A. M. D. G.

American Association of Jesuit Scientists

Eastern States Division

PROCEEDINGS

of the

Fourteenth Annual Meeting

August 31, September 1, 2, 1935

Holy Cross College, Worcester, Mass.

Published at

LOYOLA COLLEGE

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Bulletin of American Association of Jesuit Scientists

EASTERN STATES DIVISION

Vol. XIII | OCTOBER, 1935 | No. 1

ANNOUNCEMENT

For twelve years the "Bulletin of the American Association of Jesuit Scientists", Eastern States Division, was published for the members of the Science Association in the New England States and Middle Atlantic States. By the request and recommendation of V. Reverend Daniel M. O'Connell, S.J., National Secretary of Education, the *Bulletin* will soon become a NATIONAL QUARTERLY including all the Universities, Colleges and High Schools of the United States. We hope that negotiations will be completed during the present scholastic year. The above Correspondents were appointed by Father O'Connell as Assistants to the Editor, in order to bring about a coalition of the Provinces of the United States for the advancement of scientific pursuits in Jesuit education.

THE EDITOR.

Fourteenth Annual Meeting

PROGRAM OF GENERAL MEETINGS

Saturday, August 31, 7.45 P. M. — **Room 17, O'Kane Bldg.**

Address of Welcome........Rev. Francis J. Dolan, S.J.
Reading of Minutes........Appointment of Committees
Presidential Address........Rev. Francis W. Power, S.J.
Scientific Research in Our Colleges
New Business........Adjournment

Monday, September 2, 1.00 P. M. — **Room 17, O'Kane Bldg.**

Reports of Secretaries........Reports of Committees
Discussion........Resolutions
Election of Officers........Adjournment

PROGRAM OF SECTIONAL MEETINGS

Sunday, September 1, 9.00 A. M.—3.30 P. M.

Monday, September 2, 9.00 A. M.

BIOLOGY SECTION

Beaven Hall — **Room 33**

Chairman's Address........Rev. James L. Harley, S.J.
Photosynthesis: The Nature and Chemistry of Chlorophyll
Rev. Harold L. Freatman, S.J.
The Contractile Vacuole of Paramecium........Rev. John A. Frisch, S.J.
The Nutritive Functions of Carbohydrates
Rev. J. Franklin Ewing, S.J.
Salivary Gland Chromosomes and Genes..Rev. Charles A. Berger, S.J.
Blood-Groups and Serologic Tests in Legal Medicine
Rev. Clarence E. Shaffrey, S.J.
Digestion and Absorption........Mr. John J. O'Brien, S.J.

CHEMISTRY SECTION

O'Kane Building — **Room 14**

Chairman's Address........Rev. Joseph J. Sullivan, S.J.
Jesuit Training in the Sciences
Micro Analytical Methods........Rev. Richard B. Schmitt, S.J.
Report of Research Work done at Chemo-Medical Institute of Georgetown University........Rev. Lawrence C. Gorman, S.J.

A Method for Rapidly Determining the Hydroxyl Number of Some Waxes and Oils........Rev. Edward S. Hauber, S.J.

Work on Identification of Fatty Acids at Holy Cross College
Mr. Joseph A. Martus, S.J.

Visual Method of Solving Chemical Problems...Mr. Leo J. Guay, S.J.

MATHEMATICS SECTION

Alumni Hall Room 25

Chairman's Address........Rev. Frederick W. Sohon, S.J.
Isomorphism

Statistical Laws and Causality........Rev. Joseph P. Kelly, S.J.

PHYSICS SECTION

Alumni Hall Room 24

Chairman's Address........Rev. Emeran J. Kolkmeyer, S.J.
Productive Scholarship

Some Recently Completed Laboratory Projects at Canisius College
Rev. John P. Delaney, S.J.

A Relation between Voltage and Wave-Length
Rev. John A. Tobin, S.J.

Some Home-Made Instruments; Illustrated
Rev. Emeran J. Kolkmeyer, S.J.

Symposium on Atomic Physics:

The Rutherford-Bohr Nuclear Atom...Mr. James J. Devlin, S.J.

X-Ray Spectra:; Atomic Spectra of the Alkalis
Mr. William H. Dowling, S.J.

Perturbation of Electron Orbits by Electric and Magnetic Fields
Mr. William H. Schweder, S.J.

Periodic System of the Elements: Pauli's Principle
Mr. Gerald F. Hutchinson, S.J.

The Nucleus........Mr. Theodore A. Zegers, S.J.

FIRST GENERAL SESSION

The Fourteenth Annual Meeting of the American Association of Jesuit Scientists, Eastern States Division, was held at Holy Cross College, Worcester, Mass., on August 31, September 1 and 2, 1935. The first general meeting was held on Saturday, August 31, at 6.30 P. M. in the Chemistry lecture hall, the Rev. Francis W. Power, S.J., President of the Association, presided.

The Rev. Francis J. Dolan, S.J., President of Holy Cross, was unable to attend the initial meeting. He, therefore, delegated the Rev. Joseph J. Sullivan, S.J., Dean of the Chemistry Department, to welcome the scientists to Holy Cross.

After Father Sullivan's greeting, a motion was made to the effect, that, since the minutes had already appeared in the September, 1934, issue of the Bulletin, a repetition of them by the Secretary would be unnecessary. This motion was duly seconded.

Father Power then named the following committees:

Committee on Resolutions; Fr. Richard B. Schmitt
Fr. Henry M. Brock
Fr. Thomas J. Butler

Committee on Nominations; Fr. Joseph J. Sullivan
Fr. Clarence E. Shaffrey
Fr. Thomas H. Quigley

Father Schmitt, the Editor of the Bulletin, then announced that at the request of the Rev. D. M. O'Connell, Prefect General of Studies in the American Assistancy, the Bulletin is to become a National Journal for the American Jesuit Scientists. Associate Editors from the various Provinces have already been appointed.

Father Joseph Kelly of Weston College announced that he has been collecting back issues of the Bulletin, in order that he might furnish complete sets of the magazine to all of our Colleges and High Schools. These numbers may be had by applying to Father Kelly at Weston College.

Father Francis W. Power then concluded the session with his Presidential address.

PRESIDENTIAL ADDRESS

Research In Catholic Schools

REV. FRANCIS W. POWER, S.J.

In the following remarks I speak expressly of graduate work, research, or productive Scholarship along the lines of physical science, but what I say applies with equal or greater force to the equally controversial subjects of history, psychology, and education, and to the equally influential subjects of literature and the drama. Furthermore, I am directing my remarks particularly to the younger men, to stimulate them to interest in scientific research, to point out its need in our schools, and the opportunities for influence which it will give us.

The most usual difference between college work and university work is this, that in college the student gets the essential part of his information from his professors and his text books; in the university (in physical science work at least) the student gets his essential information (i.e., *qua* graduate student) directly from nature in the sense that the particular question he puts to nature has never been put before. In other words, the college student (as a college student precisely) gets his information second-hand; the graduate student (as a graduate student precisely) gets it from the sources. The one is formally receptive; the other productive.

On account of the quite different technique required for these two types of scholarship, it is obvious that the professor who directs students in graduate work must be a master of it himself—"Nemo dat quod non habet"; but one of the questions which is of serious import to us is, to what extent is an extensive graduate training necessary for the professor who conducts college courses, or even high school courses?

There is no doubt about the answer to this question in the minds of those who in point of fact have a great deal to do with the educational policies of the secular colleges, especially those in the West. The various regulations which they make regarding what graduate degrees must be held by college and high school teachers are known to all, and clearly show that their idea of a college faculty is one composed of men with doctors' degrees from eminent universities, testifying that the professors have acquired not only the information which they are to hand on to their students, but also the further training and information that comes from the technique of personal scientific investigation.

The question may be amplified thus: to what extent should the college or high school professor have been a research worker, and to what extent should he do research work while he is doing college work?

If I should assure you that I proposed to discuss fully and settle these venerable and highly controversial questions today, I would be only making of myself one of those individuals who rush in where angels fear to tread; but a few reasons pro and con will not, I hope, carry me too far into deep water.

There is, of course, one obvious danger and fallacy into which those may fall who push the idea too far of "standardizing" college faculties in terms of graduate degrees; namely, that of overstressing the purely informational aspect of a college education. It is traditional with us that formation is the end, and information only one of the means; our ideal college education is an integrated education, the aims and methods of which were so well set forth in the Fordham Convocation addresses of 1933, 1934, and 1935 by Father Bull, Father Cox, and Father Joseph A. Murphy respectively. To quote from the Holy Cross catalog, "Holy Cross College, therefore, following a system elaborated by centuries of experience and meeting the demand of modern improvements by wise adaptation and readjustment, undertakes to mould the characters of her students to habits of patient industry, of mental and moral temperance and intelligent interests."[1] I am fond of putting it this way: that the Jesuit system of education does not propose to train a man for his first job. An over-emphasis on the mere information possessed by the faculty and acquired by previous participation in graduate work will be in great danger of over-emphasizing the merely informational feature of the professors' part in the formation of the students according to general program just referred to. I speak here advisedly of over-emphasis; many vices are but virtues pushed too far, and while there is no danger of our losing sight of our general program when we gradually increase our number of strictly graduate-trained professors, there is the danger in outside schools of giving the students the impression that detailed and exact information is all that the faculty are interested in giving them.

Since, as I say, there is no danger of our students getting such an impression, I think that the rapidly increasing number of our men being assigned to strictly graduate studies possesses the obvious advantage that we are assured beforehand that the professor is thoroughly conversant not only with the actual class matter which he teaches, but also that he knows all around it so to speak; and he has the training, and let us hope the ambition also, to continue along with his teaching the investigational work he started during his graduate training. This suggests an answer to the old question about what research work is expected of a college teacher. To my way of thinking he should do all he possibly can. It is not his first duty, of course,—his first duty is to teach his class—to do his share in the integrated system of which he is a part. But secondarily, and as a corollary of this, his active participation in productive work makes him acquainted with his scientific colleagues in other schools, en-

1—Catalog 1935, p. 27.

livens his lectures, and permits him to speak with the authority of a man who knows not only what is in the text book but how it got there, who put it there, and what is likely to be there when the next edition comes out; he may even be able to say that some contribution of his own will appear when the book is re-edited. The influence and prestige with the students of such a professor is hard to overestimate.

I would like to refer briefly here to a common objection against research men as teachers: namely, that most of them are *not* teachers—that they cannot put the matter across to beginners, that they go over the heads of the students, and the students pass from deep slumber into a deeper flunk. I do not think much of this objection. I have, of course, been bored to extinction, as most of you have, at scientific meetings when some individual gets up all full of his subject and proceeds to deluge his audience with facts poorly assembled, presented usually on an illegible lantern-slide or chart, explained in a monotonous mumble directed away from his hearers, and whose only impression on them is one of unbounded relief when the speaker sits down. That is often set up as the performance of a typical research man; you say to yourself, God help the students who have to listen to that individual three or four times a week! My view is, however, that such a speaker is a bore and an unmitigated nuisance not because he is a research man but because he has no more personality than luke-warm water, and if he were reading the most powerful passages from Dickens or Shakespeare or Patrick Henry a man would pay no more attention to him than he would to a dismissed taxicab. There are plenty of research men who can present an audience with a blackboard covered with formulas or equations and make them like it, and come back for more. The biographies of almost any of the old masters are full of allusions to the personal charm and magnetism of the man and how his lectures were crowded to the doors by all sorts of students; Victor Meyer and Liebig and Dumas and St. Claire-Deville were men of that sort; so are Millikan, Hugh Taylor and Frank C. Whitmore today. If a research man turns out to be an incomprehensible bore when he faces an audience we should not blame that on his research training but rather on himself. There are plenty of first class research men who are brilliant and inspiring teachers, and there are plenty of first class teachers who would never make research men; there are simply two kinds of genius or ability—sometimes they are combined in one individual and sometimes they are not.

The teachers who have not the genius or interest for research work are not to be thought the less of for that, although as I have said before they lack, I think, that influence and authority that comes only from personal participation in productive work. At all events, whatever be your opinion on the question as to this participation, there can be no doubt about the standards commonly accepted today for the training of college and even high school teachers. In fact, so completely "sold" are outside educators on this matter that they practically judge the excellence of a college from the number of Ph.D's on its faculty, and from the standing of the university from which the degrees were granted; and from this the high school and even grammar schools are judged according to what degrees the teachers have and from what school they came.

As a de facto situation this is becoming as you know very serious and was set forth in no uncertain terms by Fr. Cooper of the Catholic University of America at the meeting of the Catholic Round Table of Science in Pittsburgh this spring. His remarks should, I think, spur us to do our best to meet the situation each one according to his own ability and under the direction of Superiors. Fr. Cooper's remarks point out the need for scientific research in our Catholic schools and allude to the historical reasons why its development has up to now not proceeded as rapidly as would be desirable. I will discuss these points briefly.

The first reason why more productive Scholarship should be engaged in by our Catholic schools and teachers is the extrinsic one that has been mentioned—namely, so that those controlling the trends of college education outside shall find our colleges and our men second to none in meeting the criterion which they set up that the professors be graduate trained men who do graduate work themselves; a criterion which, as I have said, has many reasons in its favor.

In the second place, an active and continuous participation on our part in scientific productive work is badly needed on behalf of our Catholic people. It is a trite statement to say that we are living in a scientific age. The public has been "sold" science so thoroughly and for so long that the sayings and doings of scientific men are front page matter everywhere, and their conclusions have for the past century had a leading part—perhaps the leading part—in forming the public mind, especially outside the Church, on matters even relating to life and conduct. So many of the great masters of the past century were materialists that their science and its mystery and dogmatism effectively established agnosticism as the official religion of science in the Victorian period; and now that the old-fashioned crass materialism of that period has been rendered obsolete by a later generation of scientists it has given place to the Positivism and the vague Pragmatism that characterizes current scientific thought. It is interesting to note, however, how much slower our scientific men have been to abandon the old-fashioned materialism in this country as compared with their colleagues in England and the Continent.[2] The remnants of the old classical and philosophical culture which exerted and still exert a certain tempering influence on European scientists have had no such influence in the United States since the

2—The Religion of Scientists—C. L. Drawbridge—MacMillan 1932.

rise of Electivism and Utilitarianism beginning with Eliot, who, I grieve to say, started off in life as a chemist! The result is that among practically all non-Catholic circles, in scientific publications, in "popular science" (God save the mark!) and in the public press, Evolution is accepted with as much assurance as Copernican astronomy, and a doubt expressed as to the validity of its general case would be treated by scientific men, and even by the public, in the same way they would treat a man who held that the earth was flat.

What have we, or whom have we, to represent the position of moderate realism in the face of all this positivistic unanimity? Compared with our population ratio (say 15%) in the United States we do not make a very impressive showing. For example, the last edition of "American Men of Science" lists 601 scientists chosen by their colleagues as really outstanding in all the various fields of science; the editors then set these men up in two lists—first, showing from what institutions three or more of these outstanding scientists received their degrees, and second what are the institutions with three or more of these outstanding scientists on their faculty. It is not reassuring to note that not a single Catholic college or university appears on either of these lists. That is to say, no Catholic institution is the alma mater of even three of the outstanding American men of science, and no Catholic institution has even that number on its faculty. Another example to show our comparative position in science may be drawn from the membership of the National Academy of Sciences. This body of men comprise what we may call the cream of American scientific scholarship. In the current membership list (June 1935) we find 285 names, and on looking over the institutions with which these men are connected we find not a single Catholic institution mentioned, nor as far as I know is any one of the men a Catholic personally. I suppose there are a few but certainly they are not those who are household words among us as being outstanding and militant Catholics. Another evidence of our late start along research lines is well known to all in the report published last summer by the American Council of Education in which the "recognized" graduate schools of the country were listed and in which only two Catholic institutions appeared—Notre Dame for chemistry and the Catholic University for history, classics and philosophy.

In Fr. Cooper's remarks previously quoted he refers to the excellent historical reasons for the lack of proportionate representation on the part of Catholics in productive scholarship. I would set forth these historical reasons as follows:

Our grandfathers came to this country for the most part less than a century ago with very little except their good health, their native intelligence and ability to work, and their sterling Catholic faith. When they came here they found education, social position, financial and political power, and everything else that goes to make an independent and influential culture in the hands of an alien, disdainful, and often bigoted majority. They found a meager hierarchy directing the heroic efforts of a mere handful of priests and religious towards preserving as best they could the faith of the thousands who poured in from the old world. They undertook the tremendous task of starting a pioneer enterprise in the face of powerfully entrenched competition; and so well did they succeed and so wisely and unselfishly did they build that we today, only recently descended from them, can point to an active and militant hierarchy, an army of competent and devoted priests and religious second to none in the Catholic world, and a well-organized and efficient educational system running from primary schools up to the University. And all this in about three generations. Any critical study of any phase of Catholic education which we now make must pause at the outset to pay tribute to superhuman efforts of its early pioneers on whose well laid foundations we build today.

It would naturally be expecting too much of these pioneers that they should in their leisure time engage in graduate work and meet the upper-level intellectual competition of their contemporaries whose position had already become consolidated by a century of comparative affluence. The early American bishops, priests, and religious had no leisure time; they had to spend themselves and their meager resources in the immediate and pressing problem of keeping the faith alive in their widely scattered flocks—they had no time for research work!

To my mind, however, there is the very real danger that many Catholic educators especially of the older generations are apt still to think in terms of pioneering; but as I see it Catholic education is now definitely out of its pioneer stage. If then this is beginning to be realized, as recent developments indicate, we must expect, as open competitors in the intellectual world, to be evaluated in the same terms and by the same standards as are commonplace among learned men everywhere.

Whether we like it or not, the standard on which a college or university professor is judged today is this: what contributions to productive scholarship has the man made *and published*. In other words, to what extent is he known among his intellectual colleagues as a man who appears regularly before them at their professional meetings and in the columns of their professional publications?[3] The day is past when it is sufficient to be a learned man; it is necessary now that everybody knows one is a learned man. The light under the bushel is replaced by the city set on the mountain, which cannot be hid.

The recent developments with which we are all familiar, point, I think, to a far deeper realization of all this than would have been thought likely 10 years ago; and point out clearly to our younger

3—Not in the local newspapers.

men to whom these remarks are primarily addressed their responsibility to the general cause of Catholic education and the opportunity which lies before them of putting the Church, the queen and foster-mother of true scholarship, before the American public in the intellectual position which is hers by divine right and in history.

This responsibility and opportunity focusses, I think, on what I said before about the dominating position now held in current thought by scientific men; namely, the universal and unshakable conviction of 99% of educated non-Catholics that the Church is opposed to science on principle—that she is afraid of creative scientific work—that dogma is the procrustean bed upon which science and history must be forced to fit, no matter what violence is done to reason or fact. I consider this the bed-rock difficulty which we as Catholic educators have to face, and I regard it as our prime and foremost responsibility and opportunity that we make an earnest start at demolishing it even though I fear none of us here will ever live to see the day when it has been even partially refuted to the satisfaction of non-Catholic scholars.

In so far as we are high school and college teachers we attempt to exert our influence from the bottom up as I may say; insofar as we participate personally in open intellectual competition with leaders of thought outside we exert our influence as it were from the top down. Both these methods are traditional in the Society; the second as well as the first. Xavier, Campion, Canisius and dozens of our early Fathers, under the general idea of Ignatius himself, engaged in open and successful intellectual competition with outside leaders of their own day; and the careers of de Rhodes and de Nobili still excite our admiration at the extraordinary pains they took to meet their adversaries on their own ground and beat them at their own game. This opportunity is still ours; indeed it is perhaps more insistent now than it was then. I do not see how we can reconcile an intellectual apostolate with intellectual insularity.

We all realize that the current popular belief about the fear of the Church for productive scholarship can easily be refuted by a few syllogisms; we all realize too that it is still more easily refuted by an appeal to history. The two examples of Pasteur and Mendel are sufficient of themselves to explode it. But Pasteur and Mendel are dead; so are Secchi, Dumas, and Copernicus. To my way of thinking it is perfectly futile in practice, taking people as they are today, to expect them to accept our position on the relation of science to revelation, or of philosophy to theology, by presenting them with syllogisms and biographies. What they want and must have, is the unescapable and irrefutable evidence of a group of Catholics, especially Catholic laymen, who are commonly recognized as outstanding scholars by their intellectual colleagues, and as outstanding and militant Catholics by everybody. That is the goal towards which we must strive, and all of us can at least point it out, in season and out of season, to our students, whether we ourselves for one reason or another, may never be numbered among such leaders.

Naturally this sort of intellectual apostolate is not for everybody, and it is just as well that it is not. An interest in research work is, as I have said before, merely one species of genius or ability which one man is born with and another is not. It is neither higher nor lower than any other of the particular aptitudes with which heredity and environment have endowed different men; I usually tell my students that it is a sort of disease which makes its victims exceedingly uncomfortable but they seem to like it. It only happens that in our present situation as it has been set forth, it is a very useful endowment for anyone who has it. If we do not sow we cannot reap; and unless we have an abundance of competent teachers to give our students a proper grasp of elements and fundamentals we can never expect any of them to become scholars afterwards. This is so well recognized by many schools (such as M. I. T. for instance) that the most able and most distinguished member of the staff is assigned to teach the Freshmen; both to give the young students the benefit of the inspiration which such a man can give them, and also to avoid the common prevalence among upper-class teachers of "passing the buck" back to the professor who started the students off.

I have set forth, chiefly as I have said for the benefit of the younger men, the need of scientific research on the part of our men; and I have proposed the motives that may induce those who are interested, to engage in it as actively as circumstances will permit. Our opportunity for influencing outside intellectual movements is before us. When the leaders of current scientific thought start to generalize—to philosophize—which they cannot help doing any more than anybody else, they direct their followers along the dangerous and tortuous paths of materialism and positivism and away from the "True Way that leads to Eternal Life." With the background which we have we are in a position to direct intellectual currents in the direction which we all desire. All that we ask for is a hearing; and I fear that we shall never get it until by sheer merit and recognized achievement we have won the right to be listened to with attention and respect by our intellectual equals.

We are pioneers also if you like; pioneers in the effort to impress a distinctly Catholic touch upon the American culture which is slowly developing out of the confusion of business and industrial activity going on around us. As we would now blame the earlier pioneers if they had proved recreant to their duty of establishing the fundamentals of the Faith in their people, so too can the generations who will succeed us condemn us in all justice if we, the pioneers in establishing a higher type of intellectual culture, do not now rise to the occasion and bring all our forces to bear upon it that it may become, as far as we can make it, an American Catholic culture.

FINAL GENERAL SESSION

On Monday, September 2, at 10.30 A. M., the final general meeting of the convention was held in the Chemistry lecture hall.

The Secretaries of the sections reported the officers for the coming year as:

Biology;
Chairman—Father J. Franklin Ewing
Secretary—Mr. Francis X. Wilke

Chemistry;
Chairman—Father Arthur J. Hohman
Secretary—Mr. Leo P. Guay

Mathematics;
Chairman—Father John P. Smith
Secretary—Mr. William H. Schweder

Physics;
Chairman—Father John P. Delaney
Secretary—Mr. Severin E. George

Report of the Committee on Resolutions

The following resolutions are proposed for approval:

1. Be it resolved that the American Association of Jesuit Scientists (Eastern States Division) express its appreciation and gratitude to Reverend Father Rector, Father Minister and the Community of Holy Cross College, for their cordial reception and for the gracious hospitality shown to it during its convention. They have contributed to the success and pleasure of the meeting.

2. Be it resolved that the Association express its sympathy to Reverend William C. Repetti, Chief of the Seismological Department of the Manila Observatory, in his recent illness and also its sincere hope for a rapid recovery, so that he may be able to continue his effective scientific work for the Philippine Mission and the Society.

3. Be it resolved that we express our appreciation and gratitude to the various officers of the Association for the labor entailed in making this meeting a success.

4. Be it resolved that a copy of these resolutions be presented to Reverend Father Rector of Holy Cross College by the Secretary.

Committee on Resolutions,

REV. RICHARD B. SCHMITT, S.J.
REV. HENRY W. BROCK, S.J.
REV. THOMAS B. BUTLER, S.J.

A request was made by Father Ewing through Fr. Power that the various members of the Association aid Bishop Hayes of the Philippine Islands with such Scientific apparatus as they can possibly spare.

Father Joseph Kelly then repeated his offer to furnish copies of the Bulletin and any other assistance that he can to members who will write to him on this subject.

The Editor of the Bulletin discussed the literary and financial aspects of the Association's publication and he asked for more contributional cooperation from the members. This was followed by a discussion as to which title would be given to the Bulletin, now that it is about to become a National Jesuit Publication. Fr. Frisch proposed that the phrase "Eastern States Division" might be deleted. Fr. Sohon suggested that, until the other Provinces in the American Assistancy are united to form a National Scientific Organization, we would do well to retain the nomenclature of the past fourteen years; Because, until the formation of a National Organization is completed, the Bulletin will remain the proper organ of the Eastern States Division, the original sponsors of the publication. Fr. Schmitt at the request of Fr. Brock, read Rev. Fr. O'Connell's communication relative to this matter.

Fr. Brock then proposed that the highly informative school note be retained in the Bulletin, even at the expense of an article, if necessary.

Fr. Joseph Kelly then reminded the assembly of the interest aroused by the articles in the Philosophical-Scientific Department of the Bulletin. Fr. Schmitt then brought to the members' attention the reprint of Fr. Kelly's article of the May Bulletin in the June 22nd issue of "The Catholic Mind." He also proposed that a rising vote of thanks be tendered Fr. Kelly for his energetic efforts along these lines.

The nominating committee then announced that Fr. Brock and Fr. Frisch were proposed for the Presidency, and that Mr. Devlin and Mr. Carroll were suggested for the Secretarial position. In the ensuing election Fr. Brock was elected to the office of President and Mr. Carroll to that of Secretary.

A vote of thanks was given to the retiring officials for their work during the year and also for attending to the numerous details concerning the Convention.

The motion that the convention adjourn was made and carried.

BIOLOGY

THE NUTRITIVE FUNCTIONS OF CARBOHYDRATES AND FATS

(Abstract)

REV. J. FRANKLIN EWING, S.J.

A description of the effect of the consecutive organs of the alimentary tract on carbohydrates and fats. The problems of absorption and assimilation were discussed. Some late theories on intermediate chemistry, especially of fats, were given; and the deposition and mobilization of these two of the three primary foodstuffs were analyzed. Finally, a summary of recent views of the interconversion of foodstuffs ended the paper.

SALIVARY GLAND CHROMOSOMES AND GENES

(Abstract)

REV. CHARLES A. BERGER, S.J.

The salivary gland of Diptera is a larval organ present when the larva hatches from the egg and absorbed during pupation. Throughout larval life there is no increase in the number of cells forming the gland, i.e. no cell division, but there is a continuous increase in size of the cells which become enormous in the late larval stage.

The chromosomes in these cells are from one hundred to one hundred and fifty times the length of ordinary metaphase chromosomes. In aceto-carmine preparations these huge chromosomes show a linear differentiation into a large number of bands, segments, vesicles and circles of dots which are constant in structure and arrangement from cell to cell and from individual to individual of the same species. In *Drosophila melanogaster* three thousand five

hundred and forty 'bands' have been counted in one set of chromosomes. Estimates from genetical grounds of the number of genes in this species range from three thousand to five thousand.

Stocks of *Drosophila melanogaster* having chromosome abnormalities, translocations, inversions, deletions, etc., show corresponding changes in the visible lineal differentiation of the salivary chromosomes, affording visible cytological proof of many genetical phenomena which hitherto had been supported mainly by the indirect evidence of genetical behavior. At present it appears highly probable that, 1. the bands or segments represent 'gene loci', 2. that the bands are not genes but chromatic material covering the genes and secreted or otherwise produced by them.

The salivary gland chromosome is a new and most useful tool for the geneticist and furnishes the cytologist with a material of great possibilities for the study of chromosome structure.

BLOOD-GROUPINGS AND SEROLOGIC TESTS IN LEGAL MEDICINE

(Abstract)

REV. CLARENCE E. SHAFFREY, S.J.

History of earliest work in blood-transfusion. Discovery of agglutinogens and agglutins by Landsteiner and Shattock. Classes of blood-groups, Landsteiner's, Janskip's, Moss's. Universal Principles deduced. Isoagglutination explained. Determination of individual group. Technique of typing. Sources of error: pseudoagglutination due to rouleau formation, auto-agglutination, sub-groups.

Inheritance of agglutinogens. Follows Mendelian laws. Explanation of Von Dungem and Herszfeld: four characters united in two allelomorphic pairs. Bernstein's explanation: three multiple allelomorphs. Bauer's explanation of two pairs of partially linked factors. Rejection of Bauer's assumption by Snyder. Heredity of agglutinogens, M & N. Inheritance of Sub-Groups.

Employment of blood groups, sub-groups, and agglutinogens, M & N in determination of non-paternity, identification of babies, etc.

THE ROLE AND FATE OF PROTEINS

(Abstract)

John J. O'Brien, S.J.

Proteins are necessary for synthesizing tissue. Animals are incapable of synthesizing proteins and consequently must derive them from their diet. All proteins are composed of the same fundamental units—amino acids, but we cannot say definitely how they are joined together.

In the stomach, proteins under the influence of the enzyme pepsin are broken down into proteoses and peptones. In the small intestine the pancreatic enzymes trypsin and the enzyme of the intestinal wall, erepsin, break the protein particles down into amino acids. The amino acids are absorbed thru the intestinal wall; some are resynthesized into tissue protein and the excess deaminated and then oxidized or transformed into glucose.

CHEMISTRY

JESUIT TRAINING IN CHEMISTRY

(Abstract)

REV. JOSEPH J. SULLIVAN, S.J.

Inasmuch as modern chemistry is essentially physical and mathematical, the necessity of both physics and mathematics in the training of Jesuits who wish to become chemists was stressed in this paper. In our houses of study it is essential, first, that our young students become familiar with the main branches of study to which they are assigned in those houses. In the house of philosophy therefore, philosophy is paramount. After, and only after, this schedule is fulfilled should a student turn his attention to any other subject which must, of course, be considered secondary to the main subject at hand.

Therefore, it was suggested that our young students be directed, during any time which supervenes, along the lines of physics, mathematics and the fundamentals of chemistry. Let the higher branches of chemistry wait on time of special study.

MICRO ANALYTICAL METHODS

(Abstract)

REV. RICHARD B. SCHMITT, S.J.

INTRODUCTION.—Reasons for micro analytical methods: Accuracy; economical: time, labor and reagents; observations are more direct, vivid and positive; useful: when only small traces are present; some properties cannot be had by any other means; investigation of dangerous substances: explosives; when only small quantities are available: hormones, vitamins, secretions of endocrine glands; tool for research in new syntheses; application in bio-chemistry and medical research when only small quantities are available; applications in industrial chemistry; pedagogically: a training in exact analytical methods superior to macro methods.

MICRO INORGANIC ANALYSIS.—Detection of the elements; detection of the anions. Technique with the microscope: many physical properties: behavior of substances in polarized light, refraction, optical uni-axial and bi-axial properties, index of refraction and characteristic features of crystal systems.

MICRO INORGANIC QUANTITATIVE ANALYSIS.—The sensitivity of the Kuhlmann balance.—Determination of the elements and radicals.—Electrolytical determinations.—Volumetric analysis.

MICRO ORGANIC QUANTITATIVE ANALYSIS.—At present the development is quite complete. Determination of: Carbon, hydrogen, nitrogen (four methods), halogens (three methods), sulphur, phosphorus, arsenic, metals, methoxyl, ethoxyl, acetyl, carboxyl, molecular weights.

MICRO METHODS successfully used in: Bell Telephone Research Laboratories, General Electric Research Laboratories, Rockefeller Institute for Medical Research, Coal Research Laboratories, Merck Research Laboratories and many Universities.

MICRO DETERMINATION OF GAS ANALYSIS.—Methods have been determined for: Carbon dioxide, hydrogen chloride, water-vapor, acetylene, oxygen, acetaldehyde, ethylene and other unsaturated hydrocarbons, carbon monoxide, hydrogen, methane, ethane and nitrogen. The reagents used for these determinations were also given.

MICRO DETERMINATION OF HALOGENS.—A complete description of the newest method of halogen determination was given according to Zacherl and Krainick. This method was developed at the University of Graz, Austria, and brought to the United States last year. The apparatus was displayed. The list of reagents given; and the results of actual determinations.

The lecture was illustrated by lantern slides.

IDENTIFICATION OF ORGANIC ACIDS

JOSEPH A. MARTUS, S.J.

In a continuation of the work on the preparation of derivatives of organic acids, Mr. Kelly and his students here at Holy Cross during the past year used a new reagent, p-brom benzyl bromide. Its use was prompted by former experience with a bromine atom in the compound as an aid to crystallinity. As each new reagent for the preparation of derivatives appeared, investigators applied it to fill in the loop holes left by its predecessor; these loop holes they found to be either an uncrystallisable oil or a solid with a low melting-point. At the outset of the investigation using p-brom benzyl bromide as a reagent, it was impossible to state exactly what results could be expected, but it was hoped that, at least, this reagent would give a good series of derivatives, since, as E. Emmet Reid says, "It is desirable to have several series of derivatives, so that if one fails in a particular case another may be available."

This paper will treat of the preparation of this new reagent, the preparation of the esters of several organic acids, and conclude with a discussion of experimental results.

Preparation of the reagent.—Starting with p-toluidine, p-brom toluene is the first compound to be prepared and this is done according to the Sandemeyer reaction. The equations are as follows:

$$CH_3 . C_6H_4 . NH_2 + HNO_2 + H_2SO_4 = p\text{-}CH_3 . C_6H_4N_2 SO_4H + 2H_2O$$

$$p\text{-}CH_3 . C_6H_4N_2SO_4H + HBr = p\text{-}CH_3 . C_6 H_4 . Br. + N_2 + H_2SO_4.$$

P-brom toluene is then brominated directly in the sunlight, giving p-brom benzyl bromide,

$$p\text{-}CH_3 . C_6H_4 . Br + Br_2 = p\text{-}BrCH_2 . C_6H_4 . Br.$$

Preparation of the ester.—Reid's method was used here, where the essential reaction consists in the interaction of the reagent with the sodium salt of the acid, or the di-sodium salt as the case may be, in a 63% alcohol solution, giving the ester and NaBr. The reaction is as follows:

$$Br . C_6H_4 . CH_2Br + NaOOCR = Br . C_6H_4 . CH_2 . OOCR + NaBr.$$

With monobasic acids the procedure is as follows—to slightly more than .005 mol of acid slightly less than an equivalent of Na_2CO_3 was added. Five cc of distilled water were added to this and the mixture refluxed on a steam bath until all the acid was dissolved. Upon attaining a clear solution, .005 mol, 1.25 gm, of the reagent was added and enough alcohol to bring the solution up to 63%; in most cases only 10 cc was required. This mixture was refluxed for one hour. With dibasic acids the mixture of sodium salt and reagent had to be refluxed for two hours. The resulting solution was cooled rapidly, and, in most cases, a freezing mixture was required to bring the ester out of solution. Usually the compound had to be recrystallised four times before obtaining a constant melting point.

Discussion of experimental results.—In one case the acids used were all derivatives of benzoic acid. Twenty-seven acids of this type were used, of which only two gave oils on esterification. The seventeen acids listed here are the acids the esters of which the student had time to analyze. While the melting-points are not as high as esters from, say, p-phenyl phenacyl bromide, nevertheless there are only two below 50°, while the rest are moderately high.

Thirteen dibasic acids were esterified in this manner, but the success was not quite so satisfactory. Either the esters were oils or else they were insoluble substances which rendered them difficult to purify. Only the esters of six dibasic acids survived for analysis. Upon subsequent analysis for Br content, two more esters had to be discarded due to the large discrepancy between the calculated percent of Br present and the percent found.

A VISUAL METHOD OF SOLVING CHEMICAL PROBLEMS

(Abstract)

LEO J. GUAY, S.J.

In complicated problems involving many operations of multiplication and division, time and labor can be saved, if, instead of performing each operation separately, we express them in one general solution in which all the operations are performed simultaneously. In forming an expression of a complete solution, however, it is often difficult to preserve a clear understanding of the units involved at each stage of development, unless these units can be tabulated and visualized in the general solution. The visual method of solving problems, which Rev. Joseph J. Sullivan, S.J. proposed to the students of chemistry at the Holy Cross Summer School, is a method of expressing a complete solution, in which the successive stages of development and the units involved are tabulated and visualized, though all the operations are performed simultaneously. This method was demonstrated by means of solving a few chemical problems of various types.

STATISTICAL STUDIES ON CHEMICAL ABSTRACTS

(Abstract)

ALBERT F. MCGUINN, S.J.

Statistics on the percentage of page space in this journal devoted to the various sections were tabulated. Attention was drawn to the fact that organic, bioligical and physical chemistry far exceded any of the other sections. It was suggested that the order of prominence of sections on this basis gives an indication of the prominence of the various fields of chemical research. The desirability of incorporating more organic and biological chemistry in a one year pandemic chemistry course was stressed. The figures showed clearly that over a period of twenty years, the relative percentages of organic chemistry and biological chemistry have not been lowered by the recent emphasis on physical chemistry, and reasons for this were discussed.

MATHEMATICS

ISOMORPHISM

(Abstract)

REV. FREDERICK W. SOHON, S.J.

In a previous address as chairman of this section (BULLETIN, Vol. VIII, No. 3, March 1931, pp. 36-41) I explained the nature of the undefined terms and of the unproved propositions that are so much talked about by mathematicians—that the undefined terms were not wholly undefined for their range of signification is limited to those entities for which the unproved propositions are valid. For example, Huntington says: "We have called them 'postulates' from the Latin *postulo*, because they are 'demands' or conditions which a given system may or may not happen to satisfy . . . Just as any man who satisfies the conditions set up for admission to the army is entitled to belong to that particular class of men." Thus the postulates, or unproved propositions define and limit the undefined terms and restrict their generality.

Now we are here speaking of a class of entities having certain inter-relations which are clearly set forth in a set of postulates. It is usually if not always the case that more than one example, that is, more than one concrete interpretation can be given to the undefined terms, and that for both, or all of these concrete interpretations, all of the postulates will be valid. The mathematician is not completely stopped at this point. He can go further and compare two possible concrete interpretations of his symbols in parallel columns. He may find that although both interpretations completely satisfy the postulates, they cannot be paired off or be brought into one to one correspondence. In this case, the mathematician can still distinguish between the two interpretations, and this distinction will fall within the realm of his mathematics. But if, on the other hand, two concrete interpretations can be paired off or brought into one to one correspondence, the two systems are said to be isomorphic and the distinction between them lies outside the realm of his mathematics. It he has no other data than his postulates and their consequences, he is not entitled to, and logically he cannot distinguish between them.

The first thing that naturally suggests itself is that the undefined terms or symbols stand for universals, and this is of course quite true. But it may not be fully realized that these universals are very liable to be of a very peculiar nature. By that I mean that whereas ordinary universals are expressed by common nouns, it will not be unusual in the present case to find that the English language possesses no word which ordinarily takes on the precise range of signification admitted by the postulate system. For instance, at the expense of giving myself away, we have no word which means both a wave and a particle. The defect that I speak of is not a defect of the mathematics, but a defect of language. Using words in their ordinary senses we simply cannot say what we mean. This may give the false impression that we do not know much about the subject. But my contention is that the defect is a defect of language and not a defect of the analysis, and if it prevents the uninitiated from reaping the fruits of our labors, that is merely unfortunate.

Now, of course, we may coin new words, but this is not always a good thing to do, because we are very likely to find ourselves called upon to construct a whole new language since the difficulty is apt to arise again and again. A more common practice is to use a term whose ordinary meaning suggests one of the possible interpretations—with the mental reservation that an alternative interpretation is possible. Thus in speaking of an abstract algebra of imaginary numbers, I must not forget that my imaginary number may be a very real point in a plane or an alternating current with a certain root mean square amplitude and phase angle. The interesting thing here is that as long as the interpretations are isomorphic I shall make no logical errors even if I do forget what I am talking about.

In a practical case, even knowing the interpretation of my symbols, I may carry through the analysis using the terminology of an isomorphic system if the latter terminology is richer than the terminology suggesting the interpretation actually desired. Thus a problem in algebra or in dynamics will often be analyzed as if it were a problem in space of n dimensions. For no logical errors can occur from confusing two simply isomorphic systems. They are mathematically indentical.

Entirely apart from the more or less obvious, if not trivial, difficulties which I have pointed out above, isomorphism is a much more serious snare for the person who is steeped in deductive reasoning. Such persons are usually aware in an abstract sort of way that the physical sciences employ at times inductive reasoning, but they never seem to realize that the physical sciences are inductive and empirical in their very nature. For example, I was informed recently by a bright young man, that in inductive reasoning the formation of an hypothesis must precede the formation of a law. Naturally I at once became very curious to know some hypothesis underlying the law of universal gravitation, and wondered if the young man knew that the only hypothesis worthy of the name ever proposed for the law was Einstein's General Theory of Relativity. But the young man was clearly at sea—and I never punish a student for the sins of his professor. The incident clearly shows that the philosopher thinks that the scientific process consists of conjecture, deduction and verification. Consider what the possibility of isomorphism means now. Each of two contrary hypotheses can be verified if the hypotheses are isomorphic. In this method of reasoning isomorphism is fatal, and we are back where we started from, so that science is reduced to conjecture which, even if verified, may be false. Such a philosophy does not allow empirical science to be possible.

The scientific procedure is rather different, although it does not overcome the difficulty of isomorphism. First, facts are observed. By that I mean, certain measurements are taken, measurements are the facts of exact science, and in general the observed fact can be expressed as an equation or as a relationship between measurements. Next, the inductive process is employed to discover whether the observed relation is a singular isolated fact, or whether it can be subsumed as a particular case of a universal relation. If that can be done, the universal relation so obtained is a physical law. Now it must be remembered that we are making measurements, but we are not at all sure of the nature of the things that we are measuring especially when these measurements are indirect. We have, let us assume, a number of physical laws that must be satisfied by any admissible hypothesis. So you can see that the physicist, uncertain of the precise nature of the things that he is measuring but with a certain number of physical laws with which his interpretation must be consistent, is in precisely the same position as the mathematician with his undefined terms and with his unproved propositions. Both are stopped by the problem of isomorphism. The mathematician escapes because the distinction is outside of his subject. Some physicists try to escape the same way and propose to turn the problem over to the philosophers as insoluble.

In concluding, however, I wish to make just one point in behalf of the physicist. He is stopped by the problem of isomorphism, but his science is saved, at least the inductive part of it, and only those deductive speculations that he calls hypotheses are left waving in the wind.

STATISTICAL LAWS AND CAUSALITY

(Abstract)

Rev. Joseph P. Kelly, S.J.

Classical Physics was dominated by a physical determinism. This attitude of Science is well expressed in these words of Laplace, "If an intelligence for one given instant, recognizes all the forces which animate nature, and the respective positions of the things which compose it . . . . nothing will be uncertain to it and the future as well as the past will be present to its vision." Underlying this concept was the more fundamental principle of natural causation. Accepting these two, viz: the principle of natural causation and that of physical determinism, the scientist was able to predict results not only in the laboratory but in the workings of nature outside of controlled experiments. The regular and uniform activity of natural, material bodies offered a firm foundation for the establishment of physical laws. Extensive, experimental confirmation for these postulates was found in the investigations of men of science.

Even from the early days of modern science, there was a growing tendency to follow the lead of Galileo, who asserted: "it does not seem to me advantageous to examine what the cause of acceleration is." However, scientists still retained the concept of a strict causal nexus between natural phenomena, although in their actual work they were content to describe operations rather than to seek the causes. Under the attacks of Hume, Locke, J. S. Mills and others, on the traditional philosophy, science substituted the notion of invariable sequence for the concept of a causal connection. The criterion for this invariability or causal nexus was the possibility of accurate prediction. This plan attained a high degree of success in Newtonian Physics, but when these physical laws were applied to atomic phenomena, it was soon discovered that accurate prediction failed. Considerable accuracy could be predicted with respect to groups of particles but not for the individuals. Hence, it was concluded that the laws of nature were statistical in character. The Kinetic Theory had well prepared the mind of the scientist for an easy acceptance of this conclusion. Then came the further development, viz: that the principle of causality was no longer valid in science.

The principle of natural causation in physical science was a methodic principle. It was a postulate, based on experience—though philosophy also had its influence on it— and was accepted as a working principle. It proved itself a valuable aid in carrying on experimental investigation. In later times the notion of causality became almost indentified with the possibility of prevision or at least, predictability became the criterion of judgment for the existence of a causal nexus between antecedent and subsequent events. When prediction failed, the scientist concluded that causality also failed.

Hence, when the scientist was unable to predict with accuracy the simultaneous position and velocity of a particle, his logical conclusion was that the principle of causality was no longer valid.

In philosophy, the principle of causality has an entirely different signification. It is an analytic principle; a principle of intelligibility, necessary with regard to contingent beings. All material beings are contingent, that is, they are not in themselves sufficient for their own production. They cannot bring themselves into being. Hence, when something happens, when something comes into existence, we must seek the reason of this "becoming" not in the being itself but in some cause which has produced it. In philosophy, causality does not pertain to the *prevision of an effect, but to the explanation of its coming into existence*. The contingency and insufficiency of a material being face to face with existence demands this principle of causality for an adequate understanding of its actual existence. Therefore, the scientific and philosophic concepts are quite different. They touch diverse aspects of being. Although consistent with his principles, the scientist denies the principle of causality, the philosophical and metaphysical principle remains altogether intact. Science cannot prove or disprove the metaphysical principle of causality. It is outside its field.

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METEOROLOGY

AIR MASS ANALYSIS

(Abstract)

LEO WELCH, S.J.

The term Air Mass is applied to a large body of air which acts as a unit and whose properties are horizontally homogeneous and fairly constant. The properties of the air mass, which are considered in this homogeneity, are mainly temperature and moisture content. Air masses are analyzed primarily according to the properties which they acquire in their source region and secondarily accordinging to the modifications which these properties undergo while the air mass travels through a new environment. The office of the forecaster is to determine what further will take place within the air mass itself and what will be the nature of its interactions with bordering air masses. Weather changes on a large scale are primarily found within the zone of interaction between two air masses with different properties.

PHYSICS

PRODUCTIVE SCHOLARSHIP

REV. EMERAN J. KOLKMEYER, S.J.

A very general standard, by which colleges, universities and especially their staffs are judged is the quantity and quality of published research. It may be a good standard if it is not made the sole standard. Unfortunately the educational world seems to be getting into the habit of neglecting all other criteria and sometimes or often enough seems to stress the quantity of published work, forgetting its quality. The question 'What have you published?' is coming to mean 'How much have you published?'

We have no complaint to make against research, or against research in the schools. In fact the schools should be not only the channels by which the old truths are made known to those who seek learning, but also the great sources of newly discovered truths. Nevertheless it is important at times to point out that the chief purpose of a college is not to uncover new truths: its first object is to present an ordered body of the old truths to its student, so that, enriched by this legacy of the past he may guide his thoughts and actions as befits a man, to think for himself, and perhaps to really think thoughts that are entirely new.

From this point of view it is gratifying in these days of quick and much printing to find not a few men who remember the chief purpose of their profession as teachers. They laud the work of those who are entirely devoted to undergraduate instruction, they seek to give a due and honorable standing to such of their colleagues who are prevented from reaching the journals. It is being recognized that a teacher may be excellent without a Ph.D. and that frequent printing is neither a necessary nor an all important qualification for his work.

For instance, there is the conclusion of the committee of mathematicians investigating the advisability of offering a new degree, or a greatly modified set of requirements for the old degree, for such students who are preparing for a college teaching position in mathematics and who are not seeking a career in mathematics. Realizing that less than 20% publish after their doctor's dissertation, that few have the ability to produce work worthy of the high standard of the mathematical publications, and that there are many others who would be excellent teachers but can not attain the present Ph.D., the committee was emphatic in its approval of a change; a change either in the degree or in the requirements for the degree for future mathematics teachers. One of the changed requirements would be the acceptance of a thesis no first rate mathematics journal would now take.

Again, in an invited address at the A. A. A. S. meeting at Pittsburgh last January, Dr. R. L. Jeffrey recognized the impossibility of high quality research in many good undergraduate colleges. In its place he declared for a kind of thinking that could, and should and must be found in a good staff of college professors. He called it Productive Scholarship. It will include all such advances in work, method and practice as would improve the course of instruction and the student body without attaining the proper dignity of research. The advances that are productive scholarship need not be in the nature of an increase in the world's knowledge; they might well be only personal improvements and so of no publication value at all.

An example might be the solution of an ordinary problem in teaching: The professor notices that a certain discussion does not pass over the lecture table to the minds of his students. It fails in several classes. Search for the cause of this failure of transmission and the discovery of the remedy would be a good piece of productive scholarship. So too would be new methods of solving old problems, the simplification of old methods, the correlation of parts or of the whole field in the light of the teacher's intellectual background. For the student there is honor's work, enthusiasm for the new inventions and new applications as they appear in the press, a critical study of his text and his technique. As is evident, productive scholarship, though below the publication standard of scientific journals, is still a worthy object for the progressive teacher; it is in fact a necessity. Essentially it is what Father Tondorf, years ago, said was the research we all could do and had to do: recognize and solve the problems in the work we were assigned to do. It is the thinking, the planning, the activity of the teacher not stuck in the mire of tradition. It is really not something new, except perhaps in name. May we not be excused for recalling the old, simple, fundamental practice of the active teacher, even under a new name, by the present over-insistent demands for research and the increasingly loud howls for publicity in our work.

It seems necessary to stress two points: first that it is not given to every one of our teachers of physics to do research work, and secondly that it is a misconception of the teacher's profession to think that he must make the press.

True research work is beyond most of us. A variety of reasons might be urged for such a statement, but the chief reason is that we are not trained for it. The day may come, and we all sincerely hope it will come soon, when there will be men of our province ready and able to take positions at the frontiers of the science of physics.

For the present we bear the honorable title of teachers only, having no extra training, no time, no equipment, no funds for research. Our assignments of men, places and labor are those of teachers, and teachers of undergraduates. Our duties lie with that part of the course of Jesuit education which links our present knowledge of nature with philosophy, providing besides, such instruction in physics as is demanded by the professional schools. Later there will be centers where graduate work and its attendant research in physics will be vigorously prosecuted, but for the present the bulk of our men in this science in this province are engaged in purely undergraduate teaching. For this we do not need to make the scientific journals.

Among us it is less necessary to stress the point of pushing publicity. But those who have attended meetings of the Catholic Round Table and other groups know how heavily this is hammered. We have no obligation to reach the journals, still less to make the newspapers. Publication, when it is not the routine report of new findings, is usually better called publicity and its object is advertising. Certainly we have no obligation of advertising ourselves or our departments, and I fail to see why we should be called upon to feel our consciences disturbed if we do not give the public, now and then, a few choice bits of undergraduate physics. When there is to be advertising, let it be that of the whole school, of the type of education we offer. Physics is only a part of the machinery we set up to produce in our students the culture or learning we wish to impart.

But I trust I am not leaving the impression that our physics teacher should hide his light under a bushel. This light of his thinking, of his productive scholarship is really needed. We ourselves and our teachers of philosophy would like to have dozens of questions answered, questions that are almost domestic. The proofs for the existence of those invisible bodies like the electron, the positron, the neutron, etc., etc., collected in a paper and presented in the Bulletin would be very acceptable to many of Ours.—and all the other questions we hear proposed in our recreation rooms. You will recall how good it was to read Father Sohon's set of definitions of terms common to philosophy and science. That was the response to a philosophy teacher's request.

Not one of us but has trouble with effective demonstration in the lecture room. When he has devised a new arrangement or a new instrument, it is an advance for all. Perhaps another has devised a lantern slide that is better than a complicated instrumental exposition. Perhaps the whole method of presentation of an article or a chapter should be changed.

In the laboratory the problems are as numerous and as important. Take a very common difficulty: how many experiments do we present to our students that are so cluttered up with incidentals that the student loses sight of the main purpose of his assignment?

It is this kind of work and of thinking that must find its way into the habits of our students—and we must put it there. I believe we criticise both ourselves and our methods very justly when we say we do too much for the student. The activity left for him is passivity, that of the rock receiving the form of a tombstone. He leaves us with an epitaph carved upon him. Such criticism is often applied to the courses in which a set of boiled down notes is fed to the memories of the class. But do we not often demand just that very same type of work in our laboratories? Far too frequently the college physics laboratory experiment is a vicious compound of an hour or two with a new and somewhat complicated toy, and a so called report that in turn is mixture of a copied description, pages of meaningless numbers, blind and stupid but hopeful substitution of some of these numbers in a formula and the happy acquisition of an answer. It is called a scientific report of the experimental verification of a more or less fundamental law of nature. The law was already known to him, and his experiment added nothing to his knowledge or training.

Instead the student should have learned to save time with the simple tool of logarithms. He should have compared his result with accepted values, made a numerical measure of his accuracy, but one that he understands. Above all he should have learned to explain clearly, not vaguely, both the reason for his deviation and the troubles that affected his work, troubles arising from the nature of the apparatus, the temperature and outside influences, and particularly from the method of his experiment. From it he should learn the difficulty of reaching broad truths by experiment, something of the scientific method, the reliability of experimental evidence, and something of the amount of thought that must go into a reliable conclusion. A poor numerical result on a student's paper is not nearly as bad as a report that shows no thought in evaluating results, however carefully written, however carefully arranged. An experiment with only a numerical answer from a college student is a disastrous disgrace. One that satisfies some sort of a deviation measure or a probable error is little better. One that lists only vague routine general sources of error is not much of an advance. But two such reports accepted by the department constitute a fine anaesthetic for the rest of the year in the laboratory. All real independent thought ceases right there in October.

By insistence on an analysis of the experiment and by proper grading we can soon make the student see that we appreciate more his thinking than his hours of labor, that we value more his ability to understand, to judge, to evaluate and to discriminate than we do a glib memory of facts or a straight column of data. Soon he will learn to devise his own little time savers, tricks of more direct action, inventions, possibly, of better, simpler methods.

True it is, that for such work we need instructors who are themselves alive, who are themselves forever producing, not research, but something in their own line of work. If we have a steady plodder who will set out apparatus, follow a schedule carefully and check reports without fail, we have something but not all that is necessary for a teacher. Under such a man apparatus fails and is not repaired, experiments do not improve, the students begin to wander about in a maze of screws, clamps, meters and metal, words and numbers. Quizes become dry substitutions in formulae, memory recitations of laws. When we find nothing beyond routine and repetition in the work of our instructors and no evidence of what is called productive scholarship, they are a dead loss. It is unfair to judge them and ourselves on research, it is not unfair to judge their value on productive scholarship.

SOME RECENTLY COMPLETED LABORATORY PROJECTS AT CANISIUS COLLEGE

(Abstract)

Rev. John P. Delaney, S.J.

A very satisfactory X-ray diffraction camera and X-ray spectrograph, the first in any of our eastern Jesuit college laboratories, was constructed by the writer last year at Canisius College. The difficulty of demonstrating the diffraction of X-rays is well known. For seventeen years after the discovery of X-rays, by Roentgen, the exact nature of these radiations and their wave-lengths baffled the ingenuity of scientists. Finally in 1912, Prof. Max von Laue of the University of Munich with his first X-ray crystal diffraction patterns not only solved the mystery of X-rays, but also measured their wave-lengths and opened up to science the immense new field of atomic arrangement and space lattice in crystals.

The new X-ray camera at Canisius College beautifully reproduces the Laue spot patterns, some of which we hope to submit for publication, together with a diagram of the camera, in a forthcoming issue of the Science Bulletin. The value of the camera from the pedagogical viewpoint can not be overestimated in view of the importance of Laue's contribution to modern science and in view also of the importance of Laue's contribution to modern science and in view also of the great interest of our students in this important field of modern physics.

A second project recently completed by the writer is the 41-inch concave grating Rowland spectrograph. The circular steel tracks that guide the film precisely in focus are accurately turned to within a thousandth of an inch. They were donated by the Worthington Pump Corporation, a handsome appreciation for some work of the writer in introducing their engineers to the photoelastic method of testing models of steel castings. The new spectrograph yields beautiful spectrograms on panchromatic roll film 2 inches wide by 36 inches long. These grams, as well as the Laue spot patterns and the X-ray spectrograms, supply a wealth of material for study and measurement by ambitious advanced students.

Another helpful improvement of the last year at Canisius has been the substitution of 110 volt neon glow lamps in place of sodium burners for all spectrometric and optical wave-length experiments. The yellow neon line has proved quite as satisfactory as the sodium line for these experiments, and there is the added advantage of other brilliant lines to work on if desired and the elimination of the smoke and fire hazard of the sodium burner.

A RELATION BETWEEN VOLTAGE AND WAVE LENGTH

(Abstract)

REV. JOHN A. TOBIN, S.J.

This paper presented an answer to the question: What is the meaning of the statement, that an electron has a velocity of 4 volts or that the voltage was 3000 Angstrom units? The relation between voltage and wave length comes from the inconsistency of measuring the energy in the corpuscle of light in terms of Plank's constant and the frequency. The frequency is measured by the velocity of light divided by the wave length and the wave length is measured by a diffraction grating, which can only be interpreted in terms of the wave theory.

When the quantum of light hits an atom it completely gives up its energy to the atom and this energy is used in expelling the electron. Three cases were discussed in the paper when a given metal plate is placed in an evacuated quartz bulb and the plate is illuminated. (1) If the intensity and wave length of the light remains constant, and we vary the potential of the other plate in the bulb, we find that the current flows even against a negative potential to a certain point. When the current is zero, that definite negative potential is the stopping potential. (2) If we keep the intensity constant and vary the frequency of the light, we find the velocity of ejection increases with the increase of frequency. If we plot the stopping potential against the frequency, we find a threshold frequency, or point, where, if the frequency is decreased, the photoelectric effect disappears. Below this frequency no electrons are expelled. (3) If we keep the frequency constant and vary the intensity of the light, we find no change in velocity of ejection but the rate is increased or the

current is increased. The energy of the fastest electron is measured by the voltage V and the charge e, and also by ½ mv^2. From this equation we find how the velocity of ejection may be expressed in volts.

$$V^2 = 2\frac{e}{m}V = \frac{2\times 5.31\times 10^{17}}{300}\times V_p \quad , \quad \text{as } V = \frac{V_p}{300}$$

$$V = 5.94\times 10^7\times\sqrt{V_p}$$

e. g. A velocity of 4 volts is 1.19 x 10^8cm. per sec. If we suppose the threshold frequency is 0 we may write Ve=hn and we find how the voltage may be expressed in angstrom units.

$$V_e = h\frac{c}{\lambda} \ ; \ \lambda = \frac{hc}{Ve} = \frac{6.55\times 10^{-27}\times 3\times 10^{10}}{V\times 1.59\times 10^{-20}}$$

As λ in Angstrom units = 10^{-8}cm. and 1 volt = 10^{8} e. m. u.

$$\lambda = \frac{6.55\times 10^3}{1.59\ V} = \frac{12345}{V}$$

where 5 is uncertain.

The paper then gave applications as the Duane Hunt equation for X Rays, the Bohr Theory of energy levels, and the determination of the minimum voltage for gamma rays, etc.

THE RUTHERFORD—BOHR NUCLEAR ATOM

(Abstract)

JAMES J. DEVLIN, S.J.

Using alpha particles to bombard the atom Rutherford by means of the scattering formula $\frac{dn}{n} = \frac{4\Pi s N_e^4 Z^2 \cos\frac{\theta}{2}}{v_0^4 m_a^2 \sin^3\frac{\theta}{2}}d\theta$ concluded that the atom is made up of a positive nucleus at the center of the atom with electrons outside the nucleus. The inertial mass of the atom is concentrated in the nucleus and the number of positive charges corresponds to the atomic number of the atom. Bohr by his two postulates escapes the dilemma presented by classical physics and by means of the formula $\lambda = R\left[\frac{1}{n_f^2} - \frac{1}{n_i^2}\right]$ explains the Balmer, Paschen, and Lyman Series.

THE PERIODIC TABLE AND PAULI'S PRINCIPLE

(Abstract)

GERALD F. HUTCHINSON, S.J.

Balmer derived the following equation for the wave numbers of the hydrogen spectral lines.

$$\nu = \frac{R}{2^2} - \frac{R}{n^2}$$

Rydberg extended this equation to cover all atoms, by correcting for the screening effect of the electrons between the valence electrons and the nucleus.

The energy levels may be stated in terms of quantum numbers. When this is done, four quantum numbers are used, namely, N, L, Ml and Ms. N may have unit values, L has the values $\leqq$ N-1, Ml=2(2l+1), Ms= + or - ½. Pauli's principle then states, that no two electrons in the same atom, at the same time, may have the same set of four quantum numbers. It may be stated in an equation, giving the number of electrons in any quantum state.

$$N_n = \sum_{0}^{n-1} 2(2l+1) = 2n^2.$$

The application of these principles were then shown in the periodic chart. The reason for the irregularities still existing may be found in the energies of the individual configurations.

THE NUCLEUS

(Abstract)

THEODORE A. ZEGERS, S.J.

A paper on the nucleus covered briefly, four factors entering into the theory of nuclear physics. These were:

1) The periodic chart with all the information it includes and complete tables of isotopes.

2) The principle of conservation of momentum.

3) The principle of conservation of energy, together with the relativistic equivalence of mass and energy. The units used were; IMV (million electron volts E.S.U.) $=1.59\times10^{-6}$ ergs; $=1.07\times10^{-3}$ mu. (mass units on the basis of 0=16.)

4) The principle of conservation of charge.

Then a few typical equations of nuclear disintegration were given, and a summing up of the particles that have been found.

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