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NOTICE TO AUTHORS

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Science and Philosophy

TOWARD AN ACCEPTABLE PHILOSOPHY OF SCIENCE

REV. JOSEPH T. CLARK, S.J.

The problem under discussion is characterized by three significant properties. It is, first of all, a decidedly *current* issue. It is also and undeniably an *important* topic. And it is finally, as we shall see, tantalizingly *complex and difficult*.

The problem of a satisfactory philosophy of science is indeed a lively and a contemporary issue if we may accept the professional interests of representative scientists as a criterion. Few men, for example, of our times have been as favorably situated as President James Conant of Harvard University to meet and mingle with the foremost physical scientists of the world in cooperative military projects, and to discover in intimate association with them the genuine contours of their current interests and anxieties. And it was undoubtedly in recognition of his uniquely privileged position for such excellent observation of modern scientific trends that the committee in charge extended to Dr. Conant the invitation to deliver the Terry Lectures on some important scientific topic of his choice at Yale University in 1946. It is therefore profoundly significant that President Conant selected as his general topic: *On Understanding Science* [New Haven, Yale University Press, 1947]. For an adequate understanding of science: its objectives, methodology, techniques, and results, is an integral part of the problem now in hand.

And it was just five years ago voted at the Annual Convention of the American Association for the Advancement of Science to institute and establish a new regularly constituted section, concerned exclusively with developing and discussing problems connected with an adequate philosophy of science. Since that inaugural date the meetings of this popular section have always been well attended and excellently conducted. So excellently, in point of fact, that the 1947 Chicago meeting resulted in the revitalization and reorganization of The Philosophy of Science Society with an enviable membership roster, and of its official journal: *Philosophy of Science*, with a very representative list of regular subscribers.

Although, therefore, the problem of an acceptable philosophy of science remains still a problem, only in the discussion stage, the issue is undoubtedly current, contemporary, and very much alive. No self-respecting technician, researcher, science student or science teacher can afford to ignore its presence nor fail to keep well informed and abreast of its developments.

The problem is also one of indubitable importance from a cultural and sociological point of view. For unresolved differences of opinion concerning the function of scientific research in the general social context are symptomatic of a dangerous cleavage and a perilous rift in the texture of the national intelligence. So long as this split endures to separate elements of the population into antagonistic camps, other urgent social problems cannot be attacked with that unity of understanding which is indispensable to their solution, nor can new projects for social advancement rest upon a sound and firm foundation of a unified national consciousness. For if certain segments of the population uncritically assume that science is the *sole* source of certified human wisdom, then an impassable barrier is set before the possible contributions to the general welfare of other existing agencies, such as religion, art, philosophy, music and morality. If, on the other hand, other social groups uncritically and with inherited prejudices degrade the role of science and scientists to that of insignificant tinkering with mechanical gadgets, antagonism is inevitable, remedial measures are stymied in conflict, and cooperative advances become impossible to achieve. While, therefore, it is happily true that these latent oppositions have not yet erupted in their full dimensions to disorganize the American social scene, it is undeniable that the unresolved problem of an acceptable philosophy of science remains today a danger spot of extreme concern to conscientious and responsible social analysts of our times.

The problem in hand is, therefore, I submit, both current and critical. It is also tantalizingly complex and demands the patient and cooperative attention of all who are in any way equipped to contribute insights into the pattern of its solution. Without anticipating that final solution prematurely one may state that the complexity of the problem derives, as I see it, from two unavoidable factors: (1) an inevitable and necessary diversity of method in both the scientific and philosophic enterprise, and (2) an inertia and inelastic reluctance upon the part of both the scientist and the philosopher to appreciate and, if possible, to acquire the specialized viewpoint of the other. For both the scientific and philosophic methods of inquiry into the structure of human experience and environment demand specific, trained skills of intelligence that rapidly crystallize into inveterate habits of mind. A cyclotron, a microtome, a centrifuge are of no immediate service to a social philosopher. And a syllogism, a thesis or a subdistinction are of no known use to a nuclear physicist. And thus does it happen, I fear, that the run-of-the-mill philosopher affects to disdain the cyclotron, and the average scientific researcher pretends to contemn the dialectics of the metaphysician. Experience thus suggests that the more proficient the scientist, the more inveterate and insulated his specific habit of mind is likely to become. And likewise the more competent the philosophical analyst, the more fixed and congealed is his peculiar mental set destined to remain. What is really needed, therefore, for an effective solution to our problem is an "ambidextrous" grasp of total human experience, smoothly and conjointly achieved by a simultaneous use of *both* hands: one, the physical, empirical prehension of science, and the other, the metaphysical, transempirical comprehension of philosophy. For here, as elsewhere, it would seem that an equitable solution of the bipolar problem of an acceptable philosophy of science requires neither the eradication of philosophy nor the obliteration of science from the complex scene. What is really required is a neat and integral synthesis of both branches of inquiry in a hierarchical framework of knowledge which preserves the genuine values of each without diminishing the importance of either. But to achieve such delicate poise and balance is always a singularly difficult enterprise.

The complex character of our problem and a presentiment concerning the structure of its final solution may be discerned, I think, in the contours of an illuminating parallel problem that exercised the energies of Western intelligence over many centuries of persistent effort to achieve a balanced cultural equilibrium and an assured intellectual poise. I refer, of course, to the similar problem of the relationship and reconciliation between reason and revelation, faith and philosophy, thought and theology, which perplexed the medieval mind in its three representative traditions: Latin, Muslim, and Hebrew, in Aquinas, Ibn Rushd, and Maimonides. For all three schools professed an unalterable conviction on three basic points: (1) a spontaneous trust in the scientific and philosophic ability of man's natural reason to attain (asymptotically) the genuine truth of things, (2) a cultivated and imperturbable belief that their respective Scriptures were infallible sources of information, and (3) a conscious awareness of the disturbing fact that on some common points the conclusions of reason and the pronouncements of revelation were *apparently* in conflict. The medieval problem, therefore, was precisely so to adjust the relationship between thought and theology as to leave the native rights of reason unimpaired and the preferential prerogatives of revelation intact. The solution to this problem, then required (1) an independent and critical reexamination of the role of reason in the acquisition of truth, and (2) a similar independent and critical reexamination of the function of revelation in the total context of human knowledge. And the history of medieval thought is strewn with the debris of those who, either as enthusiastic rationalists exalted reason above revelation, or as extreme religionists degraded reason far below its due degree in subjection to revelation. For many tedious and weary centuries confusion and chaos reigned. Finally, however, the formula of solution was found, and that harmony and hierarchy established which we today recognize and enjoy. For no one of any competence is currently concerned over apparent contradictions between the authentic sense of scripture and the certified conclusions of natural intelligence.

What the problem of reconciliation between reason and revelation was to the Middle Ages, that precisely, I venture to suggest, is our modern problem of the ambiguous relationship between philosophy and science. It may very well be that the final solution of our contemporary problem will repeat the main outlines of the medieval solution. Certainly the basic structure of both problems is similar, and significantly enough, the historical stages of development are in both cases remarkably alike. There was, as I have said, a prolonged period of medieval confusion over the specific functions of reason and of revelation in the total life of human intelligence. And there is abundant evidence to establish the fact that contemporary thought concerning the proper roles of philosophy and of science is likewise embarrassed and confused. This modern befuddlement may perhaps be exhibited by a random selection of statements on the point, proferred for public consumption by representative spokesmen for one or other side of the controversy.

Professor A. C. Benjamin, for example, in his book: *An Introduction to the Philosophy of Science* [New York, The Macmillan Company (1937), p. 8] summarizes, his survey of the situation by listing five alternative attitudes on the topic, as follows:

> (1) Science *describes*, philosophy *explains;* (2) science *describes facts empirically*, philosophy *analyzes symbols logically;* (3) science describes *facts*, philosophy describes *values;* (4) science describes *quantities*, philosophy describes *qualities;* (5) science *analyzes*, philosophy *intuits*.

Thus far Professor Benjamin's condensed presentation of contemporary attitudes on the relationship between philosophy and science. A moment's serious reflection, however, will, I think, indicate the inadequacy of either one or all of these five alternative positions. *Explanation* cannot be the hall-mark of philosophy nor *description* the distinctive characteristic of science. For science also in a genuinely true sense *explains*. And what is philosophy after all but a metaphysical interpretation or *description* of the universe of experience? And while the view may be true which asserts that *science describes facts empirically*, it is far less accurate to state that philosophy is only *the logical analysis of vacuous symbols*, and not the examination and and interpretation of real entities in the world. Nor is it easy to agree that philosophy is exclusively concerned with *values* whereas science occupies itself with *facts*. For *facts* also and necessarily enter into the purview of philosophy as the indispensable basis and groundwork for whatever *values* it subsequently elaborates. Nor can *quantity* and *quality* adequately distinguish science and philosophy. For the traditional categories of philosophy have always comprised both the quantitative and qualitative, both the dimensional and dynamical characteristics of the external world. Nor finally can one accept with equanimity the position that *science analyzes* while *philosophy intuits*. For while philosophy like science acknowledges the historical occurrence of sudden insights into aspects of reality, neither has more intuition at its disposal than the other in the routine performance of its tasks.

Dr. Louis T. More, discussing with retrospective hindsight the pregnant elements in "Newton's Philosophy of Nature" [*The Scientific Monthly* 56 (1943) 491-504, p. 491] remarks that

> . . . the reform of science instituted by Bacon, and completed by the genius of the seventeenth century, was founded on the thesis that science should not be concerned with ultimate reality and causes; since then science has been more and more restricted to the inquiry of *how* things occur, and not of *why*. . . .

Thus far Dr. More. But is it even historically true to summarize all philosophy as an unhappy excursion into teleology, and to distinguish philosophic enterprise from scientific research by assigning to each an exclusive interest in answers to the disparate questions of *how* and of *why*? For to my knowledge, for example, no philosopher from Thales to Whitehead ever seriously asked: "*Why* does man know?", but every philosopher from Thales to Whitehead has always asked himself the question: "*How* does man know?" The solution, suggested by Dr. More in a terse tessera, appears attractively neat and pat. But even cursory examination proves it to be deceptively simple and grossly inadequate.

Again, in a pretentious dissertation on *The Methodology of Pierre Duhem* [New York, Columbia University Press (1941), pp. 171-172], distinguished French physicist and philosopher of science in the nineteenth century, Armand Lowinger rhapsodizes in unclear modern dithyrambs as follows:

> . . . While, therefore, science in its limited aspect as a professional scientific work, a highly technical activity taking account only of the canons of scientific research, is not concerned with the reality postulated by ontology, science as an historical activity of the human Spirit, which has its roots in reality, cannot be entirely without relationship to the reality postulated by ontology. If ontology cannot guarantee that science will ever attain a perfectly adequate account of reality, it can at least assure us that it asymptotically approaches it. . . . Not to do so would be to belie an indefeasible fact in the human situation—that the human Spirit is one and that the different activities in which it expresses itself must in the end arrive at the same conclusion.

Thus far Dr. Lowinger. But what does all this mean? So far as I can see, it means two contradictory things: (1) that there is nothing in the recognized methods of scientific research to promise the deliverance of a metaphysical or ontological interpretation of the universe, and (2) yet somehow or other, although completely unequipped for the task, science will achieve just that type of ontological interpretation at some indefinite date in the roseate future, sufficiently near to appear vaguely attractive, and sufficiently remote to evade all astringent criticism. But such a subterfuge is to dissolve the problem in a mist of mysticism. It is not to solve the problem in clear, conscientious analysis of its inescapable elements.

And Mr. W. H. Watson, eminent Canadian physicist, in his very interesting book *On Understanding Physics* [Cambridge, at the University Press, (1938), p. 6] puts the problem, as he sees it, in this way:

> It must be the nature of philosophical problems that their solution does not have to await the becoming of facts. It must be irrelevant to philosophy what actually happens in the world. If this were not so, how would philosophy differ from the sciences whose business is with facts? . . .

It is painfully obvious that Professor Watson is puzzled, and seriously so, over what may possibly be the proper role and function of philosophy in the integral career of human intelligence. For if science comprises by right of eminent domain the entire realm of facts, then what is left about which philosophers may philosophize?

But Mr. B. A. G. Fuller, tracing the influence of Herbert Spencer's thought on contemporary attitudes [*A History of Philosophy*[2] (New York, Henry Holt and Co., 1946), Volume 2, pp. 406-407] unconsciously but clearly takes issue with Dr. Watson when he remarks that:

> The province of knowledge, and therefore of science and sound philosophy, is the field of phenomena. The methods of philosophy and of science are the same. Both seek by observation and induction to reach general ideas descriptive of the behavior of the sensible world, and thus to achieve unified visions of the totality of the field in which they work. The difference between science and philosophy is only one of scope. The sciences observe and infer within limited and sharply defined ranges of research. Philosophy seeks to unify the concepts arrived at by the special sciences and to weave them into one consistent whole, in which everything that happens in the universe shall have its part and receive its final explanation.

Thus far Mr. Fuller's persuasive analysis and attractive portrait of philosophy as master-mind and coordinating super-secretary of the disparate natural sciences, so preoccupied with increasing their findings as to be without the leisure necessary to keep their files in order. But is this picture really adequate? Are the varying dimensions of their respective problems the distinguishing characteristics of science and of philosophy? Does astronomy forfeit its established status as a reputable science when it dares to investigate and discuss extra-galactic universes, thousands of light-years in diameter? Does biology automatically become philosophy when it entertains the hypothesis of *cosmic* evolution? And does philosophy abandon its position when it narrows the focus of its attention upon the specific morality of a single abuse of one's vocal chords in the utterance of false, mendacious speech? The more one examines this supposed criterion of relative dimensions and scope by applying it in decisive tests to recognized departments of both philosophy and of science, the more readily and clearly one sees that the suggested criterion, although deceptively simple and attractive, is woefully inadequate.

But the late Sir James Jeans in his very popular volume on *Physics and Philosophy* [New York, The Macmillan Company (1943), p. 17] told us, and many of our contemporaries still echo his words of leadership, that:

> In whatever ways we define science and philosophy, their territories are contiguous; where science leaves off—and in many places its boundary is ill-defined—there philosophy begins. . . . Contiguous to the department of physics on the scientific side of the boundary lies the department of metaphysics on the philosophical side—that department of philosophy which lies "beyond physics. . . ."

Thus far the late Mr. Jeans.

But just when one thinks that most at least agree that there *is* a philosophy and that there *is* a science, and that the problem of our times is to work out the true relationship between them, there appears on the scene one Mr. J. A. Gengerelli who in a vehement discussion concerning "Facts and Philosophers" in *The Scientific Monthly* [54 (1942) 431-440, p. 439] remarks that:

> It is clear that there is no place where science stops and philosophy begins. If we insist upon creating an artificial dichotomy of this sort, we do ourselves a great deal of mischief; it can only result in bad scientific method and worse theory. If we would gain an insight into the problem of the relationship between science and philosophy, we have but to ask ourselves this question: What does the human mind attempt to do in its efforts to understand the universe? The answer is that it tries to embrace as many experiences as possible under the fewest possible rubrics with the minimum number of contradictions. This is the basic impulse behind the efforts of all thinking men, be they physicists, biologists, or metaphysicians. . . .

And yet Mr. Gengerelli's attempt to identify on a single level of intelligence both philosophy and science is not permitted to go unchallenged. For the Reverend Paul J. Glenn, as mouthpiece for a venerable tradition of scholastic philosophy (of the campus variety), asserts with a curious admixture of assurance and hesitation, that:

> There is a point where laboratorian physics and cosmology meet and even overlap. Rather, there is a series of such points, an irregular and intricate frontier. Hence it is not easy to determine, and

to express in a few terse words, the distinction which indicates where the physicist should stop and the cosmologist begin. But the difficulty of establishing a clear line of demarcation is no reason for denying its existence or utility, or, as the current fashion is, for ignoring it altogether. . . .

And Dr. Glenn is right in assuming that the current fashion is to ignore the distinction altogether. For Dr. Philipp Frank, spokesman in America for the Vienna School of Logical (or Empirical) Positivism ["Physical Theories of the Twentieth Century and School Philosophy", *Between Physics and Philosophy* (Cambridge, Mass., Harvard University Press, 1941), 55-103, pp. 102-103] maintains that:

> . . . it is therefore not necessary beside the thriving tree of science to assume a sterile region in which reside the eternally insoluble problems in the attempted solution of which men have been only rotating about their own axes for centuries. There are no boundaries between science and philosophy.

The foregoing cross-section of contemporary representative opinions provokes the inevitable inquiry: Where are we? and provides, I think, the elements of an answer. We are, first, I take it, confronted by a recognized problem that is current, critical in cultural importance, and tantalizingly complex. And we are, secondly, in relation to that problem and its necessary resolution, somewhere along the road at an intermediary stage of painful confusion concerning the basic issues involved. It is obvious that there is no unanimity of critical and informed opinion concerning the authentic role and function of scientific research in the acquisition of truth. It is also obvious that there is even less agreement among critical and informed persons concerning the genuine scope and function of philosophy in the total task of human intelligence. And there is consequently no established formula, acceptable to all alike, that expresses clearly the relationship and reconciliation between them. The task that remains is clear: (1) there must be a long process of patient, cooperative, and conscientious analysis of the actual methods and genuine results of scientific research. Scientists must seriously and candidly reply to the question: Precisely what and *only* what does science, *qua* science, really know and soberly hope to discover? (2) There must be a long parallel process of patient, cooperative, and conscientious analysis of the actual methods and genuine results of philosophical inquiry. Philosophers must seriously and candidly reply to the question: Precisely what and *only* what does philosophy, *qua* philosophy, really know and securely hope to discover? Only when the answers to these questions have been honestly given, independently of all privilege, prejudice, and prepossessions, and conjointly subjected to severe mutual criticism, can the modern mind hope to do for philosophy and science what the medieval mind succeeded in doing for reason and revelation, that is: work out an acceptable philosophy of science that achieves a reconciliation of both by assigning to each what each deserves and depriving neither of what each undoubtedly merits.

It would, of course, be presumptuous at this early date to present *the* final and definitive philosophy of science that achieves this desired reconciliation. The time is far too premature. For so far as I know, there is no man alive so thoroughly saturated with the spirit of science and so profoundly imbued with the philosophic mind, as to be able to manufacture single-handed the formula for reconciliation. But one may perhaps be permitted to suggest to interested technicians, researchers, and science teachers—as a preliminary schema and tentative framework—*a* working philosophy of science.

I take it, then, that the question: "What is an *x*?"—be it star or stone, flower or snow-flake, ant or elephant, has meaning. So much meaning, in fact, that it serves as the point of departure for two distinct but complementary courses of investigation: the physical sciences, for example, and the philosophy of nature. For the question is susceptible of two distinct but convergent emphases. One may, for example, accentuate the first member, and ask: "*WHAT* is an x?", and discover the answer in the ascertained results of the investigations of natural science. For *only* in a reputable scientific dictionary will you discover the recognizable answer that you desire to the question: "*WHAT* is a star or a stone or a flower or snow-flake or an ant or an elephant?". Or, alternatively, one may with equal right emphasize the second member of the question, and ask: "What *IS* an *x*?", and seek the answer in a reasoned interpretation of an acceptable metaphysics and philosophy of "*is*-ness" or being. For *only* in a reputable philosophical treatise will you discover the recognizable answer that you desire to the question: "What *IS* a star or a stone or a flower or snow-flake or an ant or an elephant?".

In each instance a distinct objective and an appropriate methodology of investigation are required. Detailed analyses on impartial, historical grounds will, I think, reveal that the natural sciences examine the phenomena of material change precisely in terms of its ascertained and correlated *variables*. For science circulates from controlled experiments and their recorded measurements through mathematical manipulation of the resultant data to the tentative construction of a more simple unitary hypothesis, and then patiently returns to an experimental test of its implied predictions. Such research proceeds by a descending and continuously refined analysis to accumulate in increasingly compressed formulae a progressively more comprehensive description of the *physical structure* of the universe and its dynamic parts, and thus enables man to *employ* for his material welfare the things of nature *as they actually are in point of fact*, or *as they may very well be in terms of some preferable hypothesis*. A parallel analysis on the same impartial historical grounds will, I think, disclose that a realist philosophy of nature undertakes to examine the very same and identical phenomena of material change precisely in terms of the *invariable* but interconnected concepts and absolute laws of reason and reality. For such realist philosophy rises from adequate observation and certified information through relevant reflective analysis to reasoned interpretations of material reality in coherent accordance with the total realm of experience. Such philosophical reflection proceeds by an ascending and continuously rarified analysis to penetrative insights into the *metaphysical* structure of the universe and its dynamic parts, and thus empowers man to *understand* in a comprehensive and coherent synthesis how the things of nature can *Be as they actually are in point of fact*, or *as they may very well be in terms of some preferable hypothesis*.

The same physical reality of a common, shared experience forms the material object or subject matter of both science and philosophy. But their respective formal objects, or the precise points of view under which each study examines an identical subject matter, are distinctly different but mutually complementary in the indispensable construction of a total knowledge of total reality. The natural sciences successfully describe *WHAT* an *x* is in the empirical terms of centimeters, grams, seconds, force-fields, and point-events in a space-time region of probability, in accordance with the *physical* principles of mechanics, thermodynamics, electromagnetics, relativity, quantum theory, and wave-mechanics. Philosophy successfully defines what an *x IS* in the trans-empirical but intelligible concepts of essence and existence, act and potency, substance, accident, subsistence, and causality, in accordance with the *metaphysical* principles of contradiction, sufficient reason and causality. Neither reply may supplant the other in the logic of a response to the precise nuance of the question that each undertakes to answer by methods peculiarly its own. But each answer supplements the other in such a way as to make possible—what an integrated intelligence requires—a comprehensive reply to *both* accentuations of the original inquiry: "What is an *x*?"—be it star or stone, flower or snow-flake, ant or elephant.

No philosopher, precisely *qua* philosopher, could ever tell you in units of distance per units of time the law of free fall of bodies in empty space. And so far forth no metaphysician, precisely *qua* metaphysician, is by right of his own resources a master in control of the motions of material bodies in our physical universe. And no scientist, precisely *qua* scientist, could ever tell you in recognizable terms satisfactory to the demands of human intelligence exactly what motion *IS*, and how such motion can actually *BE* and exist. And so far forth no scientist, *qua* scientist, is by right of his own resources free from the paralyzing entanglements of the paradoxes of Parmenides or of Zeno. I quite agree that if *all* men were genuine philosophers, but *only* philosophers, we would all know with intellectual composure and assurance precisely what motion *IS* in terms of being, but we would be powerless to avert its devastating impacts nor control its beneficial services toward the material improvement of society and civilization. And I leave it to you to judge what possible contributions to culture and civilization and society could be made, if all men were top-flight scientists, but *only* scientists, and irretrievably snared in the monistic and monolithic immobility into which the unresolved paradoxes of Parmenides and Zeno would inescapably propel them. We all recognize that the physiological mechanism by which normal binocular vision is achieved in the organs of human sight is both delicate and intricate, and consequently as difficult to maintain as it is easy to impair. What is really needed is a philosophy of science that will allow a genuinely binocular vision of the same material world: one, empiriological in terms of natural science, and the other, philosophical in terms of metaphysics. In no other way can men see things steady and see them whole.

Once upon a time three independent research investigators descended in quick succession upon the household of a certain Mr. Smith, Baptist, Democrat, and wheat-farmer in Chilicothee, Idaho. The first investigator represented the United States Department of Agriculture, and was conducting a farm-to-farm survey in order to tabulate the total number of wheat-croppers in the State of Idaho. His key inquiry was: "Mr. Smith, are you or are you not a wheat-farmer?". The answer was in the affirmative. The second researcher was deputed as an agent for the Federal Council of Churches to take a census of the number of Idaho citizens who acknowledged themselves as Baptists in religious affiliation. His basic question to Mr. Smith was: "Are you or are you not a Baptist?". The answer was in the affirmative. The third research investigator was a travelling representative from the Political Action Committee of the CIO, interested in determining the gross total of declared Democrats in the area. His proper question was: "Are you or are you not a Democrat?" The third answer was likewise in the affirmative.

Now it so happened that these three independent investigators met that same evening in the lounge car of the west bound Pullman, and over coffee and cigars fell to discussing the estimable Mr. Smith of Chilicothee, Idaho. The Department of Agriculture agent remarked innocently enough that Mr. Smith was a wheat-farmer. But the Federal Council of Churches' representative refused to agree. On the contrary he asserted with much vehemence and violence that Mr. Smith of Idaho was a Baptist. And—as you may already have anticipated—the PAC of CIO representative contradicted them both and argued implacably that the same Mr. Smith was a Democrat. For each assumed blandly and bluntly that if Mr. Smith was one of the three alternatives, he could not possibly be with equal right and truth either or both of the remaining two. Now it goes without saying that the description of wheat-farmer is of no significance in a political vocabulary, nor is that of Baptist of any importance in an agricultural report, nor is that of Democrat of any relevance in a memorandum to the Federal Council of Churches. The terms are certainly not synonyms. The terms are certainly not interchangeable. But they are interconnected in a total understanding and description of citizen

Smith. For while the investigators fumed in trying to force Mr. Smith into one or other of the three exclusive categories, the Idaho gentleman in question calmly continued to exist and to eke out a living at one and the same time as Baptist, *and* as Democrat, *and* as wheat-farmer.

The moral is, I venture to suggest, decidedly obvious. The real world of our human experience, like the redoubtable Mr. Smith, is far too rich and far too complex to be comprised within the narrow confines of some one preferential system of descriptive categories. There are, I submit, alternative vocabularies, each of which is irrefutably valid, irreducibly different, but not irreconcilably opposed, rather mutually complementary. It may at the moment be far more interesting and profitable to you to describe an atom or an organism as a configuration of electrons or as a colony of cells in thermodynamic equilibrium. And it may at the moment be far less interesting and downright boring to hear the same atom or the same organism described as a substance, composed of two substantial coprinciples, known as prime matter and substantial form in the technical jargon of philosophy. But the point at issue is not interest nor immediate profit, but—if we may take at face value the legend which has ever been emblazoned on the banners of science—the point at issue, I repeat, is *truth.* If Mr. Smith is *also* a Democrat, it is both right and true to think so and to say so without infringing in the least degree upon the rights of others to say truly that he is also a wheat-farmer and a Baptist. And if an atom is *also* a substance and a compounded essence, it is both right and true to think so and to say so, without in the least degree offending those whose preferential interests induce them to favor an alternative vocabulary of electrons, protons, neutrons, *et cetera.*

And in the total gamut of human experience the time may come—in an election year, for example, in a period of serious drought, or on the occasion of a religious controversy—when the emphasis on Mr. Smith may shift in relative importance from Democrat to wheat-farmer to Baptist and back again. And likewise in the total context of human experience occasions may arise when the accent on the atom or on the organism may shift in relative importance from nuclear structure or cell structure to essential, substantial structure and back again. To know nuclear structure is to know the basic formulae for an atom bomb. And that knowledge, I quite agree, is of colossal social significance. But to know the essential structure of an atomic substance is to know the basic formula of the irresistible evidence for the existence of God. And that knowledge is, I submit, also of colossal social significance.

To all philosophers I sincerely recommend a try at the painstaking performance of genuine scientific research. And to all technicians, researchers, science students and science teachers I suggest an equally impartial attempt to appreciate and, if possible, to absorb and assimilate the assured insights of a responsible metaphysics. For if you profess to ignore the problem and neglect to acquire by conscious selection a satisfactory philosophy of science, equipped to withstand successfully all able criticism, you are bound to become the victims of your own insouciance by unconsciously imbibing what chance philosophy happens to infest the atmosphere at the moment. And nothing is more dangerous and less scientific than the sorry spectacle of a reputable Doctor of Science, confidently mouthing as certified truth the platitudes of an uncritical and uncriticised philosophical illusion. To save the soil, rotate the crops.

Biology

LIFE IN PROTOPLASM

William D. Sullivan, S.J.

Protoplasm was probably seen for the first time by Rosenhoff (1775), for at about that same time the amoeba was first seen, though unidentified. O. F. Muller (1782) was the first scientist to describe the amoeba. One-half century later Dujardin (1835) is believed to have uncovered the living substance in cells which he called *sarcode*. He described the protoplasm of lower animals as that substance which has the properties of life. To him, at least, is credit due for first realizing that the 'moving material' in living cells is the substance which is alive. His description is as accurate today as any other description which has been given since his time.

Von Mohl (1846) noted a similar substance in plant cells which he called *schleim*, or protoplasm as we now know it. He applied the term protoplasm to the cellular substance of plants and showed that the animal *sarcode* of Dujardin was the same substance as that of plant cell. The word 'protoplasm' was first introduced into biological terminology by Purkinje (1839) in describing the living substance of animal eggs and embryos.

According to the findings of Von Mohl and Dujardin, a superficial description of protoplasm has been given as follows: a jelly like substance, usually soft and fluid, though sometimes tough and highly elastic, glutinous and fibrous, taking up water with avidity; it is translucent, of grayish cast, slimy, hyaline, free flowing, but with relatively high consistency, visible structures rarely missing, and with no definite pattern of granular distribution. A definite pattern of the latter was to be found by later investigations in the Echinoderm eggs, though in no other cells.

Two regions of protoplasm were first noted in the amoeba: the *endoplasm*, or granular zone and the *ectoplasm* or clear hyaline zone surrounding the endoplasm. What was later to be described as the *plasma membrane*, surrounding the entire cell, was only surmised in early investigations.

Biologists are ever trying to distinguish between the finer differentiations in the protoplasmic structure of the cell. Strasburger (1895) recognized two constituents, *kinoplasm*, or the active protoplasm responding to stimuli and *trophoplasm*, the nutritive protoplasm feeding the kinoplasm. Scarth and Lloyd (1930) revised this theory and description of protoplasm. Seifriz (1936), first to use the Spierer lens in examination of protoplasm, identified the two parts of protoplasm as *phaneroplasm*, the dispersed phase having a brilliant appearance under the microscope and the *cryptoplasm*, the non-brilliant substance, apparently the matrix and the streaming part of protoplasm. In spite of later terminology, the description of Mast (1926) is used almost exclusively. The two regions of protoplasm as seen in the pelomyxa, he describes as *plasmagel*, the non-flowing and *plasmasol*, the inner flowing part of protoplasm (Sullivan, 1949).

The protoplasm of almost every cell includes *cytoplasm* and *nucleoplasm*. Because examination to date has failed to reveal the presence of nuclei in filterable viruses and certain bacteria, it is impossible to say that every cell possesses a nucleus. The nucleoplasm is of a slimy, elastic, sometimes quite thin, sometimes quite thick material, physically like to the cytoplasm, and influenced by the latter. It, in turn, influences the cytoplasm, if it does not, to some extent, control the cytoplasm. A nuclear membrane separates it from the cytoplasm. During cell division this membrane breaks down in such a way that the nucleoplasm and cytoplasm are intimately associated. It is possible that at this time the cytoplasmic constituents are transferred to the nucleoplasm and thus transmitted as hereditary characteristics.

Thirty-four elements, in all, have been discovered in protoplasm. C, H, O and N are present in such compounds as proteins, fats and carbohydrates. S and P have also been found, but to a less extent. Some of the not so common elements found in protoplasm have been enumerated as follows: Na, Ca, K, Li, Rb, Sr, Mg, Al, Fe, Cu, Mn, Zn, Co, Ni, As, Si, Sn, V, Hg, and Au. Through experimentation, the following have been noted: Na is not necessary for the growth of some plants; Li, though found in marine animals, is not an universal constituent of protoplasm (Fox and Ramage, 1931); Ca, characteristic of the higher plants, has been found lacking in the nutrient material of molds (Rippel and Stoess, 1932)[1]; Rb is found in all human tissue except bone (Fox and Ramage, 1931; Sheldon and Ramage, 1931); Sr is typical of marine invertebrates (Webb, 1937); Mg and Ca differ in percentage in various organisms (Uvarov, 1928); Al is more commonly found in plant tissues than in animal tissues (Underhill and Peterman, 1929); Fe is a necessary constituent of vertebrate blood cells, and is sometimes found in the invertebrates (Warburg, 1930); both Fe and Cu influence oxidation in cellular respiration, and Cu is found in the blood cells of the invertebrates (Yosikawa,

[1] Mast and Pace (1939) dispute the findings of these men.

1939; Schultze, 1940); Mn influences the growth of molds and is also found in the ant (Vinogradov, 1937); Zn has been found in many of the lower animals (Galtsoff, 1934; Akao, 1939; Eggleton, 1940); Co is present in larger quantities than Ni in animal tissues, but in plant tissues the Ni content is greater (Bertrand and Macheboeuf, 1925); As and Si are present in the higher organisms, man included (Tabulae Biologicae, 1926); Sn is present in some of the lower marine forms (Webb, 1937); Au in the soil is absorbed by plants and stored in the horns, hoofs and hair of deer on eating these plants (Némec, Babicka and Oborsky, 1937); V has been noted in the blood cells of Ascidians (Webb, 1939); and Hg has been found in some of the organs of man (Stock, 1940).

In general protoplasm is composed of three-quarters water which differs in various organisms in maximum and minimum content. The maximum is found in the jelly-fish where it is said to be 99.8%. In paramecia it is said to be about 79% (Iida, 1940) and in man about 66%. The human brain and kidney cells are composed of 80% water, which is higher than that of the blood (Close, 1933). The minimum water content is found in dormant seed where it is said to be in the vicinity of 8%. Some seeds are known to have lived in this dormant state for more than one hundred years with the water content remaining approximately the same. The average seed contains from 15 to 35% water. It is still doubtful whether or not some of the water content of cells is bound chemically with other substances in the living cell.

The remaining constituents of protoplasm are fats, carbohydrates, and salts. The percentages of the different solid contents of protoplasm from the sea-urchin, *Paracentrotus lividus* (Ephrussi, 1933) and man (Bouchard, quoted from Tschermak, 1924) are noted in Table I.

TABLE I

Compound	*Paracentrotus lividus*	*Man*
Water	77.3%	66%
Protein	15.18%	16%
Fat and lipoid	4.81%	13%
Carbohydrates	1.36%	.61% (This figure is from a newborn child.)
Ash	.34%	5%

According to Heilbrunn, the fats and ash have a high percentage due to the presence of many non-protoplasmic constituents of the cell. The same author notes that the content of carbohydrates in plants are sometimes noted to be as high as the protein content.

The cell is said to be a physical mixture rather than a chemical compound in the true sense. There is no direct evidence of great complexity in protoplasm, but if viewed dynamically, considering the extreme diversity in the form and behavior of organisms, and reflecting for a moment on the manifold activities of a single cell, such as the brain cell in man, it would seem that protoplasm must be a highly complex and superbly organized system.

Complexity in structure leads to the question whether or not this structure or any other property of protoplasm is characteristic of it alone, i.e., of life. It is recognized that living material is in certain respects quite distinct from non-living matter. Further a definite structural organization or specific constituent of protoplasm may and has been said to be responsible for this distinction, but whether it is or not cannot be known for certain. Attempts to distinguish between what is living and what is not living in protoplasm have been futile. However, fat droplets, starch granules, etc., are generally considered dead matter. Water is certainly not living when taken alone but it has been said that it is so intimately associated with the other constituents of protoplasm that it is said to be alive in a sense. These protoplasmic centers where metabolic activity is greater—centers of organic synthesis such as the chloroplasts where sugar is formed and pyrenoids where starch is formed—would appear to merit the title of living as much as, if not more than, any other part of the cell; but it is these very processes and the substances involved which are capable of the strictest physical-chemical interpretation. Chlorophyl may be viewed simply as a catalyst or a 'color screen' and thus robbed of all vital significance.

Hanstein distinguished between the living protoplasm, the *active* protoplasm, and the non-living, the *passive* protoplasm. Sachs called the living protoplasm *energid*, and the non-living, he called the *energid products*. Other authors have mixed philosophy with science and postulated certain vital bodies which give protoplasm the properties of life. These vital bodies have been conceived as gigantic molecules, called *biogens;* all other constituents of the cell are non-living. Though it was Verworn (1903) who first proposed the term 'biogen', it was Pfluger (1875) who first proposed the idea of a 'living protein' in protoplasm. Chemically the theory of Pfluger and Verworn is not sound today. Pfluger thought that the oxygen molecule produced in the metabolic processes of the cell combined with this 'biogen' in such a way that its nitrogen and carbon atoms were bound together in a linkage similar to cyanide. Heilbrunn (1943) points out that sufficient proof against such a theory consists not in showing absolutely that Pfluger and Verworn are incorrect, but rather in the fact that the "modern ideas of protoplasmic metabolism are hard to reconcile with it".

The general belief among physiologists today is that protoplasm does not consist in a single complex chemical compound but rather in a mixture of chemical compounds physically situated in the cell so as to become a living organism. On the other hand Spencer postulated a 'physiological unit' as the life giving principle of protoplasm; Altman believed that *bioblasts* were the unit of life. These 'bioblasts' were minute granules which were supposed to be living organisms of the most minute character. Darwin called them *gemmules*, de Vries called them *pangens* and Haeckel said that they were *plastidules*. In modern genetics they are *genes*, though before this terminology came into being, Weisman called them *biophases*. Nagli's *idioplasm* theory, though speculative like all the others mentioned above, has the advantage of resting on a chemical foundation in harmony with known facts. This theory accounted for hereditary traits by a specific molecular orientation, and lead to the conception of elementary units of structure or aggregates of molecules which Nagli termed *micelles*. These 'micelles' were destined to play a prominent role in the subsequent theories of protoplasm and colloidal structure.

In his theory of life in protoplasm, Seifriz (1936) names three possibilities: 1) There is present in protoplasm an ultimate vital substance which may be of an hyaline material; 2) protoplasm is a mixture of substances, the individual components of which taken alone are non-living, but combined constitute a living substance; 3) in protoplasm the physico-chemical constituents are non-living, and life comes from an extra-mundane principle. The first of these possibilities connotes a substance protein in nature. Many scientists are interested in the importance of the protein substances in protoplasm, and are coming to the conclusion that the ultimate living matter is some protein complex. The constituents of this complex are probably of the nature of enzymes, or organic catalysts. According to Leathes (1925) proteins alone display signs of life; carbohydrates, fats and salts do not.

As opposed to all these theories, the viewpoint of some of the authors rests on the assumption that no distinction exists between the living and the non-living in protoplasm. The cell is living because it is an organized system, the components of which are lifeless when considered separately; associated in a coordinate system, they are living. Hopkins says that the cell's life is the expression of a particular dynamic equilibrium which obtains in a polyphasic system. Life is the property of the cell as a unit. Wilson says that life is the collective name for the sum total of activities of the whole system. Mast claims that the structure of protoplasm must involve the cell as a whole, all the constituents of which are organized into a living working unit.

From this viewpoint any substance acquires the property of life when it becomes part of a living system. Protoplasm is a unit, and is not made up of living and non-living parts. All the parts are essential for the living substance. This does not mean to say that the opposing viewpoint of the 'living protein' does not require the presence of these other particles or granules found in all protoplasm. Rather, they who hold for the 'living protein' or some one substance or complex of substances which ultimately represent living matter, say that this substance can maintain life only when this substance is surrounded by and intimately associated with its environment of water, proteins, fats, carbohydrates, salts and all other necessary constituents of protoplasm.

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Chemistry

NOTES ON TEACHING THE GRIGNARD SYNTHESIS

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The Grignard Synthesis is, in most of its applications, a carbon to carbon condensation in which a variety of functional groups can be produced. This gives an extremely versatile reaction which threads its way through almost every chapter of elementary organic chemistry. As a condensation reaction, it is of the utmost importance. It takes its place with the nitrile, aldol, Wurtz-Fittig, acetoacetic and malonic ester, and the Diels-Alder Diene Syntheses. All of these provide methods for building carbon skeletons. Most other organic reactions merely add desired functional groups to a given structure, or degrade its carbon skeleton. But condensations produce the carbon skeleton. Familiarity with the Grignard Synthesis is presumed in the discussion to follow. It is written with a view to deepening our insight in the synthesis as a whole.

Three reactions are commonly distinguished in the Grignard Synthesis: 1) the preparation of the reagent, 2) reactions of this to produce desired products, and 3) hydrolysis of the inorganic residues.

The preparation of the Grignard Reagent (1) is a specialized case of the preparation of metal alkyls, given by

$$n\ RX + 2\ M \longrightarrow RnM + MXn \qquad (1)$$

where R designates alkyl (sometimes aryl) radicals; M, the metal; n, the valence of the metal; and X, any halogen except fluorine. When the metal is divalent, as with Mg or Zn, a further equilibrium can be written

$$R_2M + MX_2 \longleftrightarrow 2\ RMX \qquad (2)$$

There is evidence that the Grignard Reagent can be written as RMgX under certain experimental conditions. For convenience, it is generally so written.

The second general reaction (2) can be divided in the following way: a) reaction with active hydrogen to decompose the reagent into hydrocarbons and inorganic salts, b) reaction with alkyl halides to effect hydrocarbon condensations, c) reaction with carbonyl and nitrile functions for the production of primary, secondary and tertiary alcohols, aldehydes, ketones, and di-thio, sulfinic and carboxylic acid structures, and d) the production of other metal alkyls, such as zinc alkyls, tetra-ethyl lead and silicone structures. The last division is merely listed for the sake of completeness.

The active hydrogen reaction (a) is a decomposition reaction. It can be used for the determination of moisture when the volumes of liberated hydrocarbon gases are measured. Similarly active hydrogen in given compounds can be measured. Hydrocarbons with the same number of carbons as are had in the radical of the reagent are produced. The strengths of solutions of Grignard reagents can be measured by acid titration. Accordingly the reaction warns against moisture in reagents and apparatus, if the use of this reagent is to be successful. The effect of moisture may also be negative catalytic in nature.

The reaction with alkyl halides (b) resembles the Wurtz Condensation. But it offers certain advantages over the latter. In the Wurtz Condensation, both alkyl groups have to be identical if maximum yields are to be obtained and by-products minimized. Condensation to unsymmetrical products in the Wurtz Reaction are generally not feasible for alkyl reagents. In the Grignard Synthesis the difficulty is avoided to a great extent, because the metal is already incorporated in the hydrocarbon structure of the Grignard Reagent. In the last analysis, though both reactions seem to involve free radical mechanisms which make for random concentrations of products and by-products, the Grignard Reagent because of its structure, RMgX, makes for a unique or single product.

The use of the Grignard Reagent with carbonyl and nitrile functions (c) is by far the best known of the Grignard Reactions. It involves the condensation of the radical in the reagent with carbon, while the inorganic part of the reagent becomes attached to oxygen or nitrogen: R C OMgX or R C=N MgX. The inorganic elements are hydrolyzed later (3). This condensation makes for branching, or at best, lengthening of the carbon chain.

In most of these synthetic reactions (c), an advantage is gained by emphasizing the structures with which the Grignard Reagent reacts, rather than by emphasizing the products desired. The advantage is that of deepening the student's knowledge of common organic structural theory. It requires the arrangement of a syllabus of Grignard Syntheses in teaching practice. The syllabus is based on the lack of time to conduct extensive reviews of matter which extends through many chapters, and it substitutes for formal review as such, the exercise of correlation. For, in the opinion of the author, correlation includes review and surpasses it in value. Illustrations to follow might clarify the problem.

To emphasize reactants rather than products is tantamount to emphasizing chemical properties under a given topic rather than synthesis.

In a fundamental discussion of alcohols, it seems better to postpone the Grignard Synthesis of primary, secondary and tertiary alcohols for students who do not recognize aldehyde and ketone structures, and to study these alcohols as chemical properties of the aldehydes and ketones. This reviews and correlates the alcohols.

But in studying the synthesis of aldehydes and ketones, it is convenient to anticipate and borrow the nitrile structure. At this point, the synthesis of aldehydes in which the carbonyl group is terminal, can be looked upon as a special case of the synthesis of ketones. Hence, while higher nitriles, acetonitrile excepted, serve the Grignard Synthesis of ketones, formonitrile, HCN, provides the necessary terminal carbonyl group for aldehydes. In this way the structural significance of these reactions is emphasized. It seems that aldehydes and ketones have much more in common in their synthesis than they do in their chemical properties.

The value of the Grignard Synthesis in this chapter lies in the fact that it provides from readily available reagents the best method of preparing principally unsymmetrical ketones, along with certain aldehydes and branched ketones. Symmetrical ketones, while also susceptible of Grignard Synthesis, are generally available through the pyrolysis of organic salts or the catalytic decomposition of acids at relatively high temperatures. Using a mixture of these acids or their salts, in the hope of unsymmetrical products, is to invite a mixture of products in correspondingly low yield.

At this point, it is not advisable to teach the synthesis of aldehydes and ketones from esters by the Grignard Synthesis. The poor student is in the midst of the most difficult early chapter in the treatise. The ester structure may have to be anticipated. The acetal structures involved have to be cited. It will be shown that there is definite advantage in taking this up under the chemical properties of esters to be studied later.

Coming now to the chemical properties of the aldehydes and ketones, the synthesis of primary, secondary and tertiary alcohols comes up as already mentioned: primary, by the reaction of formaldehyde; secondary, by the reaction of higher aldehydes; and tertiary, by the reaction of ketones—with the Grignard Reagent. Correlation and review of alcohols is obvious.

It is to be noted that in making the primary alcohol one carbon atom is added to the carbon content of the radical in the reagent. Some authors have considered this to be a reduction of an aldehyde to an alcohol. This confuses the whole question of the reducing power of the Grignard Reagent. For, we would certainly not consider formaldehyde to be reduced to butyl alcohol by a reaction with normal propyl magnesium bromide, nor to ethyl alcohol with methyl magnesium bromide. The only terminal state of such reduction in the true sense of the word can be methyl alcohol. It is not produced here.

The addition of two carbon atoms to the Grignard radical is to be had on its reacting with ethylene oxide. A recent and reasonable literature survey by the author has uncovered no similar reaction for adding three or more carbon atoms by means of higher cyclic ethers or alkylene oxides. Indeed, reactions with cyclic ethers, higher than the butyl, would not be expected, due to the difficulty of rupturing the carbon to oxygen bond. With alkylene oxides of various types, interesting structural possibilities arise.

In coming to the chapter on acids, the action of carbon dioxide on Grignard reagents to produce a C_{n+1} acid structure comes up. It gives incidentally further reason for protecting the reaction from air in the laboratory with a calcium chloride and soda lime tube at the outlet of the apparatus (vs. HOH and CO_2). It shows structural correlation with di-thio and sulfinic acid syntheses (CO_2, CS_2 and SO_2 respectively).

Passing on to the chemical properties of the esters as acid derivatives, it is discovered that an excess of ethyl acetate, for example, with Grignard Reagent supplies a ketone that corresponds to the acyl radical of the ester and to the Reagent. Again, the special case of terminal carbonyl, aldehyde, is supplied by esters of formic acid. Here the hemi-acetal structures are encountered as intermediates, thus supplying in part a review of matter from an earlier chapter.

But why the excess of ester? Answer: because an excess of Grignard Reagent would react with the aldehyde or ketone produced to yield secondary or tertiary alcohols. Indeed, such alcohols can be synthesized from appropriate esters by using two mols of Grignard Reagent to one of ester at the outset. In upshot then, this gradually built attack culminates in substantial correlation and review of the Grignard reaction all of the way back to the alcohols. But small addendum is needed to complete the review.

By this time some astute students are convinced of the versatility of the Grignard synthesis; feel that, when mastered, it will be the key to a "pass" in quizzes and examinations; nor do they keep it a secret from their less gifted friends. This is no undue publicity. In due time the student is warned to be prepared on his syntheses with and "without benefit of Grignard"!

The idea that the Grignard Synthesis is a good master key to common organic structures can be dangerous. Obviously the first member of a given homologous series cannot be synthesized by a carbon to carbon condensation reaction. Further, a second functional group on any hypothetical Grignard Reagent, which is capable of entering a Grignard Reaction on its own, is calculably unsuited to this synthesis. This is due to the large number of molecules present in a given sample. Presumably molecule number one would form the reagent and number two would react with number one and so forth, so that there would be no reagent present to react with some other species, such as formaldehyde at the end of this "preparation" of the reagent. There might be cases of cyclization where the technique is worked out for using this principle to advantage. But it is no general principle. The Reformatsky Reagent, in which zinc is substituted for the magnesium of the Grignard, can be used in some of these cases. For example, a zinc reagent of ethyl bromacetate is feasible.

One device used to bring out the structural significance of the Grignard Synthesis may be of interest. Consider, for example, the synthesis of the unsymmetrical secondary alcohol, butanol-2. It can be produced by two sets of reactants: the methyl Grignard Reagent on propionaldehyde, or the ethyl reagent on acetaldehyde.

Returning to the third reaction in the Grignard Synthesis (3): the hydrolysis of inorganic residues:—it will be noted that this reaction is unnecessary in the active hydrogen (a) and the Wurtz-like (b) applications. It has been listed because it has to be used with (c), the carbonyl and nitrile applications which have such an extensive scope. Unwary students sometimes try to "ring it in" in the other applications.

$$\text{R}\ \overset{/}{\underset{/}{\text{C}}}\ \text{OMgX} + \text{HOH} \rightarrow \text{R}\ \overset{/}{\underset{/}{\text{C}}}\ \text{OH} + \text{MgOHX} \qquad (3)$$

$$\text{R}\ \underset{/}{\text{C}} = \text{NMgX} + \text{HOH} \rightarrow \text{R}\ \underset{/}{\text{C}} = \text{O} + \text{MgNH}_2\text{X} \qquad (4)$$

The writing of these final reactions depends on the wide variety of conditions which attend them. Textbook practice seems then most inconsistent in this respect. Strong mineral acid, non-committally H^+, is the general hydrolytic agent. Accordingly, the by-product, MgOHX, is generally erroneous, but frequently overlooked. Ammonium chloride is used in delicate decompositions. Similarly, in the decomposition of the imino group, various styles of writing the by-product are employed: MgOHX, NH_3 + MgOHX, NH_4X + MgX_2 etc.

Physics

JOURNALS FOR THE PHYSICS AND CHEMISTRY LIBRARIES

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The question of the best way to invest the limited funds allotted to physics and chemistry journals is often a difficult one for our colleges. A statistical study which appeared recently throws some light on this problem (Herman H. Fussler, "Characteristics of the Research Literature used by Chemists and Physicists in the United States", *The Library Quarterly*, *19*, no. 1 and 2, January and April, 1949).

The title of this article indicates that it was only concerned with research journals, not with popular or teaching journals like *Physics Today* or the *American Journal of Physics*, and these journals were in the fields of "pure" chemistry and physics. Hence engineering journals were also excluded.

The study aimed at determining what journals are most *used* by research men in these two fields. The criterion decided upon for determining the use made of a journal was the number of times an article in that journal was referred to by contributors to a few outstanding American journals in the field. A random sampling was made of the citations in these "key" journals over the space of a year by listing all the references cited on every nth page throughout the volume. Then the results were analyzed statistically. The four years chosen for the study were 1899, 1919, 1939 and 1946. Only the results of the 1939 study will be considered here, since the 1946 study was less reliable statistically due to the smaller number of samples taken, and was undoubtedly influenced by the effect of the war on European publications.

For the 1939 study the following "key" journals were used:

Physics	*Chemistry*
Physical Review	Journal of American Chemical Society
Journal of Applied Physics	Chemical Reviews
Journal of Optical Society of America	Journal of Chemical Physics
Reviews of Modern Physics	Journal of Physical Chemistry
	Journal of Organic Chemistry
	Organic Synthesis

The six most frequently cited journals in each field, as indicated by the sampling procedure outlined above, are given in tables 1 and 2, together with the percentages of references to each journal out of the total number of references (over 1,000 in each case), and the cumulative percentages, that is, the percentage of references contained in the first two, three, four etc., journals in the list.

Journals below the first six each contain less than 2% of the total references in the case of physics and less than 2.6% in the case of chemistry. There are so many of these and the number of references to each is so few that their order is not of great significance.

The only change in the 1946 list in physics, which was based on the *Physical Review* only, was that *Helvetica physica acta* replaced the *Annalen der Physik* in the first six. In chemistry, for which the only "key" journal in 1946 was the *Journal of the American Chemical Society*, three other journals occur in the first six: *Justus Liebig's Annalen der Chemie*, *Naturwissenschaften* and *Transactions of the Faraday Society*. In both chemistry and physics the three most frequently cited journals are the same in both 1939 and 1946.

TABLE 1: MOST FREQUENTLY CITED JOURNALS IN PHYSICS, 1939

Rank	Title	Per cent of References	Cumulative Percentage
1	Physical Review	34.6	34.6
2	Zeitschrift für Physik	9.0	43.6
3	Proceedings of Royal Society	6.2	49.8
4	Annalen der Physik	3.5	53.3
5	Nature	3.4	56.7
6	Reviews of Modern Physics	2.3	59.0

TABLE 2: MOST FREQUENTLY CITED JOURNALS IN CHEMISTRY, 1939

Rank	Title	Per cent of References	Cumulative Percentage
1	Journal of American Chemical Society	27.0	27.0
2	Bericte Deutsche chemische Gesellschaft	9.1	36.1
3	Journal of Chemical Society of London	6.9	43.0
4	Journal of Chemical Physics	4.5	47.5
5	Journal of Physical Chemistry	3.7	51.2
6	Zeitschrift für physikalische Chemie	3.5	54.7

Another interesting fact that comes out of this study is the distribution in time of the references given. This is summarized in table 3, in which the percentages of references contained in journals for different time periods prior to their citation in 1939 are given.

TABLE 3: TEMPORAL DISTRIBUTION OF REFERENCES CITED IN 1939

Number of years prior to citation	Per cent of References Chemistry	Physics
0 - 2	30.22	40.41
0 - 5	51.29	69.42
0 - 10	71.26	88.21
0 - 15	78.74	93.79
0 - 25	85.67	97.17
0 - 50	97.41	99.52

Table 3 indicates that the bulk of the literature used for research purposes in physics is more recent in date than that used in chemistry. As a result a library would only have to have the last 25 years of physics journals to have 97% of all the journals that would be desired for research purposes. To have a corresponding percentage in chemistry 50 years of back journals would be required.

Another interesting result of the study is the national distribution of the references. In 1939 in physics 52.3% of the references were to American journals, 22.1% to German, and 14.3% to British journals. In chemistry the corresponding percentages were 48.5%, 25.0% and 16.0%. Of course these figures are somewhat colored by the fact that only American journals were used for the sampling process.

The number of references to mathematical journals on the part of both physicists and chemists was completely negligible, as was the number of references to books and monographs. However, it is clear that these are very often used in connection with a research paper without giving any explicit reference.

Conclusions

Though there are objections that might be brought against the method used in this study, and though it may not be too significant as regards the finer details of its results, it does seem to point quite conclusively to the presence of three "big" journals in each field, one American, one German and one British. These journals kept their positions of pre-eminence in the 1946 study, and were all, with the exception of the *Zeitschrift für Physik*, in the first five in their field ever since 1899. In 1939 the first three in physics include almost 50% of all references, in chemistry 43%. Hence if any choice is to be made of journals, these three should certainly have first preference in their respective fields. This is interesting because many of our Jesuit colleges in the East subscribe to a goodly number of American Physics journals, but not to the *Proceedings of the Royal Society*, and yet, if we exclude the *Physical Review* and *Reviews of Modern Physics*, this study would indicate that the *Proceedings* would be used just about as much as any other five American journals put together. Hence if the journals are chosen on the basis of the use that will probably be made of them, the *Proceedings of the Royal Society* is a very good investment.

Though these three journals in each field include almost half of the references cited, to include a much larger percentage of the reference requires quite a large number of journals, and it is difficult to distinguish them in importance on the basis of this study, especially since the situation has changed somewhat since 1939. Thus the new German journal *Zeitschrift für Naturforschung* is of growing importance, as are some of the Russian and Japanese journals.

As regards purchases of back issues of periodicals, since in physics 88% of the references were contained in the journals for the past 10 years, and 97% in the journals for the past 25 years, purchases of physics journals more than 25 years old for any reason other than historical interest, would seem to be a bad investment. It would be a much sounder investment to purchase the past ten years of some one of the top journals in the field. In chemistry the same is true to a lesser extent, for here the past ten years would include only 71% of the references.

News Items

BOSTON COLLEGE HIGH SCHOOL

PHYSICS DEPARTMENT. The Physics Department of our new Boston College High School occupies a choice location on the upper floor at the southwest end of the building. The principal room, running the full width of the wing is 76 by 22 feet with spacious windows on three sides. This room is both laboratory and lecture room with the lecture table and students' desks at its south end, near the bay, and away from the Old Colony Boulevard. Students and teacher face across the narrow dimension of the room. The students' desks are arranged on "risers" for better observation of lecture demonstrations. Two smaller rooms make up the Physics "suite"—a Preparation Room and a Radio Room or workshop.

There are six student experiment tables twelve by four feet and three feet high, each separated from its neighbor by four and a half feet of space. This allows us 36 individual places at these tables, or, if the boys work in pairs, as usually happens, we can easily accommodate 72 pupils experimenting at the same time. Each location at the tables is equipped with gas and electricity.

We like to think that our electrical set-up both for lecture table and students' experiment tables is something special. The sub-contract for electrifying both the Chemistry and Physics laboratories was given to Standard Electrical Time Co., of Springfield, Mass., who have done an excellent job. A large electrical panel about 8 by 5 feet is within easy reach of the lecture table and controls all experimental electrical outlets of both laboratories. The teacher and students have at their disposal Direct Current from batteries ranging from 0 to 24 volts, as well as Direct Current from a five kilowatt motor-generator, variable from 0-125 volts. Alternating Current is also supplied at each location controlled by a powerstat with a potential from 0-150 volts.

This wide range of basic currents is supplied to all experimental outlets in the Lecture Room, Preparation Room, Radio Room, Chemistry Laboratory and Chemistry Preparation Room. There are more than a hundred of these outlets. The batteries supplying low voltage are the nickel-cadmium type with a tungar rectifier located on the main panel for charging the batteries. An electrical timer with a foot large dial is set in the upper part of the panel and is clearly visible to an assembled class. Lecture demonstration meters on the wall behind the teacher flank each side of the blackboard and are controllable from two large subpanels inserted in the front of the lecture table.

A new, or at least a different idea, on gas outlets is incorporated in the students' tables. Instead of having the gas cocks mounted above the tables where they are more or less in the way, they are set along the center of each table, below the table top with each gas nipple pointing upward and available at surface level through a ¾ inch hole. The gas is controlled by handles inserted in the apron of the table much as gas burners, are operated from the front of the conventional home kitchen range. Since the apron of the table also carries the electrical outlets, this leaves us with a completely clear table top, with no gadgets whatever cluttering its surface. A feature of this arrangement is that the tables are easily cleaned, oiled and polished. There is nothing in the way on the tabletop; nothing to bump into; no gas cocks to collect dirt or catch wires.

At each student's location there is a chromium plated clamp to hold cloths used for drying beakers, balances, hydrometers, etc., which the student uses in the course of the year. Glass-enclosed cabinets line the walls in the laboratory and preparation rooms. They are made with two-foot wide shelves and they run from floor to ceiling. Three soapstone sinks complete the general features of our new Physics Laboratory equipment at B. C. High.

WESTON COLLEGE

Physics Department. A small workshop for work in electronics has been set up in the Physics Department here at the college. The first step was to acquire the necessary tools and spare parts for this type of work. Up to the present time the work done in the shop has been confined to making much needed repairs on old electronic equipment in the laboratory and constructing new measuring equipment. So far we have constructed a Geiger counter, an r.f. oscillator, a condensor checker for measuring the values and characteristics of the many surplus condensors which we have accumulated, a square and sine wave generator, and a vacuum tube voltmeter. The last four have been made with the Heathkits which we find satisfactory. An impedance bridge kit put out by the same company will be purchased in the near future and its construction will be our next project. Construction of these instruments is not difficult and is very instructive. They represent the application of principles studied in class and give considerable practice in reading circuit diagrams, wiring, alignment of parts in a chassis, and adjustment.

Seismological Observatory. Monsignor Louis Kaas, Director of archeological investigations at the Vatican, has invited Father Linehan and Father Joseph Lynch of the Fordham Seismological Observatory to conduct a seismic survey in conjunction with the archeological excavations currently in progress at Vatican City. It is hoped that this survey will speed up excavation operations by eliminating much useless digging in areas which are of no interest to the archeologists. Since this is probably the first use of seismic methods for archeological purposes, the success of this survey might well introduce to archeologists a new tool for quick reconnaisance of questionable sites. A new set of the modern seismic refraction equipment, incorporating several features developed here at the Observatory, has been built and donated for this survey in Rome by the Century Geophysical Corporation of Tulsa, Oklahoma. This is the third set of equipment given to the Observatory in the past few years by these good benefactors.

In January, a seismic survey was operated in Portland, Maine, at the site of the new Cheverus High School to determine bedrock contours as an aid to the architect in planning the foundations for the new building. Water supply surveys were conducted in North Bergen, New Jersey, for Mooney Bros. Corporation; in Poughkeepsie, New York, for the Novitiate of St. Andrew-on-Hudson; at Concord, Massachusetts, for a new housing development.

Father Linehan directed a seminar in Geophysics at the Massachusetts Institute of Technology under the auspices of the Air Force Cambridge Research Laboratories. On January 14 and February 28 respectively Father Linehan gave lectures on Seismology to the Holy Name Society of Cohasset and the Building Inspectors of Massachusetts.

Nine graduate students are now studying here for their Master's degrees in Geophysics. Some of the problems already assigned for theses cover a wide range of the Geophysical field. "A Magnetic Survey of the Connecticut River Valley", "Special Study of Earthquakes at Distances of 1380 to 2760 miles from Weston", "The Period of Sound Waves at Varying Distances From Explosions", "Comparisons of Electric Resistivity in Various Types of Soils". Mr. John R. Mills, M.S. in Geology from Lehigh University, is observing seismic field technique and instructing in Geology.

COLLEGE OF THE HOLY CROSS

Department of Chemistry. Father Fiekers addressed the Seminar at Clark University, Nov. 29, 1950, on *Variable Valence and Schemical Structure*; Father M. A. Leonard spoke before the Fallon Clinic in Worcester, Feb. 15, 1951, on the *Determination of Non-protein Nitrogen.* On March 3, 1951, Dr. Baril was a judge at the St. Peter's High School Science Fair, Worcester. On March 24, 1951, Dr. Baril will address the New England Association of Chemistry Teachers at their Pomfret (Conn.) School Meeting on *The Vocational Aspects of Clinical Technique.* Recent addresses of scientific interest by guest speakers at the College include: Anthony Standen, Jan. 11, 1951, before the Cross and Scroll on *Science, the Sacred Cow*; Samuel F. Walton of the Exolon Co., in Tonawanda, N. Y., Feb. 15, 1951, before the Cross and Crucible on

Man Made Gems; David Bradley before the Cross and Scroll, March 1, 1951, on the topic, *Must We Hide?* (A-Bomb); Dr. Harold C. Hodge, Pharmacologist of the Rochester University School of Medicine and Dentistry, March 12, 1951, before the chemistry seminar on *Dental Caries*. Fathers Busam, Connolly and Fiekers have been appointed to the Pre-Medical Recommendation Board of the College by Father Rector. On April 28, 1951, the Mathematics and Science Unit of the New England Section of the College and University Department of the National Catholic Educational Association will meet at Holy Cross. Fathers Busam, Connolly, Fiekers and Smith constitute the committee in charge. Father Fiekers has been appointed to the Nominating Committee of the New England Conference on Graduate Education.

The International Sugar Journal for September 1950, pp. 298-299 contains the departmental publication: Turner, C. F., Galkowski, T.T., Radle, W. F. and VanHook, A., *Grain Formation by Sonic Irradiation*. This work was done in co-operation between the Department of Chemistry and that of Physics and Mathematics. The *Hormone*, publication by students of the department, who belong to the CROSS & CRUCIBLE CHEMISTS' CLUB, has been published monthly, October 1950 through January 1951. The dread reality of midyear examinations has caused further publication to lapse. It is expected that another issue or two will appear before the scholastic year closes. Many encouraging communications have been received by the editor, spontaneously and on the occasion of reprint requests. The magazine goes to practically every chemistry department in the country. Such training seems to pay dividends in, for example, the following way. A recent perusal of the indexes of Chemical Abstracts and other sources reveals over two hundred items contributed by alumni over the years in the form of books, articles and patents. This survey was made possible by the recent publication of the Holy Cross Alumni Directory. Members of the CROSS & CRUCIBLE were guests of the Graton and Knight Leather Co. in Worcester, visiting the plant on Nov. 21, 1950; a similar tour of the Narragansett Brewery was conducted at the beginning of the Christmas holidays. But it was restricted to graduate students and staff!

A syllabus study is being made in the department for the organization of the topic, dynamic equilibrium, in all courses, Freshman through Senior. Emphasis on analytic chemistry in the curriculum suggests the following plan of curriculum, now in partial effect. Qualitative inorganic acid analysis will be conducted for a few weeks at the end of the Freshman Inorganic course. Qualitative inorganic basic analysis will come at the end of the corresponding quantitative course for eight weeks in sophomore. The former is macro; the latter, semi micro. Advanced organic chemistry for seniors will include, as in the past, qualitative and quantitative (elemental and group) organic analysis. Even colloidal chemistry, it is planned, will culminate in some experiments on colloidal analysis for dispersion types. For the present, instrumental analysis is treated systemmatically throughout these courses and in the course of physical chemistry.

Physical improvements in the department include the completion of a sprinkler system for fire protection throughout O'Kane Hall and some work by the chairman on laboratory hoods, done in stainless and transite.

Reviews and Abstracts

L'Association des Jesuites Scientifiques et Son Bulletin. The following is a translation of part of a news item which appeared in the *Bulletin De Liaison Des Scientifiques S.J. no. 12, Novembre 1950.* "We have already mentioned the Association of Jesuit Scientists, Eastern Division, in Bulletin no. 8, June, 1948. This Association has over 200 members. It was founded in 1922. Every year it holds a 3-day convention. The Association specializes in the problems of teaching the sciences, research, and the harmony of science with philosophy and theology. The Bulletin published by the Association is extremely well done and serves as a model for similar publications in the Old World.

The issue of October 1949 is an index of the preceding 25 years. It constitutes a valuable document on the scientific work done by the American Jesuits during the last 20 years." *Ed.*

Chemical Research at Loyola College, Madras, India. *A Quarter of a Century's Growth* is the title of the Silver Jubilee Booklet, published by Loyola College in Madras in 1950. *Chemistry-Research*, by Father Lourdu M. Yeddanapalli, is one of the many interesting articles in this booklet (pp. 25-27). The article contains three interesting cuts: one of the Chemistry Building; one of research students at work; and one of the author at his glass blowing table.

The author distinguishes between academic and industrial research and goes on to give a brief technical background and a description of Loyola research, already done, now in progress, and that contemplated for the future.

Father Yeddanapalli is continuing his work on the vapor phase decomposition of the metal alkyls with which most chemists of this Association are familiar through the original publication on aluminum trimethyl. This work was done at Canisius College in Buffalo with Father C. J. Schubert, S.J. and was published in the *Journal of Chemical Physics, 14*, 1 (1946). Father Yeddanapalli and co-workers then re-examined certain results of other workers on tin and silicon tetramethyl. The results were expected to be published in the October 1950 issue of the *Journal of the Indian Chemical Society*. The decomposition of all of these metal alkyls "fit into the three-halves order kinetic expression and find a satisfactory interpretation in terms of free radical intermediates." Work on mercury and zinc dimethyl is now in progress. Plans call for the extension of the work in gas kinetics to include photochemical, electrical and catalytic (gas-solid) reactions.

The study of high polymers constitutes the second general research program. It centers on the isolation and characterization of certain phenolic principles in dark brown cashew shell liquid. Father Yeddanapalli and co-workers have reported this in two papers to *Current Science*, *19*, 209-210 and 281-282 (1950) and to the Indian Science Association Congress of January, 1951. Condensation of this phenol with formaldehyde is being studied and a report of this phase of the work has been made. Other work of local interest is being done on Indian turpentine oil.

Father Yeddanapalli did his theology at Enghien in Belgium before World War II. and did some of his advanced chemical work at Louvain University. During the War he studied at Princeton University in this country and received the Ph.D. degree in Chemistry. He then taught for a few years at Canisius College before returning to India. He also studied plastics at Brooklyn Polytechnical Institute and has published on the topic in this BULLETIN. He has published in Belgian, British and American journals on physical chemistry. We have reported here remarkable productivity under his direction from a small laboratory over a period of only two years.

REV. B. A. FIEKERS, S.J.

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The Editor feels that the members of the ASSOCIATION might like to know where their BULLETIN goes every quarter. Four hundred copies of each issue of the BULLETIN are printed. Of these about two hundred and ten go to the members of the ASSOCIATION in the United States and on the missions. One hundred and sixty five to the various destinations listed below and the remaining twenty five to the archives. The mailing list is as follows:

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