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Ways of Writing the History of Science

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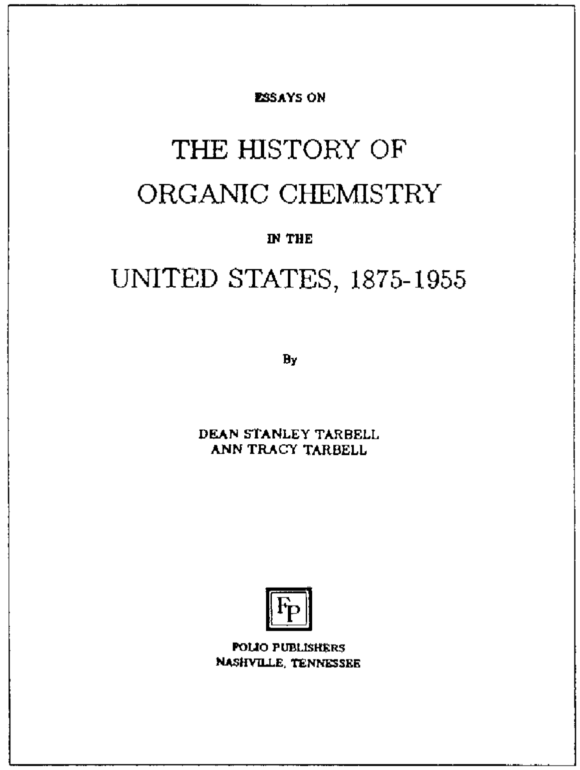
The history of science, and of chemistry in particular, has developed into a recognized intellectual discipline in the years since 1930, when I entered college. There are a number of academic centers with sizable programs in the field; every established university now tries to have at least one person specializing in this area, usually, although not always, attached to the history department. The HIST division is showing increased vitality, and the accomplishments of Arnold Thackray and his associates in building a vigorous center at Penn for the history of chemistry are familiar to you. A notable number of able young scholars is appearing in the field.

One of the leaders in establishing the history of science in this country was George Sarton, the founder of *Isis* and *Osiris*. Unfortunately, I never heard any of his lectures while I was in college, but I did discover *Isis* and the early volumes of his *Introduction to the History of Science* while poking around in the Widener reading room as an undergraduate (1). I usually read part of *Isis* and, particularly, Sarton's very numerous book reviews. This early interest persisted, and I gradually acquired nearly all of Sarton's published books. James B. Conant, the organic chemist who was president of Harvard from 1933 to 1953, improved Sarton's lot at Harvard very materially; Sarton had hitherto been only a research associate paid by a grant from the Carnegie Institution. Conant gave Sarton a full professorship as well as an honorary degree. Sarton's multivolume *Introduction*, although impressive in its scope and enormous learning, reached only into medieval times, and was in fact an annotated bibliography. His historical skill is better shown in his important two-volume *History of Science*, which unfortunately he was able to carry only to the beginning of the Christian era (2).

The founders of the History of Chemistry Division, among them E. F. Smith, C. A. Browne and F. J. Moore, were all trained as chemists, and their historical studies were an avocation, although Moore wrote a history of chemistry (3). Smith also wrote a number of historical books, but his greatest service was undoubtedly in making his valuable collection (the Smith Collection) of source materials for the history of chemistry. In the terminology of some historians of science, these men were "internalists". Their writings required some knowledge of chemistry, were directed primarily to chemists, and showed the internal development of the field by surveying the sequence of ideas and personalities.


ESSAYS ON

THE HISTORY OF
ORGANIC CHEMISTRY
IN THE
UNITED STATES, 1875-1955

By

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FP

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The "externalist" historians of science usually have some knowledge of the subject matter of science but are more concerned with placing the science in its social, economic and intellectual context. This classification of "internalist" and "externalist" historians is not clear cut but represents only the extremes of the spectrum.

Internalist history is useful for students and teachers in the field; it is particularly valuable for professional scientists, because many of them do not realize that they are working in a fundamental discipline with a distinguished intellectual and experimental heritage. One of the surprising and gratifying results of our book on American organic chemistry was comment from many people saying that it raised their morale and that of their junior assistants to feel that they were part of something worthwhile and important.

Internalist history can degenerate into antiquarianism, or into the diligent collection of many facts without many organizing ideas. Internalists, like the founders of the History Division, have usually been professional chemists who took up history as a hobby (frequently a full time one).

Externalist historians may have training in a variety of disciplines, such as history, sociology, philosophy. Their writings appeal to other historians and teachers of the history of science and, unfortunately, to a lesser degree, usually, to practitioners of the science. At its best, externalist history is a valuable contribution to intellectual history, with insights and connections which internalist history may miss. It may play an important role in clarifying and formulating public policy. At its worst, it may become an amiable collection of anecdotes without much solid intellectual content. One of the leading historians of science, C. C. Gillispie, has called for writers to be better informed about the fundamentals of the science they are discussing.

The leading sociological historian of science is undoubtedly R. K. Merton, whose Ph.D. thesis, under Sarton, examined the relation between Puritanism and the development of science in 17th century England (4). Merton has also examined the reward system for scientific work; the "Matthew Effect", which he proposed, can be summed up concisely, if inelegantly, by the sentence "Them as has, gets".

Some externalist historians deal with the economic basis of science and of research support, a topic on which anyone with decades of directing research programs has extensive working knowledge.

A particularly important branch of externalist history is the study of the relation of the federal government to science. As the importance of science and technology grows for the nation, this field becomes ever more significant. Federal support of science is not something new. It goes back at least to the Lewis and Clark Expedition of 1804-1806, sponsored by President Jefferson for scientific as well as for geopolitical purposes. Throughout the 19th century, scientific research by the army, navy, the coast survey, the geological surveys and the Smithsonian were government projects. A recent brilliant book by W. H. Goetzmann of the University of Texas, *New Lands, New Men*, which I commend to all students of 19th century American science, shows that, between 1840 and 1860, Congress supported the publication of 75 reports dealing with scientific exploration (5). Many of these were multivolume and the publications, as well as the expeditions, were expensive. Goetzmann estimates that in some of these years the expense amounted to one third to one quarter of the federal budget. Research on agricultural problems broadly defined was done in the land grant colleges, by state experimental stations, and by federal agencies. This history has been reviewed to 1940 by Hunter Dupree in *Science in the Federal Government* (6), and Don Price has covered the immediate post-war years in *Science and Government* (7). Nathan Reingold, former editor of the papers of Joseph Henry, has also made useful contributions (8).

The role of the federal government in supporting research has of course increased very strikingly since World War II. The examination of its role has therefore become extremely important in recent years.

Some indispensable books, like Aaron Ihde's *The Development of Modern Chemistry*, are difficult to classify as internal or external history (9). Ihde has had experience in studying and teaching both chemistry and the history of the science, and this book is valuable because of his varied experience.

I would like to mention in more detail two outstanding books, one by a non-scientist, the other by a scientist. The first is *The Discovery of Insulin* by Michael Bliss, Professor of Canadian History at the University of Toronto (10). This fascinating book deals with the most important medical discovery between the advent of antiseptic technique and the discovery of the sulfa drugs. Bliss based his book on much unpublished material, including laboratory notebooks of F. G. Banting and C. H. Best. He received scientific help from a number of people knowledgeable in the life sciences, and his book is a gripping account of the frustrations and triumphs of scientific research. He has a clear understanding of the difficulties of getting reproducible results in isolating and testing biologically active materials. He also has a novelist's talent for describing the development and interaction of personalities, with which the insulin discovery was well supplied.

Suspense is added by the story of the diabetes victims, like Elizabeth Hughes, daughter of the future Chief Justice Charles Evans Hughes. Will enough insulin concentrate become available to save their lives? It did, and Miss Hughes lived, married, had three children, and died of a heart attack 60 years after her first injection of insulin.

Bliss gives a full discussion of the award of the Nobel Prize to Banting and Macleod, an act which aroused endless controversy because C. H. Best and J. B. Collip, both key workers on the problem, could not be honored. This raises the insoluble question, inseparable in any award, of who gets honored and why.

The role of the Eli Lilly Co. was indispensable in making insulin available in sufficient quantity to meet the demand. G. H. A. Clowes of Lilly, noting the first reports of active pancreatic extracts, had followed the Toronto work closely. By enlisting Lilly resources on the isolation of active extracts and eventually of pure insulin, he brought the discovery to clinical usefulness. Lilly's control of the field was a major factor in its growth into a major pharmaceutical company.

Presumably Bliss' book is externalist history, but it is difficult to see how the story could have been told better or more authoritatively by anybody. It is no denigration of his book to point out that it deals with events limited in space and time, even though the results of the insulin discovery were of worldwide significance.

Ernst Mayr's book, *The Growth of Biological Thought*, is internalist history (11). Mayr, the Alexander Agassiz Professor of Zoology Emeritus at Harvard, is one of the leading evolutionary biologists of his time, a fact of which he makes frequent mention. His book is thus based on a detailed knowledge of the field and also on extensive reading on the history of ideas in biology. He started work on it in 1970 when he was 66 and it was published in 1982. His book unfolds against a vast background of the history of the earth and of all life, from one-celled structures to man.

His volume has an introduction of 147 pages dealing with the nature of biology and an overview of the changing intellectual milieu of biology from antiquity to the present, a summary of the remainder of the book. In the body of the work, he has three main divisions. The first deals with diversity of life, a history of taxonomy and the changing ideas about species and higher taxa. The second division sets forth the history of evolutionary ideas, the leading figure being Charles Darwin, with subsidiary billing to Alfred Russel Wallace, the codiscoverer of evolution through natural selection, and to my mind a more interesting person than Darwin. The third division, on variation and its inheritance, has Gregor Mendel as its hero, with a large supporting cast, among whom is the geneticist Thomas Hunt Morgan, the fruit fly man of Columbia.

Obviously, even if I were qualified, it is impossible for me to review a very long and closely reasoned book like this, written with a clarity of analysis and a vigor which compel respect and even awe. I want to mention, instead, some of the facets of Mayr's thought and give a little of the flavor of his book. Mayr speaks his mind bluntly on many things, believing that making categorical statements "leads more quickly to the ultimate solution of scientific problems than a cautious fence sitting".

There are two themes, almost leit-motivs, which recur constantly in Mayr's discussion. One is the inadequacy of physics as a starting point for understanding biology. "Most physicists seem to take it for granted that ... once one understands physics, one can understand any other science, including biology." Mayr comments on the statement of an eminent physicist that the scientific world view is based on the great discoveries of the 19th century about electricity, heat and the existence of atoms and molecules as follows (p. 33):

> ... as if Darwin, Bernard, Mendel, and Freud (not to mention hundreds of other biologists) had not made a tremendous contribution to our scientific world view, indeed, perhaps a greater one than the physicists. In biology, one rarely deals with classes of identical entities, but nearly always studies populations consisting of unique individuals. This is true for every level of the hierarchy, from cells to ecosystems ... While entities in the physical sciences, let us say atoms or elementary particles, have constant characteristics, biological entities are characterized by their changeability.

Mayr repeats this point often. It does not mean that one must invoke a vitalistic force to study biological phenomena, but that one must use a population point of view because of the variance of individuals. Mayr does not rule out the possibility that physical and chemical terms may ultimately be sufficient to describe organisms, and this seems very likely to me, although it may take a very long time. As I have written, "structural organic chemistry, with its limited number of basic principles, acts as a bridge between quantum mechanics, which explains chemical bonding, and the complexities of living systems".

When a well-known Nobel laureate in biochemistry said, "There is only one biology, and it is molecular biology", Mayr comments, "He simply revealed his ignorance and lack of understanding of biology" (p. 33).

The second point which Mayr continually combats is the notion of "essentialism". By this he means the doctrine, due to Plato, that (pp. 38-39):

> ... the variable world of phenomena ... was nothing but the reflection of a limited number of fixed and unchanging forms, *eide* (as Plato called them) or *essences* as they were called by the Thomists in the Middle Ages. These essences were what is real and important in the world ... Constancy and discontinuity are the points of special emphasis for the essentialists ... Essentialism, with its emphasis on discontinuity, constancy and typical values (typology) dominated the thinking of the western world to a degree that is still not yet fully appreciated by the historians of ideas. Darwin, one of the first thinkers to reject essentialism (at least in part), was not at all understood by the contemporary philosophers (all of whom were essentialists), and his concept of evolution through natural selection was therefore found unacceptable. Genuine change, according to essentialism, is therefore possible only by the saltational origin of new essences. Because evolution as explained by Darwin is by necessity gradual, it is quite incompatible with essentialism. However, the philosophy of essentialism fitted well with the thinking of the physical scientists, whose "classes" consist of identical entities, be they sodium atoms, protons, or pi-mesons.

It can be seen that Mayr's rejection of physical science as a satisfactory approach to biology and of the essentialist philosophy, are very closely related.

Some other aspects of Mayr's thought include his low tolerance for philosophers of science (p. 130-131):

> It is still subject to controversy whether or not philosophy has made any contribution to science after 1800. Not surprisingly, philosophers generally tend to answer this question in the affirmative, scientists often in the negative. There is little doubt, however, that the formulation of Darwin's research program was influenced by philosophy ... In recent generations philosophy rather clearly has retreated into metascience, that is, an analysis of scientific methodology, semantics, linguistics, semiotics, and other subjects at the periphery of science.

Externalist science can of course be written from a Marxist standpoint. Presumably all Soviet history of science is based on the Marxist view, and some western historians have used it also. Mayr comments (p. 492):

> The Marxist claims that Darwin and Wallace were extending the laissez faire capitalist ethos from society to all nature to make a Weltanschauung out of the new captains' of industry's utopia of

progress through unfettered struggle is not supported by any evidence whatsoever ... Darwin did not live in an ivory tower; he must have seen what went on in England all around him; he read all the relevant literature, and this might have facilitated his acceptance of certain ideas. Yet, if the theory of natural selection were the logical and necessary consequence of the zeitgeist of the industrial revolution, it should have been widely and enthusiastically adopted by Darwin's contemporaries. Actually, just the opposite is true; Darwin's theory was almost universally rejected, indicating that it did not reflect the zeitgeist.

The recent publication of Darwin's diaries allows the development of his thought to be followed in great detail.

Although Mayr disclaims any intention of writing a biographical history of biology, his book contains some memorable sketches of his major figures. Among these are Buffon (who published a 39-volume work on natural history), Cuvier, Larmarck, Mendel, Darwin, Wallace, and many more, including (by implication) himself. After reading 828 pages of text and 30 pages of epilogue in his book, one feels well acquainted with the personality and modes of thought of Ernst Mayr.

The epilogue offers some shrewd comments on sciences and scientists. Mayr emphasizes the "doubts, hesitations, inconsistencies, contradictions, and frequent changes of mind" (p. 830) in the emergence of a finished piece of scientific work, although in the conventional presentations of results the process is made to appear logical and direct. He observes that, with the advantage of hindsight, it is frequently hard to understand why it took so long to reach a seemingly obvious conclusion or to do an obvious experiment. A pertinent example, from the history of chemistry, is the contention of Arthur Michael that addition of bromine to a carbon-carbon double bond is *trans*. He produced some plausible but not conclusive evidence for *trans* addition. It was nearly 25 years (in 1911) before anyone did the conclusive experiment; one which seems obvious now. That year, both Emil Fischer in Germany and Alexander McKenzie in Britain showed that the dibromo acid from (*cis*) maleic acid was resolvable; that is, had no plane of symmetry and therefore must be the result of *trans* addition.

To return to Mayr's epilogue, he has equally wise words on research strategies; one should separate complex problems into components that can be attacked; one should not continue to belabor a point which is already generally accepted. New ideas or discoveries require clear presentation and should not be buried in papers with uninformative titles or which contain highly heterogeneous material. The whole epilogue well repays reading by a scientist in any field.

Not content with the volume at hand, Mayr has written a second volume, which has not yet appeared, dealing with physiology, developmental biology and neurobiology.

Mayr, by writing major books on the history of biology, shows the importance of the history of science in his mind. He expresses this feeling explicitly in numerous places. The first two sentences of his preface read, "much of modern biology ... cannot be fully understood without a knowledge of the historical background of the problems. Whenever I made this point to my students, they would ask me in what book they should read up on these matters." His book is his answer to this query.

Personally, I feel that some more emphasis on the history of chemistry in the beginning course would make the subject much more attractive to students. Instead, we frequently offer freshmen a dreary melange of topics, without much apparent relevance to their daily lives and with little to arouse their interest.

Students and teachers of science frequently have little appreciation of the long and varied history of the subject, or of careers and personalities of scholars responsible for our present knowledge. Students, in particular, are apt to feel that the science emerged fully grown in the enormous text books which they study. Some sense of the slow development over many decades of the ideas and experimental methods of science might materially enliven their studies. Furthermore, the fact that a science, or any other field of learning, is dynamic, not static, and that important new experimental observations or theories are constantly altering our views is a stimulating and salutary idea that should be emphasized for students. Science and chemistry, in particular, desperately need an informed public view of the nature of the scientific enterprise; a view which the public is not now getting.

All of the varieties of the history of science I have discussed, and others as well, have a real contribution to make to our understanding of science and its place in our culture. I hope that the field will continue to flourish.

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KASIMIR FAJANS (1887-1975): THE MAN AND HIS WORK

Part II: America

Reynold E. Holmen, White Bear Lake, MN

Kasimir Fajans, circa 1953

It was in September of 1936, in the midst of the depression, that I first entered graduate school at the University of Michigan, though for only a year's stay, prior to landing a much needed job. I remember the apparent pride and satisfaction with which Dr. E. H. Kraus, then Professor of Mineralogy and Dean of the College of Letters, Sciences and Arts, told me of the newly arrived Professor Fajans. A study of the chemistry department's staff needs, a study in part induced by Chairman Moses Gomberg's impending retirement, had resulted in several recommendations. Foremost of these was that of securing a leading personage, such as Fajans, to bolster the physical chemistry staff, which some felt was rather inbred. In our analytical chemistry course, Professor Hobart Willard, when introducing the topic of adsorption indicators in argentometry, also reminded us that the man who had founded the field was now a faculty member. Unfortunately, during this first stay at Michigan, I met and talked with Fajans only once and for but a few minutes about the subjects of photochemistry and argentometry.

Fajans quickly made his presence felt. He continually challenged his students to view critically any explanation of chemical phenomena. His 1930 American visit had led him to question some teaching practices common in this country. He could not understand how a student could graduate from high school without having studied physics, a subject he considered even more important than chemistry. His firsthand involvement at Michigan with American chemistry textbooks and teaching practices disturbed him further, particularly after briefly taking over L. L. Brockway's introductory physical chemistry class near the end of World War II. (Brockway was engaged in defense-related research.) He did not readily adjust to this type of classroom task after years of teaching advanced students. The latter were less apt to have a problem with his accented English and were better prepared to benefit from his presentation of subject matter; especially when, as in his inorganic chemistry course, the content was fixed by himself rather than being based on someone else's syllabus, thus offering less frequent temptations to digress. In contrast to Brockway's carefully organized and logical presentation of introductory physical chemistry, Fajans' approach was replete with digressions, causing some difficulties for students acquiring their initial foundation in the subject. In spite of this, at least one student, who experienced the lectures of both men, decided that the deeper questions and insights were presented by Fajans (49). Additionally, his office door was always open to any student, freshman or doctoral, having a question or seeking information.

Certain recurrent themes and points of emphasis probably would be recognized by many of Fajans' former students as characteristic of him. Among these were his stress on the importance of the interaction of specific factors in chemical behavior, such as thermodynamics and kinetics, and how our inadequate knowledge of this interaction operates in countless cases to prevent us from making absolute predictions. Typical examples included such statements as:

Our body is unstable thermodynamically relative to CO_2, but it exists quite comfortably for 70 or more years ... There is no absolute stability, only relative stability ... London in 1927 stated that no oxide of fluorine could exist. The next year one was made ... In chemistry you always borrow and give with respect to energy ... Contrary to valence-bond theory, chemistry does not involve a saturation of a definite number of forces but rather a screening of charges - more or less complete ... Saying that each of two atoms can attain closed electron shells by sharing a pair of electrons is equivalent to saying that husband and wife, by having a total of two dollars in a joint