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EDITORS OF THE BULLETIN

THE EDITOR'S PAGE

HIGH SCHOOL WAR COURSES

The "All Out For War" spirit of the day has taken an enthusiastic grip on our colleges and transformed their schedules so that we hardly recognize our own schools. In a war which demands so many highly trained men it is inevitable that war education should start back in the high school. The start should be made as soon as possible, and most high schools, including our own, have been trying to arrange the most efficient schedule possible for the present needs. In peace time there is little specialization in high school because all educators recognize certain fundamental courses as the ground work of all future education. Thus a high school teacher and especially a science teacher may well be puzzled as to what sacrifices of the normal high school course should be made, or how additional courses can be fitted into an already busy schedule. This confusion is not lessened by the flood of text books and high pressure salesmanship that inevitably accompanies the opening of a new field. In this uncertainty we who have the responsibility both to the boys in high school and also to our country, are fortunate to have a direct communication from the War Department, signed by the Assistant Secretaries of the Army and the Navy, telling us just what it expects. These demands are contained under five headings. 1) Physical and mental qualifications. 2) Mathematics and physics. 3) Pre-flight Aeronautics. 4) Physical Training. 5) Military Drill. The first of these is merely directive in selecting proper boys for war courses. The last two are mentioned as "advantages" and cannot be expected of all schools. Most, but not all, of our schools will be unable to supply both of them. The other two requirements can be taken care of without seriously disturbing our regular curriculum. Most teachers will agree that the most important of these is a sound grounding in fundamental mathematics. From the various manuals and notices circulated by the war department the conclusion is emphasized that the facility of aviation and other students in algebra, geometry and trigonometry is not satisfactory. These subjects and an elementary course in Aeronautics can be emphasized in our high schools without sacrificing anything that is fundamental in our early training. By doing this we are fulfilling the part asked of us by our Government and at the same time preparing our boys in the best possible way for their whole life as fighters and as citizens after the war.

One of the things which puzzles a new-comer in the field of high school science teaching is the matter of definition. Science is supposed

to teach its students clarity of thought and accuracy of expression of this thought. Yet many definitions, which are supposed to be the very crystallization of accurate expression, are either evasively inaccurate or what we are later taught is definitely wrong. For instance, a saturated solution is frequently defined in terms which mean a solution which has all of the solute it can hold. Sometimes saving terms are added, which are difficult for the student to understand and which really cause more confusion than the true definition would cause. If the idea of equilibrium is too advanced for students at the time they learn about solutions, can the idea not be paraphrased in a way that they can understand it. Thus for example, a saturated solution is one which has such a quantity of solute in solution that, even if more solute is present none will dissolve and none will precipitate from the solution. Such an idea can readily be transfered to the scientific terms of equilibrium later. If such is not feasible, would it be better to omit such definitions entirely until a correct and accurate definition can be understood?

CONSIDERATIONS ON THE "OLD" AND SOME "NEW" THEORIES OF PROBABILITY

By Rev. Edward C. Phillips

"Probability" has various meanings; e.g., it may designate a quality of a judgment or opinion, or a vague surmise that something will happen. We may call the object of these and similar meanings "subjective probability". There is also a mathematical or scientific meaning relating to what we may designate as "objective probability", as when we say that the probability of a coin falling head-up is one half. This mathematical probability forms the subject matter of this paper.

All of us, no doubt, are familiar with as much of the theory of mathematical probability as is contained in a section of the chapter of Alegbra on permutations and combinations, or, perhaps, in a separate chapter devoted to Choice and Chance. We could readily answer many questions about games of dice or about the probability of drawing at random a white ball out of a bag containing a given number of white, black and red balls, etc. If asked for a mathematical definition, we would, I suppose, give the classical one ascribed to Laplace which states that "Probability is the ratio of the number of favorable cases to the total number of equally likely cases".

This definition and the rules derived from it for determining the mathematical probability for various practical cases, is quite sufficient and correct within a limited field of problems. However, it suffers from a logical defect, since it seems to define the term "probability" by another phrase which means the same thing: that is, "equally likely cases" seems to be tautological for "equally probable cases". Some authors go so far as to say that probability is such an elementary idea as to defy definition. This is not a satisfactory position to take, and if we are to claim for the theory of probability the standing of a science, as, for example, we claim for the theory of Hydrodynamics in Physics, we should be able to give strict definitions of the terms used as the basis of the science and in its systematic development.

Granting, however, that the definition given above can serve as a correct one, there remains another defect, namely, that this "classical" theory is too limited in its application and does not include many of the practical problems of vital interest to us. It did very well for the older problems of games of chance to which it was applied with great acumen by many writers during the 18th and 19th centuries; it does not apply to many of the problems of life insurance, unless we throw off the strict limitations involved in its definition. And even in games of chance, the strict classical theory breaks down when we deal with abnormal cases: for example, in throwing a normal die the

chances of throwing a three is one sixth, because there are six different faces to the cube and there is equal likelihood of any one of the faces turning upwards: but suppose the die is biased (intentionally or accidentally)—where then are the *"equally likely"* events?

Dissatisfaction with the classical theory has led many modern writers to seek a logical and wider theory to replace it. I will here consider the views of one such writer, the well-known author on probability and statistics, Richard von Mises, Professor of the University of Istanbul. His theory was first proposed in 1919; during the intervening years he has developed and also defended it on scientific grounds in a number of works, one of which I would recommend to your reading and study, even though we cannot agree with his conclusions. I refer to his Lectures entitled *"Probability, Statistics and Truth"*, first published in 1928; a later revised edition was translated into English by J. Neyman, D. Sholl and E. Rabinowitsch, and published in 1939 by The Macmillan Company.

This theory is based on a new definition and developed from a strictly *a posteriori* viewpoint instead of the older *a priori* one; it has the advantage of being applicable to a very much wider field of events, and also escapes the danger of falling into the tautology charged against the classical definition and theory. Von Mises defines probability as the limiting value of the relative frequency of a given event in an indefinitely long series of associated events. If asked how we know what that limit is, or even if in a given field of events such a limit exists, he would reply that we know it only from experience (of ourselves or others) and that therefore probability is an empirical property of events or classes of events. This view does not *necessarily* imply that there are no definite natural laws predetermining the course of "probable" events; just as our dependence on experience for our knowledge that stones fall to the earth and with a definite acceleration does not imply the denial of deterministic natural causes. Von Mises, however, does seem to hold that we cannot speak of the deterministic happening of such events: for him everything subject to treatment by the theory or calculations of probability is the result of purely "statistical laws"; he even goes so far as to exclude the principle of causality in this field—or at least that is the only way in which I can understand certain statements of his, as, for example, the following concluding paragraph of his book:

> "The opinion that statistical theories are temporary explanations as compared with the final deterministic ones, which alone satisfy the craving of our mind for causality, is nothing but prejudice. Historically it is understandable; but it is doomed to disappear with the growth of knowledge."

I will return briefly to this philosophical aspect of his work towards the end of this paper.

According to von Mises' definition, probability pertains then only to certain classes or certain collections of events; before we can determine probabilities we must have a series of repetitive events of indefinite extent; such a series he calls a "collective"; for example, to determine the probability of a six turning up in the game of throwing a die, we must consider an indefinitely large number of throws, and note as we go along which throws result in a six and which result in some other number; we then take the ratio of the number of times six appears to the total number of throws so far made; if this ratio approaches a definite limit as the number of throws is indefinitely increased, this limit is the probability of a six appearing. Experience shows in fact that $1/6$ is the limit of the ratio when an unbiased or a homogeneous die is thrown repeatedly; if the die is biased the ratio, and therefore the probability of throwing a six will be something else; it might be $1/2$ or it might be $1/20$, or some other fraction, according to the way the die is biased in favor of or against casting a six. From this example we can see a first advantage of this theory; it is serviceable even though there is no *a priori* way of knowing what is the probability of casting a six, or any other specified number, with a biased die—and in fact von Mises seems to say that there is no *a priori* way of knowing what is the probability of casting a six even in the case of an unbiased die—if such a die exists.

A further quality however of the series or collective is necessary to assure its being a proper object of probability calculations; beside offering a definite ratio for the relative frequency of each of the several attributes of a collective (and there must be at least two attributes) it is necessary that this ratio remain unchanged by any arbitrary selection of a sub-series out of the total series; and this quality he calls its *randomness*. Thus in the case of tossing a coin, the two attributes are "heads" and "tails". If we toss an ordinary coin we get a series of events each of which is a "head" of a "tail", and we find from experience that as the series progresses indefinitely, the ratio of "heads" to the total number of tosses approaches one-half; but suppose that we were to take only every third toss, would the ratio still remain $1/2$? If it does not, then the collective does not possess randomness and the varying ratio is not a probability ratio. An example of a non-random series is given by von Mises, as follows: (cf. p. 31)

> "Imagine, for instance, a road with stones placed on the footpath to indicate distances, large slabs for whole kilometres and smaller ones for tenths of a kilometre. If we walk long enough along this road, noting the stones and calculating the relative frequency of large and small slabs, the value found in this way will lie around 1:1o. . . . The deviations from the value 0.1 will become smaller and smaller with the increase in the number of stones passed; in other words the relative frequency tends towards the limiting value 0.1. . . .

[40]

"This sequence is quite different from that of odd and even numbers shown by a long series of casting a die. One is "regular" (the stones) and the other is completely lawless. With the milestones if we start our observations (counting) with a large stone and then take every other succeeding one, we will find that the relative frequency of large stones is now $1/5$ instead of $1/10$. By other regular selections of stones we could get other values for the relative frequency—this however is not so with the casts of a die; no matter what regular selection we make of the elements the relative frequency remains unaltered.

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X O O O O O O O O O O X O O O O O O O O O O
X O O O O O O O O O O X O O O O O O O O O O  X;
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(the ratio is $1/10$)

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X O O O O O X O O O O O X O O O O O X O O O O  X;
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(the ratio is $1/5$)

There are random series in which the ratio or relative frequency of one attribute approaches zero as its limit: we say that the probability of this attribute (or of such an event occurring) is zero; the probability of the alternative event is then unity. This points to another characteristic difference between the classical theory and the new theory. In the strict classical theory a probability of zero means that the event is impossible and cannot occur; whilst unity means that the event is certain and cannot fail to occur; in the new theory, however, a probability of zero means that the occurrence of such an event is relatively very rare; a probability of unity means the event occurs, relatively speaking, almost always: this difference is founded in the terms themselves of the two definitions: the old definition says that probability is *the ratio* of the number of favorable events to the total number of equally likely events; and the new definition says that the probability is *the limit to which this ratio,* i.e., the relative frequency, approaches as the number of events increases indefinitely. One is a static definition, the other a dynamic one. And yet in the old theory we did not hesitate to apply the calculations to infinite series also, and hence to consider the limit of the varying ratio instead of a determinate and static ratio implied by the classical definition. For example, in Philosophy we state that the probability of an orderly and self-perpetuating complex cosmos (world) arising through chance is zero; and yet among all the possible results of a random interaction of vast collocations of matter there are infinitely many "favorable cases"; to get an actual ratio of two numbers, both numbers must be finite, which is not the case for possible orderly worlds: however the greater the number of possible collocations of matter we consider or select at random, the closer will the ratio of "ordered" collocations to the totality of collocations approach zero as its limit. Hence the new theory appears to respond more closely than the older to our actual mode of argument in the proof for an intelligent Creator.

If we are to have an art as well as a science of probability, it is necessary that in addition to a defensible definition, we must build up a complete or at least an extensive manual of operations which allow us to solve the various practical problems of human experience and even of human speculation. The old theory did this to a fair extent, and we remember that there are rules for the addition of probabilities in some problems, and multiplication in other problems, etc. Von Mises, starting with his definition, builds up the theory in a similar way; and it would seem that he does so in a simpler and more complete or at least a more consistent way than the classical theory. There are only four fundamental operations with collectives which are required in his theory, just as there are the four fundamental operations of addition, subtraction, multiplication and division in arithmetic; from these four operations, all the others, even very complex ones, are deduced by the proper combinations and sequences.

Let us now consider briefly the philosophical aspects of probability and von Mises' reactions to or opinions on them. The author does not despise the philosophers, but seems to look on them as impractical or even as extreme subjectivists inasmuch as they seem to him to state that the assertion "I *presume* that these cases are equally probable" is equivalent to the proposition "These cases *are* equally probable". This judgment of the subjectivists (but certainly not of *all* philosophers) seems to be justified by the statements of some writers, such as the following: "the probability which we ascribe to a certain event, or, more correctly to our assertion of its impending occurrence, depends on the degree of our knowledge; the assumption of equal chances for several events is the result of our absolute lack of knowledge" (p. 110-111)! I do not think that any "Philosopher" worthy of the name ever expressed such a view, rather they would say that where the causes producing certain events present a *prima facie* indication of being able to produce a definite number of different results and there appears no reason to make us believe that these causes favor one event rather than another, we have no valid reason for not considering all of the events equally likely, i.e., equally probable, and hence we conclude that in the long run each of these events will occur as often as the others. This seems to me to be an *a priori* presentation of probability as the expected relative frequency of the occurrence of a given event in a given long series of trials or observations: whereas von Mises will arrive at the same probability by the *a posteriori* method in which the relative frequency (or its limiting value) is directly or indirectly obtained from experience alone.

This stand is, it seems to me, an exaggerated empiricism which ultimately denies the existence of any abstract science. However we must acknowledge that in ordinary actual cases we merely "presume" that the coin is a normal one, or that the dice we happen to have in hand are unbiased—and as this presumption may be false, von Mises

is right in holding that the only way we have of finding out whether this particular die is an unbiased one is to examine it experimentally.

A final word about the relation of probability and causality. In von Mises' view the theory of causality amounts to the explanation of any given physical event by an accurate description of the forces which unambiguously predetermine the given event. The Newtonian Mechanics is cited as the most perfect expression or embodiment of the principle of causality as applied to physical phenomena. But the methods used by Newton and throughout the "classical" period of physical science breaks down, according to von Mises, in a great variety of physical events: take for example, the path of a ball down an inclined board in which there are erected a large number of pins at equal intervals slightly larger than the diameter of the ball; we start a large number of balls one after the other from the central point at the top of the board and allow them to roll down to the bottom; now the balls, instead of following the same path and reaching the bottom of the board at the same point, take many different paths; in this case we are asked, how can we use the principle of causality to predict which path each ball will take? and if the path of each ball is unambiguously predetermined by physical laws why do the balls, all supposed to be equal homogeneous spheres, take so many different paths? We could of course *post factum* conceive a combination of forces derived from almost insensible air currents, from slight changes in the temperature of the balls which are not perfectly homogeneous, from slightly different degrees of resiliency, of the pins, of slight tremors produced by passing vehicles, etc., which would determine these paths: but this is so artificial, so complex and has such an *"ad hoc"*, as well as a *"post hoc"*, appearance that it does not satisfy the mind. The statistical treatment however of this problem according to the theory of probability gives as von Mises assures us, a perfect answer as to the paths that will be followed in the long run, and in fact, experience has shown that the number of balls falling into each position at the bottom of the board follows with satisfying accuracy the theoretical frequency distribution of the normal curve of errors. Von Mises' conclusion is that this being so, it is reasonable to discard the theory of causality and to accept the theory of probability as the only sufficient and even as the only possible basis of a rational explanation of this and of an ever-growing field of other physical events known as mass phenomena.

It seems to me that von Mises, like many other scientists, has been led by the acknowledged success of empiricism in this field to believe that no other or no deeper explanations are either necessary or possible. We have, indeed, to concede that the theory of probability has solved many a problem—in a statistical manner—which our limited knowledge of physical causes and our limited means of multitudinous precise measurements of extremely small and often hidden quantities pre-

[43]

vent us from solving predeterministically. Let us, therefore, grant and gladly accept the undeniable usefulness and the remarkable correctness of statistical methods based on a rational theory of probability, whilst at the same time we hold fast to the undeniable principles of a sane philosophy which teaches the existence of natural laws and of the ineluctable reign of causality throughout the width and the depth of the universe both in its majestic greatness and its alluring minuteness.

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Coefficient Analogues in Differential Equations of Oscillatory Systems.

Stanley J. Bezuszka, S.J.

In general, the fundamental elements of an oscillatory mechanical system are its inertia, stiffness and frictional resistance. These parameters when used in any specific problem are found to be the coefficients of a second order differential equation and are intimately connected in the solution. Progressive theoretical and experimental work on electrical circuits and acoustic resonators has shown the similarity of these equations with that of the mechanical case, and investigators have harmonized the group by showing the analogous characteristics and physical interpretation of the coefficients used. Since the treatment of this problem has been restricted either to the mechanical case alone, or to the electrical (with slight if any mention of the acoustical) in the ordinary sources, very few references can be given where the analogy has been developed and stressed. This article is an attempt to coordinate the three systems so that the resemblances will be made obvious and accessible to those desiring to supplement the subject still further.

For the mechanical case, we begin with the consideration of a particle which is attracted toward a fixed point with a force proportional to the distance from it, and then gradually impose other conditions. Thus the equations of motion become[1] :

$$m \frac{d^2x}{dt^2} + \left(1/C_M\right) x = 0 \qquad \cdots\cdots\cdots \quad (1)$$

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1. For a pictorial reference that will involve the three equations consider a mass attached to a spring and sliding in a wooden groove.

[45]

$$m \frac{d^2x}{dt^2} + r_M \frac{dx}{dt} + x/C_M = 0 \qquad \cdots\cdots \quad (2)$$

$$m \frac{d^2x}{dt^2} + r_M \frac{dx}{dt} + x/C_M = Fe^{jwt} \qquad \cdots\cdots \quad (3)$$

Eq. 1. Simple harmonic motion with no resistance. Thus force is proportional to the displacement and we have

F is proportional to x
$F = -x/C_M = ma = m \ d^2x/dt^2$

Eq. 2. Equation of motion with resistance proportional to velocity

R is proportional to dx/dt
$= -r_M \ dx/dt$

Eq. 3. Particle of mass m and one degree of freedom acted upon by a periodic force F cos wt , a restoring force $-x/C_M$ and a frictional retarding force $-r_M x$.

The theory of the electrical circuit and its parameters can now be compared to the mechanical analogue. From experimental researches into the action of a coil of wire inserted in a circuit, we know that the current does not reach its maximum value instantaneously. Obviously, inductance then has inertial properties very much similar to those of mass. Current is defined as the time rate of change of charge, that is, $i = dq/dt$, and is therefore analogous to velocity. Whereas the charge, electromotive force and resistance can be compared respectively to a displacement, mechanical force and the frictional dissipation in a mechanical system. We thus have another set of differential equations whose coefficients, though electrical parameters (and connected in series) behave practically the same way as the mechanical elements.

$$L \frac{d^2q}{dt^2} + Rq/C_e = 0 \qquad \cdots\cdots\cdots \quad (4)$$

$$L \frac{d^2q}{dt^2} + r_e \ dq/dt + q/C_e = 0 \qquad \cdots\cdots \quad (5)$$

$$L \frac{d^2q}{dt^2} + r_e \ dq/dt + q/C_e = E \ e^{jwt} \qquad \cdots\cdots \quad (6)$$

[46]

Eq. 4. A condenser discharging through an inductance coil. A resistance has been included and the case is inserted merely for the analogy rather than for any practical use.

Eq. 5. Circuit equation containing a charged condenser, a resistance factor and an inductance.

Eq. 6. An electric charge q in a circuit containing a resistance, capacitance, inductance and subject to an impressed force $E \cos wt$.

The above sets of equations are quite similar and symmetrical. Unfortunately,[2] there has been (as far as the writer knows) no such detailed analogy made in the acoustical case for equations (1) and (4). But there is an equivalent for the other equations and this is best approximated by the Helmholtz resonator. The Helmholtz resonator is an enclosure communicating with the external medium through an opening of small area. The opening may be flat or it may be in the form of a narrow neck. In both cases it is relatively simple to separate the resonance elements.

Inside the resonator there is a volume of gas of magnitude V which is alternately compressed and expanded by the movement of the gas in the opening. It thus provides by analogy the stiffness element of the system. The gas in the opening moves as a whole and provides the mass or inertia element. At the opening, there is a radiation of sound into the surrounding medium leading to the dissipation of acoustic energy and providing the dissipation element. Now since all these elements will be the coefficients of the differential equation, we must calculate their magnitudes.

If the opening has a neck of length l small in comparison with the wave length, and with a cross-sectional area S, the mass of gas

2. It is true that certain of the acoustical parameters are discarded or set equal to zero in acoustic filter theory, but the equations here are usually quadratics and no analogy can be readily seen.

in the opening is

$$m = DV = dSl$$

where $d =$ constant mean density of
medium at any point.

This can be written in another form by introducing the quantity $g = S/l$,
which is called the acoustic conductivity of the opening. We substitute
this into the expression for mass whence the latter becomes

$$m = S^2 d/g = ld/S .$$

To get the expression for the dissipative force, we need to calculate
the amount of acoustical energy that is radiated from a hemispherical
source of sound in a fluid. This reduces to

$$(S^2 wd/W) \, dy/dt$$

where $y =$ particle displacement
$W =$ wave length.

Finally, we must calculate the stiffness coefficient. For this it is
necessary to calculate the force acting on the area S of the opening.
If the volume V of the resonator is decreased adiabatically by the
amount dV, the excess pressure is

$$p' = sc^2 d = (-c^2 d)dV/V$$

where $c =$ velocity of propagation
of the disturbance.

for by definition, since the mass is constant, $d(V_d) = 0$ and
$s = -dV/V$. Now $dV = -Sy$, if the displacement producing the volume
change is y. Therefore the force acting on area S is $S^2 c^2 yd/V$.
Putting $S = Y/y$ and supposing that the resonator is first force free
and then in the following case driven by an external force producing
pressure P, we have

$$m \frac{d^2 y}{dt^2} + K \, dy/dt + fy = SP$$

whence since

$$m = ld/S \qquad K = (S^2 wd/W) dy/dt \qquad f = S^2 c^2 yd/V$$

we have

$$(ld/S) \, d^2 Y/dt^2 + c^2 dY/V = 0 \qquad \ldots\ldots\ldots(7)$$

$$(ld/S) \, d^2 Y/dt^2 + (wd/W)dY/dt + dc^2 Y/V = 0 \qquad \ldots\ldots (8)$$

$$(ld/S) \, d^2 Y/dt^2 + (wd/W)dY/dt + dc^2 Y/V = P \, e^{jwt} \qquad \ldots (9)$$

Eq. 7. Equation of motion with coefficient of friction absent. An ideal case.

Eq. 8. Equation of motion for force free case.

Eq. 9. A medium in an acoustical system of one degree of freedom, with an applied force acting, Y is the volume displacement and S the cross sectional area and V the volume of the cavity.

Though we have set the impressed force equal to $E\,e^{jwt}$ and so on in all three cases, yet this has been done to facilitate the mathematical solution of the equations and the chief interest will usually center about the real part.

As a summary of this discussion on the coefficients, the following table has been prepared. (The subscripts refer to their respective system, that is, e for electrical, M for mechanical and A for acoustical.)

Resistance	Friction	Fluid Resistance
$r_e = E/I$	$r_M = f_M/u$	$r_A = p/U$
E = voltage	f_M = mech. force	p = pressure
r_e = resistance	r_M = " resistance	r_A = acoustical resistance
I = current	u = velocity	U = volume current, product of particle velocity and area of conduit.

Inductance	Mass	Inertance
$E = (L)dI/dt$	$f_M = (m)du/dt$	$p = (M)dV/dt = (m/S^2)dU/dt$
		S = area over which the mass is distributed
		M = inertance.

Capacitance	Compliance	Acoustical capacitance
$i = (C_e)dE/dt$	$f_M = x/C_M$	$p = (-c^2 d)\ dV/V = Y/C_A$
	x = displacement	Y = volume displacement
	The constant of the spring stiffness is the reciprocal of the compliance.	C_A = acoustic capacitance.

[49]

RECENT DEVELOPMENTS IN
NAVIGATIONAL INSTRUMENTS

By Rev. Paul A. McNally, S.J.

(Abstract)[1]

During the past few decades the traditional type of sextant has received a number of new forms of instruments partly supplement it and partly displacing it. The two-fold purpose of these new inventions is to enable the navigator, whether on sea or in the air, to take observations of the altitude if a heavenly body without dependence on a visible horizon. Several forms have been developed using a spirit level to establish the vertical position of the instrument. Another form, the Hagner Sextant, uses the principle of the pendulum to secure the verticallity of a tube carrying at its bottom a ground-glass plate in the horizontal position upon which small lead pellets are dropped whilst sighting the celestial object; these pellets make marks on the plate from which the angle of elevation may be read or calculated.

The most recent instrument, invented at Georgetown (by the author of this paper), instead of a pendulum uses a steel sphere rolling in a circular runway and thus determining, when it comes to rest, the division of the circular scale vertically under the center of rotation of the telescope; the sphere is then clamped in position and vertical angle of the celestial object (i.e. the complement of the altitude) is read off to the nearest 2 minutes of arc by means of a vernier.

A long series of observations made with this instrument, which is to be covered by a patent already applied for, shows that the accuracy of observation is well within the limits required for all practical navigational purposes.

The question of producing the instrument on a commercial basis is now being considered by one of the largest manufacturers of airplanes and aviation instruments.

1—These abstracts are of papers at New York Summer Convention.

THE NEW RULES FOR THE COMPUTATION OF ACADEMIC
GRADES IN THE REVISED RATIO STUDIORUM

Rev. Edward C. Phillips, S.J.

(Abstract)

The present rule was indicated by an example, and attention was called to the main difference between the new rules and those which had been in force from 1933 to 1941.

The main change is in the weights assigned to the work of the first two years in the course for the Licentiate in Philosophy and the first three years in the Licentiate course in Theology. In the previous rules, followed up to 1941, each of these years was given only unit weight; the sum of all the weights for the four years of Theology was then about 26; and hence in most cases· the degree of success obtained by the student in his first three years had very little, if any, effect on the final grade or "Qualitatis approbationis" for the Licentiate. In fact, in the typical case given as an example in the *Acta Romana* (Vol. VII, 1932, p. 211) the final grade would have been the same whether the student had earned only the lowest passing grade of 6 or "probatus" in each of his first three years, or the highest possible grade of 10, or "Summa cum laude probatus". Thus the work of a whole year had no more influence in the final mark than one of the "Disciplinae ·auxiliares" such as, e.g., the one-semester-hour course in Ascetics or Liturgy.

In the new rules each of the first three years is given a weight equal to the sum of all the weights of the individual courses of the year. This has resulted in giving the first three years a combined weight of 36 out of a totality of 92, or close to 40%, instead of a weight of 3 out of a total weight of 26, or only 12%, according to the old rules. It should be remembered that the final oral examination (the "De universa" in Philosophy, or the Theological portion of the "Ad Gradum" in Theology) always receives a weight of 50% in both the old and the new rules.

The rules assigning the weights will be found in the *Appendix*, and not in the body, of both the *Statuta* and *Ratio Studiorum*, and hence can be changed by the Society without the need of any new pontifical *legislation* or dispensation.

CHEMISTRY

HISTORY OF ATOMIC WEIGHT DETERMINATIONS
Rev. Gerald F. Hutchinson, S.J.

Part One — Early Work[2]

John Dalton—the first table of atomic weights.

The eighteen century had scarcely given way to the nineteenth when the science of chemistry, recently freed from the trammels of an erroneous phlogiston theory, turned the corner into the broad road of atomism, on which road it has advanced by leaps and bounds to its present position in our own day. About the year 1802-3 John Dalton was engaged upon the work of determining the composition of methane and acetylene. His keen mind recognized quickly that there was four times as much hydrogen per unit carbon in the first compound as in the second. He therefore, followed up this discovery with an investigation of the ratio of the elements in other compounds. Working with carbonic acid and oxide he found again a simple ratio of the carbon content of each compound per unit oxygen. His next study was directed to that set of compounds which every teacher of elementary chemistry uses nowadays to illustrate the law of multiple proportion.

N_2O	Dinitrogen oxide[3]
NO	Nitrogen oxide
N_2O_3	Dinitrogen trioxide
NO_2	Nitrogen dioxide
N_2O_4	Dinitrogen tetroxide
N_2O_5	Dinitrogen pentoxide.

1) This paper was first composed as a seminar at Holy Cross College. The author is unfortunately unable to give individual references. He has used various References contained in "Chemical Abstracts", and gratefully acknowledge all of them.
2) Other papers on later developments will follow in the Bulletin.
3) The nomenclature of these compounds is taken from the "Report of the Committee of the International Union of Chemistry for the Reform of Inorganic Chemical Nomenclature, 1940."
   Cf. Jorissen, Bassett, Damiens, Fichter and Remy.
   Journ. Am. Chem. Soc. *63* 889, 1941. or Reprint.

These results were sufficient to convince Dalton that such regularity could not be due to chance, and he enunciated his now famous law of multiple proportion. The explanation of this numerical relationship was to be found in the theory, that combination of the elements took place between minute homogeneous parts of matter; and in this con-

clusion, the philosophical hypothesis of the Greek Democritus became the scientific atomic theory. It is enunciated in the two simple statements,

1—Every element is made up of homogeneous atoms whose weight is constant.

2—Chemical compounds are formed by the union of atoms of different elements in the simplest numerical proportions.

Dalton then set to work on a determination of the relative weights of these atoms, and it is that work, started by Dalton more than a hundred years ago and continued to the present day, that we are about to trace.

Being a pioneer, Dalton found many assumptions necessary, the most important of which was that the best known compounds have the simplest formulas. Analyzing water, the only compound of hydrogen and oxygen, hydrogen peroxide being unknown at the time, and therefore having the simplest formula, HO, he found the ratio of the weights to be 1: 6.5. Analytic methods were in a very undeveloped state at that time, so it was impossible that his results could be accurate. Yet, to Dalton credit is due for giving us the law of definite proportion, the atomic theory and the first set of atomic weights.

The speed with which Dalton's atomic theory took hold of the scientists of the time is shown by the fact that, in the year 1805, only a couple of years after the work of Dalton, Gay-Lussac published his law of combining volumes. Chemical reactions between gases take place in simple volumetric ratios, and when contraction occurs, the dimunition in volume stands in simple ration to the volumes of the original gases. Since chemical reactions occur between simple multiples of combining weights, and since the volume of combining gases also stand in simple ratios, it follows that the weights of equivalent volumes stand in simple ratios to combining weights. There were however, difficulties to be overcome before these relations could be used for atomic weight determinations.

$$N+O=NO \quad \text{Nitrogen oxide.}$$

The volume of NO should be half the combining volumes of nitrogen and oxygen. But actually it was equal to the combining volumes.

In 1811, Avagadro came forward with the solution. He said that equal volumes of ALL gases contain the same number of MOLE-CULES, under the same conditions of temperature and pressure. There is no distinction between elementary and compound gases. There is for the first time the distinction between atoms and molecules.   He pointed out that any convenient number of atoms may be assumed to be in the molecule, but two is the maximum number usually required. The anomaly above is now easily explained.

$$N_2+O_2=2 \ NO.$$

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Unfortunate, indeed, was it for chemistry, that Avagadro was entirely ignored until his countryman, Cannizzaro, brought him to the fore at the Congress of Chemists at Karlsruhe in 1860.

PROUT BERZELIUS AND STAS.

Atomic weight determinations received a great stimulus in the year 1815, when Prout brought forth his famous hypothesis. Noting that most atomic weights were nearly whole numbers, he suggested, that if the determinations were made accurate enough they would be found to be exactly whole numbers in every case, and thus exact multiples of the atomic weight of hydrogen. The extreme of the theory was, that all elements were compounds of hydrogen atoms, and Prout did not hesitate to go the limit.

Two men in particular met the challenge, Berzelius and Stas.

Atomic weight determinations had by this time resolved themselves into two problems, 1—the determination of the combining ratios, and 2—the determination of the numerical ratio of the atoms concerned in the molecule. Most of the work to date had been done on the first problem, the second being taken care of by assumptions, usually arbitrary in nature. Berzelius realized the hopeless arbitrariness of the assumptions of Dalton, especially that the best known compounds have the simplest formulas. He therefore, looked for other criteria, in order to determine the number of atoms in the molecule. Gay-Lussac's law of combining volumes, the weight of equal volumes of elemental gases are proportional to their atomic weights. provided an excellent standard for the elemental gases. Its application, however, to compound gases was not clear to Berzelius. If the views of Avagadro had received their due from the scientists of the day, the progress of chemistry might have been advanced by several decades, but "IFS" have no place in history.

Berzelius had to find another criterion for the numerical proportion of the atoms in the molecule. He was always an ardent admirer and student of Lavoisier, and now the French influence came into play. He took as his standard the varying quantities of oxygen that unite with the various elements. A typical example shows the cleverness and originality of his reasoning.

$$3PbS + 8HNO_3 \rightarrow 3PbSO_4 + 4H_2O + 8NO$$

He formed lead sulphate from lead sulphide. He noticed that there was an excess of neither lead nor sulphur in the supernatant liquid, and therefore, concluded that the ration of lead to sulphur was the same in both compounds. Lead sulphate was considered a compound
1—What follows is the reasoning of Berzelius.

of lead oxide and sulphur trioxide, which at the same time was thought to be sulphuric acid. The amount of oxygen in lead oxide was known,

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and therefore, the balance of it, or three times that amount must be in the sulphur trioxide. The number of atoms of oxygen, therefore, in sulphur trioxide, must be three, or some multiple of three. In the absence of any good reason for postulating a larger number, Berzelius assumed the number was three. The atomic weight of oxygen was fixed by its density, and therefore the atomic weight of sulphur was 32.

Berzelius next argued that, IF the atomic weight of lead is the weight of that element that will unite with 16 units of oxygen, the same would also be true for Ba, Ca and other similiar elements. It is obvious that the reasoning of Berzelius was far superior to the reasoning of Dalton, but even yet it is not free from assumptions. Following the law of Gay-Lussac, he noticed that two volumes of hydrogen united with one volume of oxygen to give one volume of water, and therefore wrote for the first time, the formula of water, H_2O, ratio, $2/1$.

Berzelius was, perhaps, the first to realize that atomic weights, exactly known, were a necessity for the new chemistry. He therefore, set himself the task of determining the combining weights with the greatest possible accuracy. Within the space of little more than ten years, he accomplished this for 43 elements, by the preparation, purification and analysis of over 2000 compounds. This feat he accomplished with scarcely more than the advantages of a modern kitchen. In most cases he had to make his own starting materials, and more often than not devise his own methods. Thus quantitative analysis saw great progress during his work. His results speak for themselves, his values comparing as favorable with the values of today as was possible at his time.

In 1819, Dulong and Petit called attention to the remarkable relationship between atomic weights and specific heats. They showed that the heat capacity for all solid elements was the same. Therefore the product of the specific heat and the atomic weight gave a constant value. Today, the mere mention of the specific heats to the student of physical chemistry brings to mind an almost infinite series of complications, so that this method is dependent on too many factors to serve as an independent means of atomic weight determinations. At the time it was proposed it offered another measurable physical property, free from the assumptions found in the reasoning of Dalton and Berzelius, which might serve as a method of determining atomic weights, and therefore deserves a place in our history.

The law of isomorphism published by Mitscherlich in 1818, may be summarily stated in his own words.

"The same number of atoms, combined in the same manner, produce the same crystalline form; the crystalline form is independent of the nature of the atoms, and dependent solely on their num-

ber and mode of combination. This law can be used only in conjunction with other methods to ratify atomic weights."

If we take two compounds,
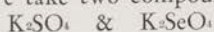
K_2SO_4 & K_2SeO_4

and assume

1) that the first has one atom of sulphur
2) that each has the same number of atoms
3) that the atomic weight of sulphur is 32.

then: 32 : At. Wt. of Se :: the wet. of any arbitrary amt. of K_2SO_4:
the wt. of the same amt. of K_2SeO_4.

or in a concrete case,

$32 : X :: 18.39 : 45.34.$
$X \quad 79.00$

However, the method most extensively used, and to which our present values are largely due, is the chemical method, based upon gravimetric analysis and first attempted by Dalton and remarkably improved by Berzelius.

The hypothesis of Prout, daring and original as it was, aroused the admiration as well as the ire of scientific men. One of the most anxious for the welfare of the theory was Jean Servais Stas, a Belgian chemist. One method of proving conclusively the truth or falsehood of Prout was an accurate determination of atomic weights reaching the acme of analytic perfection. To this work then, Stas devoted his life. His work was that of a genius, and so accurate were his values that it required another genius, T. W. Richards, to point out their inaccuracy. It is worthy of note that his ultimate conclusion, that Prout's hypothesis is absolutely untenable, was regarded as the last word by his contemporaries. Since then, however, the discovery of isotopes and particularly the more recent discovery of the isotope of hydrogen, has thrown the question of Prout wide open to discussion again.

Nevertheless, all the previous work on atomic weights was surpassed by the labors of Stas. He refined the process of quantitative transformations to a degree never before equalled, worked with materials highly purified, employed exceptional weights of substances, often several hundred grams, made his weighings on a balance of unprecedented precision, and exercised extraordinary care in manipulation. Indeed, so determined was he to avoid the last trace of error, that some of his precautions were illusory as far as final results were concerned.

Stas, was, perhaps, the first to fully appreciate the value of silver in atomic weight determinations. This value is due to the slight solubility of the silver halides, especially the chloride, and to the fact that these salts can be readily prepared in a high state of purity. If the

compound, containing the element whose atomic weight is to be determined, is thoroughly purified and then decomposed by silver, the atomic weight can be readily calculated. Oxygen, with the atomic weight 16, was Stas' standard. Therefore, that the atomic weights might be determined by the use of silver and then referred to oxygen, the silver-oxygen ratio was of extreme importance. It is unfortunate that silver oxide is not sufficiently stable to serve in determining this ratio. An indirect method must be used. The method of Stas was as follows, and illustrates the determination of the atomic weight of lithium, and the ratio of silver to oxygen.

1) By decomposing $LiClO_3$, he obtained the ratio,

$$\frac{3\ O}{LiCl}$$

2) By the displacement of Li, from LiCl by Ag he obtained the ratios,

$$\frac{LiCl}{Ag} \quad \& \quad \frac{LiCl}{AgCl}$$

3) By multiplication,

$$\frac{3\ O}{Li1Cl} \cdot \frac{LiCl}{Ag} \quad \frac{3\ O}{Ag}$$

Three things are worthy of note, 1—the choice of the starting materials, 2—the preparation of pure silver, 3—the purification of the LiCl. This work is so closely connected with the work of Richards, that a comparative study of the two men's work will be most advantageous. This will be given in a separate paper in a later issue of the BULLETIN. It will be pointed out that Richards proved several errors in the work of Stas which vitiated his results. We should remember, though, that Stas came before Richards, and therefore did the pioneer work necessary for the wonderful work of Richards. It is history that before any work can reach the peak of perfection, a certain amount of spade work must be done. Very often this preparatory process requires a genius just as much as the perfect work which follows. Stas a genius cleared the way for Richards another genius. Let us give each the credit due him.

(To be continued)

NEWS ITEMS

BOSTON COLLEGE
Biology Department

The regular weekly seminar on medico-moral topics is being conducted again this year. A referendum concerning birth control was on the ballot for voters of Massachusetts this year. Just before election day this subject was discussed in detail during a regular meeting. On November 19th, the Rhythm theory was the topic. Other topics to be discussed during the year are, Sterilization, Therapeutic abortion, Ectopic Gestation, Euthanasia and Eugenics.

Chemistry Department

The chemistry department of Boston College was approved by the American Chemical Society at the Buffalo meeting last September.

There are over seven hundred students registered in the various chemistry courses at this year.

Forty students are engaged in an evening course in General Applied Chemistry, sponsored by the E. S. M. W. T.

Forty-eight students from St. Clement's Junior Seminary are doing their Chemistry laboratory work here Saturday mornings.

CANISIUS COLLEGE
Biology Department

The Mendel Club Lecture Program
1942-1943

September 28th.   MAXWELL L. LOCKIE, M.D., Professor of Therapeutics, School of Medicine, University of Buffalo, "Arthritis—Its History and Treatment."

October 12th.   HERMAN E. BOZER, M.D., Associate Professor of Otolaryngology, School of Medicine, University of Buffalo, "The Uses of the Bronchioscope." Illustrated.

November 2nd.   WILLIAM T. CLARK, M.D., Superintendent of Meyer Memorial Hospital, Professor of Hygiene and Public Health, School of Medicine, University of Buffalo, "Epidemiology."

November 23rd.   MARGARET WARWICK, M.D., Pathologist, Sisters of Charity Hospital, Instructor of Pathology, School of Medicine, University of Buffalo, "Tumors—General Cause and Behavior."

December 14th. To be announced. Alumni Banquet during Christmas Holidays, date to be announced.

January 4th. JAMES P. KINNEY, M.D., Chief of Department of Anaesthesia, Sisters of Charity Hospital, "Principles of Anaesthesia."

February 1st. CHARLES F. BECKER, M.D., Instructor of Pathology, School of Medicine, University of Buffalo, "Poison Gases."

February 22nd. SISTER M. ALOYSIA, St. Mary's School for the Deaf, "Problems in Contacting the Mind of a Child—Born Deaf, Dumb, and Blind."

March 1st. ANNUAL BANQUET.

March 22nd. LEON J. GAUCHAT, D.D.S., Assistant Professor of Principles of Practice School of Dentistry, University of Buffalo, "Dentistry—Your Teeth—The War." Illustrated.

April 12th. PAUL J. RUTECKI, M.D., Medical Examiner, Erie County, "Medico-Legal Aspects of Post Mortem Examinations."

May 10th. HOYT E. DE KLEINE, M.D., Associate Surgeon—Buffalo General Hospital, Plastic Surgeon, Millard Fillmore Hospital, "Plastic Surgery."

May 24th. To be announced.

Chemistry Department

Dr. James H. Crowdle, Professor of Chemistry at Canisius College, was appointed general gas protection officer and head of the decontamination organization for the City of Buffalo on Saturday, October 24, 1942.

The appointment was made by Mayor Joseph J. Kelly, Director of the Buffalo office of civilian protection.

Mayor Kelly also appointed Mr. Austin V. Signeur, Assistant Professor of Chemistry at the College, deputy gas protection officer and deputy head of the local decontamination organization. Dr. Crowdle and Mr. Signeur will be assigned two places on the main panel-table of the Buffalo Control center, from which all agencies and functions of the Committee would be directed in case of a war-time emergency.

They will conduct the simulated gas raids and work out plans for the decontamination of civilian areas in case of a real raid.

The Western New York Section of the American Chemical Society has selected Thursday, January 21, 1943, for its meeting at Canisius College. Lieutenant James R. Barrett, who received his Master of Science degree from Canisius in June 1942, and who is the ballistic expert at the laboratory of the Buffalo Police Department, has been selected by Father T. Joseph Brown, S.J., Chairman of the Department of Chemistry, to give the dinner address at the meeting. Lieutenant Barrett will talk on scientific crime detection in the City of Buffalo.

Chemistry Department — Canisius College Alumni

Chemical Society Program for 1942-1943

Monday, October 5. S. M. LAZARUS, National Aniline & Chemical Co., "Patent Chemistry."

Monday, November 2. LOREN SMITH, Curtiss-Wright Corp., "Chemistry in Aircraft Production."

Monday, December 7. EMMETTE F. IZARD, E. I. du Pont de Nemours & Co., "Chemical Warfare."

Monday, February 1. RAYMOND BOTT, Will Corporation. "Electrical Instruments for Chemical Analysis."

Monday, March 1. F. L. KOETHEN, American Lubricants, Inc., "The Historical Development of the Niagara Frontier."

Monday, April 12. JAMES O. RALLS, University of Buffalo Medical School, "Recent Advances in Biochemistry."

Monday, May 3. WILLIAM R. SHERIDAN, Dunlop Tire & Rubber Corp., "Synthetic Rubber."

All Meetings in Horan-O'Donnell Science Building, Canisius College, 8:00 P.M.

HOLY CROSS
Chemistry Department

Departmental Research Program — 1942-1943

Graduate Assistants:

Chromatographic Adsorption with special emphasis on problems in the department.

Mr. Robert M. Dee   Directed by Prof. Charest

A Colorimetric Analysis for Hydroxylamine.

Mr. Bernard J. Digris   Directed by Fr. Fiekers

Refractive Indexes of the Picrates.

Mr. John A. Green   Directed by Prof. Tansey

The Rearrangement of Methyl Aniline.

Mr. John P. Hardiman   Directed by Prof. Casey

Relative Rates of the Formation of Phenyl-substituted Quaternary Ammonium Compounds.

Mr. Michael F. Kilty   Directed by Prof. Tansey

Formation of Diphenylamines in the Gattermann Reaction.

Mr. James M. Owens   Directed by Prof. Baril

Senior Laboratory Theses:

Wetting Agents in Volumetric Analyses.

Mr. Robert R. Kiely   Directed by Prof. Charest

The Stripping Action of Various Reagents on Zinc Covered Wire.

Mr. Charles W. Chagnon   Directed by Prof. Charest

Theory and Technique of Superheated Steam Distillation with Application to the Separation of Benzene Sulfonyl Chloride and

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Dimethyl Aniline.
Mr. Charles A. Polachi   Directed by Prof. Casey
Two Component Phase Diagram: Diphenylamine-Benzoin.
Mr. Michael D. Riordan   Directed by Prof. Tansey
Catalytic Hydrogenation.
Mr. William P. Whelan   Directed by Prof. Casey
Condensation: Alkyl Halides, Aryl Amines.
Mr. Charles J. McNulty   Directed by Fr. Fiekers
Acetone and other Solvents of Crystallization.
Mr. Peter P. Salatiello   Directed by Prof. Baril
Azeotropic Mixtures in the Chloroform Synthesis.
Mr. John P. Loughman   Directed by Prof. Baril

SEMINAR PROGRAM
(Winter Term 1942-1943)

November 6. HISTORY OF CHEMISTRY.
Father Theodor Wulf, S.J., and his Theory of General Radio-
activity.
Mr. Robert M. Dee, B.S. '42 (Graduate Assistant)
                                         Directed by Fr. Fiekers

November 13. ORGANIC CHEMISTRY. The Vitamins.
Mr. Andrew S. Carten, (Senior A.B.)   Directed by Prof. Baril.

November 20. INORGANIC ANALYSIS.
          Wetting Agents in Volumetric Analysis
Mr. Robert R. Kiely, (Senior B.S.)
A Study of the Stripping Action of Various Reagents on Zinc
Covered Wire.
Mr. Charles W. Chagnon, (Senior B.S.)
                                   Both directed by Prof. Charest

November 27. PHYSICAL CHEMISTRY.
The Two Component Phase Diagram Diphenylamine-Benzoin.
Mr. Michael D. Riordan, (Senior B.S.)   Directed by Prof. Tansey

December 4. ORGANIC CHEMISTRY.
     The Theory and Technique of Steam Distillation
Mr. Charles A. Polachi, (Senior B.S.)
               Catalytic Hydrogenation
Mr. William P. Whelan, (Senior A.B.)
                                    Both directed by Prof. Casey

December 11. INORGANIC CHEMISTRY.
       Decontamination in Chemical Warfare
Mr. J. William Quinn, (Senior A.B.)

       ORGANIC CHEMISTRY
    Alkyl Halide — Aryl Amine Condensation
Mr. Charles J. McNulty, (Senior B.S.)
                                  Both directed by Fr. Fiekers

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December 18. ORGANIC CHEMISTRY.
Organic Solvent Analogies to Water of Hydration
Mr. Peter P. Salatiello, (Senior B.S.)
Azeotropic Mixtures in the Chloroform Synthesis
Mr. John P. Loughman, (Senior B.S.)
Both directed by Prof. Baril

January 8. ANALYTIC CHEMISTRY.
Devising New Colorimetric Analyses
Mr. Bernard J. Digris, B.S. '42, (Graduate Assistant)
Directed by Fr. Fiekers

January 15. PHYSICAL CHEMISTRY.
Refractive Indices of Solids
Mr. John A. Green, B.S. '42, (Graduate Assistant)
Directed by Prof. Tansey

January 22. ORGANIC CHEMISTRY.
Theory of Nitrogen Rearrangements
Mr. John P. Hardiman, B.S. '42, (Graduate Assistant)
Directed by Prof. Casey

January 29. PHYSICAL CHEMISTRY.
Comparative Condensation Rates
Alkyl Halides — Aromatic Amines
Mr. Michael F. Kilty, A.B. '42, (Graduate Assistant)
Directed by Prof. Tansey

February 5. ORGANIC CHEMISTRY.
Recent Interpretations of the Sandmeyer and
Gattermann Reactions
Mr. James M. Owens, B.S. '42, (Graduate Assistant)
Directed by Prof. Baril

ST. PETER'S COLLEGE
Chemistry Department

On Wednesday, October 14, the Students Chemists' club had their first meeting of the first semester. The subject of the lecture was "Chemical Warfare." The lecturer was Dr. Claude Schwob, of the chemistry department of the college.

St. Peter's College Chemists' Club had their regular monthly meeting at the college, in the science building, in the chemistry lecture-hall on November 12th. The lecture was delivered by C. M. Gooding, Ph.D., Chief Chemist of the Best Foods Corporation, Bayonne, N.J. The subject of the lecture: "Researches on Vegetable Oils and their Application in Industry." After the lecture, a long discussion was held.

At the next meeting of the St. Peter's Alumni Chemists' Club,

on December 17, the lecture will be presented by Rev. Richard B. Schmitt, S.J., D.Sc.; the topic will be: "Finger-prints of Crystals."

CHEVERUS HIGH SCHOOL

A class in Elementary Aeronautics has been organized by Fr. Hutchinson, S.J. The class meets after the regular school hours, and therefore, due to the large number of boys who must work during the afternoons, the class is small, numbering fifteen. Several of these are veteran model plane builders, and so the class is well supplied with demonstration material. The text books used are "Elements of Aeronautics", Pope-Otis and "Mathematics in Aviation" Osteyee, one of the Air Education Series.

Several articles have been written to show the value of toy balloons in lecture demonstrations.[1] In the beginning of the school year, when something startling is needed to get attention and is really expected by the students, these balloons can be used to demonstrate the following points all in one demonstration. By fastening a balloon over the mouth of a test tube containing zinc and a mineral acid the balloon is blown up with hydrogen. This demonstrates, 1) the generation of the gas, 2) heat is given off by the reaction, 3) the reaction proceeds in spite of the pressure caused, 4) diffusion of gases, since ordinary cheap balloons can be blown up to a certain point only. If the balloon is then taken off from the test tube and tied around the neck and released it will rise to the ceiling thus demonstrating 5) that the gas is lighter than air. By touching a match to it while at the ceiling it is safe to demonstrate 6) its explosive property. By stoppering the test tube with a rubber stopper and small glass tube the hydrogen may be burned thus demonstrating 7) the difference between the conditions for explosion and quiet burning.

1. Mitchell, Journ. Chem. Ed. *18* 6 June 1941.

Walton ibidem *8* 303X 1931.