be approximately equal to the corresponding average values. Darwin and Fowler also created a simple, convenient, and mathematically rigorous apparatus for the computation of asymptotic formulae. In this respect Darwin and Fowler exhibit a significant improvement on Gibbs.

In both systems however the primary function is to *construct* probabilities that issue in predictions about observables that are verifiable by measurements, conformable to the statistical theory of errors. Statistical mechanics therefore succeeds in transforming some latent observables into possessed observables by introducing into the theoretical structure some suitable probability aggregates. It remains to be seen whether such devices work equally well in all the areas of modern physics.

(To be continued)

SCIENCE FACULTIES OF NEW ENGLAND CATHOLIC COLLEGES AT MID-CENTURY—A STATISTICAL REVIEW

BERNARD A. FIEKERS, S.J.

Recently the writer had occasion to compile a mailing list of all professors of mathematics and the natural sciences at Catholic Colleges in New England. A collection of the catalogs of their respective colleges for the year1950 provided the primary source of information for this list. This afforded an eminent opportunity for creaming off statistics with a minimum of additional effort. They are presented here.

There are 23 Catholic colleges in New England: 13 for women; and 10 essentially for men. This excludes seminaries, religious houses, juniorates and the like. The graduate faculties of two colleges in the area which offer graduate courses were likewise excluded. Catalogs of five colleges were either not available or failed to publish a list of faculty members. In two cases the faculty lists were acquired through private correspondence.

The list carries a total of 188 names. Of these 166 were listed for teaching one science only. Men outnumber women, 137 to 51; lay people outnumber religious, 106 to 82; lay men outnumber religious men, 89 to 48; religious men outnumber religious women, 48 to 34; religious women outnumber lay women, 34 to 17.

Eight religious men, 7 religious women, 7 lay men (but no lay women) teach more than one science. Each of two college deans teaches one of the sciences. The list is confined largely to biology, chemistry, mathematics and physics; meteorology and bacteriology claim one full-time woman professor each. By full-time, we mean that a professor is engaged in no second science.

Mathematics claims the largest overall number of professors: 39 full-time, 27 engaged in other sciences also: total 66; then biology, 44 full-time, 21 in other sciences also: total 65; then chemistry, 56 full-time, 5 in other sciences also: total 61; finally physics, 25 full-time, 14 in other sciences also: total 39. But full-time occupation in a single science shows chemistry leading with 56; biology, 44; mathematics, 39; and physics, 25. The combinaiton of two subjects occupying the largest number of professors is mathematics-physics: 14.

Many of the catalogs do not give the doctor title along with the designation of religious order or congregation. But the practice of indicating doctorates among lay people seems to be uniform and reliable. The figures are: 14 men's doctorates in chemistry; 7 men's and one woman's in biology; 3 each men's doctorates in mathematics and physics; total 28. All of these doctors are listed for teaching one science only.

It seems that in general a woman's preference in science follows the following order: biology, chemistry, mathematics and physics. This holds for a lay woman. With religious women, the order is chemistry, biology, mathematics and physics. With men the order runs: chemistry, mathematics, biology and physics. This order holds for lay men. With religious men the order runs: mathematics, physics, chemistry, biology. With Jesuits the four subjects are about equally populated.

In absolute numbers, the largest overall mathematics and science faculty in the area is Boston College, 43 members; next, Providence College, 23 members; next the College of the Holy Cross, 22 members. Of the 23 colleges, 16 have 10 science professors or fewer. The Department of Chemistry at Boston College indicates the largest number of laymen with doctorates on its staff, 7 out of 12. Providence College indicates the largest number of religious who teach scientific subjects, 15 out of 23. If we neglect the colleges with ten or fewer mathematics and science staff members, Providence College has the largest percentage of religious teaching these subjects. The Department of Physics and Mathematics (combined) of the College of the Holy Cross naturally accounts for 6 out of the 14 in the area who teach both mathematics and physics.

Of the 48 religious men, 21 are Jesuits. They are distributed as follows: biology, 5 out of 11; chemistry, 6 out of 11; mathematics, 3 out of 10; physics and mathematics, 3 out of 5; and physics, 4 out of 8: total, 21 out of 48.

Figures such as these may be pleasing to those who have a flare for the statistical. But they really help us to take account of our position in science and mathematics at mid-century. There may be as many interpretations as there are readers. If they stimulate us to try to get a better objective insight into the course our work is taking, they will have accomplished their purpose. It is believed that these figures are substantially correct according to the sources from which they stem.

MEDIEVAL INTEREST IN AND OBSERVATION OF NATURE[1]

AN HISTORICAL NOTE

JOSEPH P. KELLY, S.J.

"This healthy interest and commendable curiosity concerning real things was not confined to Albert's students nor to the "rustic intelligences." One has only to examine the sculpture of the great thirteenth century cathedrals to see that the craftsmen of the towns were close observers of the world of nature and that every artist was a naturalist too. In the foliage that twines about the capitals of the columns in the French Gothic cathedrals it is easy to recognize, says M. Mâle,[2] "a large number of plants: the plantain, arum, ranunculus, fern, clover, coladine, hepatica, columbine, cress parsley, strawberry plant, ivy, snapdragon, the flower of the broom and the leaf of the oaf, a typically French collection of flowers loved from childhood." Mutatis mutandis, the same statement could be made concerning the carved vegetation that runs riot in the Lincoln cathedral. "The thirteenth century sculptors sang their *chanson de Mai*. All the spring delights of the Middle Ages live again in their work—the exhilaration of Palm Sunday, the garlands of flowers, the bouquets fastened to the doors, the strewing of fresh herbs in the chapels, the magical flowers of the feast of St. John—all the fleeting charm of those old-time springs and summers. The Middle Ages, so often said to have little love of nature, in point of fact gazed at every blade of grass with reverence."[3]

But it is not merely a love of nature but scientific interest and accuracy that we see revealed in the sculptures of the cathedrals and in the note-book of the thirteenth century architect, Villard de Honnecourt,[4] with its sketches of insect as well as animal life, of a lobster, two parroquets on a perch, the spirals of a snail's shell, a fly, a dragonfly, a grasshopper, as well as a bear and a lion from life, and more familiar animals such as the cat and swan. The sculptors of gargoyles and chimeras were not content to reproduce the existing animals but showed their command of animal anatomy by creating strange compounds and hybrid monsters—one might almost say evolving new species—which nevertheless have all the verisimilitude of copies from living forms. It was these breeders in stone, these Burbanks of the pencil, these Darwins with the chisel, who knew nature and had studied botany and zoology in a way superior to the scholar who simply pored over the works of Aristotle and Pliny."

[1]Thorndyke. "History of Magic and Experimental Science." Vol. II p. 536. Macm. N. Y. After describing St. Albert's interest in scientific details, he adds this comment on the acute power of observation and the accuracy of reproduction, characteristic of the medieval artisans and sculptors.

[2]E. Mâle. "Religious Art in France in the 13th Century." P. 52.

[3]*Idem*.

[4]*Idem*. p, 53.

JESUIT CLASSICS OF SCIENCE

BERNARD A. FIEKERS, S.J.

Two of the source books in science carry items of historical interest to "ours" in the form of "contributions" from Father Athanasius Kircher, S.J., 1602-1680, and from Father Francesco Maria Grimaldi, S.J., 1618-1663.

Father Kircher's item is entitled "the Subterranean World" and appears in Mather and Mason's *Source Book in Geology* (1). It has been translated from Kircher's *Mundus Subterraneus*, which was published in Amsterdam in 1678. The editors state that this work was the standard geological treatise of the seventeenth century.

The contribution of Father Grimaldi is entitled "Diffraction of Light" and appears in Magie's *Source Book in Physics* (2). The extract is taken from Grimaldi's *Physico-mathesis de Lumine, Coloribus et Iride* (1665). The editor states that Father Grimaldi is known as the discoverer of the diffraction of light.

It seems to this contributor that there must be somewhere available a large number of Jesuit Classics of Science, which could well be excerpted, translated and published in THIS BULLETIN. To suggest material, one can mention among others Fathers Algue, Boscovich, Clavius, Faura, Hagen, Kugler, Lana Terzi, Perry, Ricard, Ricci, Schall (Bell), Scheiner, Schott, Secchi, Vitoria, Wasmann, and Wulf.

REFERENCES

1. Mather, K. F. and Mason, S. L., *A Source Book in Geology*, 1st ed., McGraw-Hill, N. Y., 1939.
2. Magie, W. F., *A Source Book in Physics*, 1st ed., McGraw-Hill, N. Y., 1935.

RELIGIOUS PERSPECTIVES OF COLLEGE TEACHING IN THE PHYSICAL SCIENCES, *by Hugh S. Taylor*, Dean, Graduate School, Princeton. This is one of a series of essays put out by the Edward W. Hazen Foundation, 400 Prospect St., New Haven 11, Conn. These essays "are available gratis in limited quantities. Orders or inquiries should be addressed" to the above Foundation.

The purpose of the series is outlined in the preface. "Three years ago, Professor George F. Thomas of Princeton University . . . urged the need for careful studies by natural scientists, social scientists and humanistic scholars concerning the religious issues, implications and responsibilities involved in the teaching of their respective disciplines."

Fortunately they chose Hugh Scott Taylor, an outstanding Catholic Scientist, to undertake the essay of which this is a review. To report on this remarkably clear statement on the relation of religion to other areas of knowledge would, in justice, require the transcription of the essay *verbatim*. However, here is an attempted synopsis.

After allowing that science has achieved an exalted role in human affairs of today, he states his thesis. "It is the purpose of this essay to deny that science can ever assume the central position in human affairs to which present tendencies appear to urge it. It will suggest that these tendencies arise from a mistaken view with respect to the nature of science and ignore its limitations. It will set forth briefly the essential nature of science. It will trace, in barest outline, how science has developed from its earliest origins to the position of intellectual hegemony into which it may be elevated. It will record how religion and science have areas which they share in common, and how also they each possess areas distinct and characteristic."

1. "It will set forth briefly the essential nature of science". "The scientist is concerned with the nature of the physical world as it is apprehended through the senses. His method is experimental. . . . It is also theoretical in that an attempt is made to correlate observations and provide a unified structure of reasoning, which will embrace, ever more successfully, the sum total of things observed."

2. "It will trace . . . how science developed." In the 16th century, Francis Bacon proposed "the examination of our universe by observation and experiment instead of through the medium of philosophy and the deductive method." Soon the Royal Society of London was founded and similar societies in Germany and France. In those days, be it noted, however, that science was accepted as part of the "universitas" of all learning. As years went by, fragmentation (specialization) of studies obtained. Today, this fragmentation has attained a high position of importance.

3. "Religion and science have areas they share in common." For example, the Einstein equation and the atomic bomb. Moral problems immediately arise. The University, therefore, must integrate these and other relationships which enter into Western Culture.

4. "Each possesses areas distinct and characteristic".

   a) There are certain types of experiment which will not immediately evoke a question of "purpose", as Milliken's experiments on oil drops.
   b) There are certain problems which touch on faith, as the "days" of Creation.
   c) The data of astronomy, physics or chemistry have little to tell us of the regulation of human behavior.
   d) Knowledge from science touches only on "proximate causes". Knowing nothing of "ultimate causes", it can not determine ends.

In concluding, Dr. Taylor says: "In the pursuit of wisdom, the teacher of science must find the opportunity to convince the student that beyond the areas covered by science and scientific conclusions, beyond the testimony of history, there are areas of truth which supplement those of knowledge to yield *sapientia.* These embrace art, literature, philosophy and religion."

In the encyclopedia of knowledge, there should not be any separation of science from religion. All knowledge forms one great unity. Yet, because of the age-long struggle for primacy between the material and the spiritual, we face an ominous stumbling-block. And, quoting Christina Rossetti, "Up Hill", he asks: "Does the road wind up hill all the way? Yes, to the very end." *Joseph J. Sullivan, S.J.*