THE GROWTH CURVE OF A SCIENTIFIC LITERATURE

NITROGEN FIXATION BY PLANTS[1]

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The Problem

One of the most obvious and yet almost neglected aspects of scientific research has been the accumulation of a technical literature. To label this literature "neglected" might appear to be a generous overstatement, in view of that constant demand for current periodicals and bound volumes in the majority of our scientific libraries, a demand that gives rise to that most irritating statement, "The journal you want is out." Appreciation of the literature, however, is confined primarily to its use as an additional tool for more research—it serves as a fountain of information and indicates guide-posts for present problems. While practically every scientist claims acquaintanceship with the literature of his particular field, his knowledge is usually restricted to the factual material contained therein. Study of the literature as an entity is almost completely unknown; its function in science is primarily that of a technical accessory—its meaning for the philosophy of research is little more than that furnished by gelatine plates, guinea pigs or graph paper.

It appears probable that this limited view of the function of publications might decidedly handicap the service of literature to general problems of science, however satisfactorily it meets the demands for mere technical assistance. At present, with the values of science seriously questioned by layman and official, an inventory of past and present activities is indicated to answer the recurrent questions of "Whither Science? Why? and How Far?" It is suggested that a study of the *biological properties* of the literature of various fields might provide one method of attack for the necessary inventory. By biological properties is meant the growth and development of a literature, especially as these are influenced by and reflected in the environment; subject-matter as well as size should be considered in evaluation of the data.

A census-taking of the publications in a given field should provide valuable information for the interpretation of past production and might afford some basis for prediction of future trends. Possible uses of an analysis based on the counting of heads among published papers would include the following:

(1) Provides an insight into the nature of the dependence of interest in a given problem upon external factors of an economical, political or emotional nature.

(2) Gives to the novitiate in a field an idea of the volume and distribution of the literature which forms the organic body of data to which he hopes to add.

(3) Enables a limited prediction with respect to future production of both number and content of papers. Awareness of future trends in production might allow a measure of control if this is deemed wise or necessary.

In order to lend itself to an analysis such as that proposed, the body of relevant material should possess certain characteristics. For example, interest in the subject-matter of the literature to be examined should have extended over a number of years. This insures that a

[1] Herman Frasch Foundation in Agricultural Chemistry, Paper No. 88.

more or less steady development rather than the mushroom type of growth has occurred. Likewise, the subject-matter should cover a fairly large but definite problem. The field should not be too sharply delimited; otherwise the volume of publications will not be sufficiently large to allow statistical treatment. On the other hand, unless the papers are concerned with a perfectly definite problem, the growth curve will represent the sum of a number of interlocking developments and as a result may defy rational analysis. Finally, the field of study represented by the literature being examined should be coherent and stable. If this is so, the research at a given time is intimately connected with past work and does not depend on the whim of the fad type of scientist.

Method of Attack

A body of literature that apparently satisfies the foregoing requirements is that which deals with the subject of nitrogen fixation by plants, especially by the *Leguminosae*. Because of the practical application, the literature on this subject boasts of an ancient and respectable lineage. A Chinese writer of the fifth century B.C. describes the use of the mung bean for green manuring. Reference to leguminous plants, likewise, are found in Egyptian writings of the pre-Christian era. Greek and Roman writers comment, often quite astutely, upon the importance and function of the *Leguminosae* in agricultural art. When in the early nineteenth century, agriculture was definitely regarded as a suitable field of inquiry for natural scientists, foremost among the contributions to the laws of husbandry were those concerned with the use of leguminous crops.

Albrecht Thaer, the foremost agriculturist of his day, strongly advocated the inclusion of the *Leguminosae* in crop rotation to the point of advising rotations in which leguminous crops predominated. So great was his influence that in 1856 he can state, "latterly the practice of sowing white clover with the last crop has become very general; only a few apathetic and indolent agriculturists, or men who are firmly wedded to old opinions and customs neglect this practice." Schultz-Lupitz, "the father of modern green manuring," urged the turning under of clover, lupines and peas for soil improvement. In America as early as 1801 Bordley writes in his "Essays and Notes on Husbandry and Rural Affairs": "Clover plowed in, together with the remaining of grain stubble, year after year will gradually meliorate the soil."

In light of these practical observations concerning the value of leguminous crops as enrichers of the soil, it is not surprising that the earliest researches of agricultural experiment stations deal with an examination of leguminous plants, especially with their nitrogen nutrition. In Europe as well as in America, the history of both federal and state experiment station is intimately connected with studies on the various problems associated with the question of fixation of atmospheric nitrogen by plants. The literature resulting from these researches has been reviewed by Fred, Baldwin and McCoy,[2] who in an effort to examine all important publications on this subject collected a file of over 2,000 references. Access to such a population arouses an irrepressible urge in the student of statistical science to make some sort of classification of the catalogued items. The problem then reduces itself to selection of a basis of classification which will provide the maximum information with respect to the biological properties of the population.

[2] Wisconsin Studies in Science No. 5, 1932. The authors wish to express their appreciation to Dean Baldwin and Professor McCoy for their cooperation in the preparation of this manuscript.

Before discussing the data, it might be well to consider briefly the methods used and to indicate certain limitations in the sample. The publications of a given year were divided into nationalities with respect to *total number* and to *total pages*. By nationality is understood that of the journal publishing the paper rather than the native country of the author, since the latter is frequently difficult to determine. In a large sample, heterogeneous "crossings," *i.e.*, Frenchmen publishing in German journals, should tend to compensate one another. In the case of papers appearing in journals which publish in several languages, *e.g.*, *Zentralblatt für Bakteriologie,* the publication was credited to that nation whose language was used. In the latter case it was necessary to ascertain the nationality of the author of those papers published in English. In order to eliminate, at least in part, the variations which arise from time lag in the date of publication after receipt of the manuscript, the data were averaged for a two-year period.

It might appear to be advantageous and desirable to classify the material according to subject-matter. This plan was rejected when it was found that many publications would fall into several groups with a resulting confusion that would defy analysis. Instead, the predominant topics of investigation of a given period will be discussed briefly in a general way in the text.

Although the authors of the monograph endeavored to collect all extant original contributions to the subject, it is obvious that certain papers were overlooked or eliminated from consideration. These will include: (1) A vast number of popular articles and semi-popular bulletins, the scientific nature and value of which are open to question; (2) papers of recent publication and hence not yet in the "literature," *i.e.*, abstracted or referred to by other authors; (3) papers appearing in obscure journals that are not abstracted; (4) papers with practical value for a given locality and whose circulation is accordingly limited, *e.g.*, station bulletins and reports. Cases three and four will tend to create a bias in favor of the United States, since very rarely would a publication on this subject in the United States fail to be noted in the *Experiment Station Record.*

The Embryonic Period—Before 1886

Production of research articles since 1884 pertinent to the subject of nitrogen fixation by leguminous plants is indicated in Fig. 1. Prior to the middle of the nineteenth century, a few papers had appeared on this subject, notably the studies of Boussingault, Ville and Liebig dealing with whether or not plants in general can utilize the free nitrogen of the air. From 1850 to 1884 this subject received increasing attention, a total of 90 papers appearing during this period. The contributions came from many countries and the rate from any one nation was erratic, hence no attempt was made in this figure to plot the growth prior to 1884.

The data were conflicting, the publications were often polemical in nature, and the question of the ability of plants to fix atmospheric nitrogen remained unsettled. It is interesting to note that in many of these papers the answer to the perplexing problem was shrieking to make itself heard above the din of controversy. *A posteriori* it is easy to see that Ville, Lawes and Gilbert, Boussingault and Atwater all published experiments which might have clarified the muddled situation, had the proper interpretation been forthcoming.

Ville in France claimed that all plants —legumes, grains and grasses—can utilize free nitrogen and convinced a referee board including such distinguished scientists as Dumas, the chemist, and Chevreul and Regnault, the physicists, of the truth of his claims. Ville's fellow countryman, Bossingault, likewise presented data from field experiments which had been carried out for 16 years and which seemingly offered strong proof that leguminous plants fixed atmospheric nitrogen. Liebig convinced the majority of investigators, including Boussingault himself, that the observed gains in nitrogen were only apparent and rested on errors in the analyses of the manure added to the fields.

Lawes, Gilbert and Pugh at Rothamsted in England conducted experiments which were masterpieces of technique and concluded that no plant is able to use free nitrogen. Cereals and leguminous plants were grown under glass in sterilized soil and supplied with sterile water and air; their exact control kept out the bacteria necessary for nitrogen fixation by the *Leguminosae*. These workers were cautious enough to suggest that the conditions of their experiments were artificial and abnormal, since field experiments indicated that *Leguminosae* obtained more nitrogen than was present in soil, rain and seed.

Probably closer to the solution of the problem than any of these was Atwater, of Wesleyan University, Middletown, Connecticut. In the early eighties, through "the generosity of Hon. J. W. Alsop, M.D., who, by defraying the larger part of the pecuniary cost and by aid in other ways, has made the investigation practicable," Atwater and his assistant, Woods, made rather extensive experiments with the pea plant. The plants were grown in the greenhouse and in the open in the presence of varying levels of nutrients, including combined nitrogen. The nitrogen balance at the end of the experiments showed conclusively that nitrogen had been acquired from the air in such quantities as to preclude the conclusion that it had been assimilated as ammonia. In spite of the fact "that plants assimilate free nitrogen is contrary to the general belief and the results of the best investigators on the subject," Atwater clung persistently to the belief that leguminous plants could use free nitrogen. In his first report (1884) before the British Association for the Advancement of Science, Atwater offered no explanation for his data; later in the *American Chemical Journal* he attempted to reconcile his findings with those of the "best investigators." Two factors that might have influenced his results and not those of the others were suggested—electricity and microorganisms. Both of these were novelties in science and Atwater appeared to embrace both without favor or prejudice as the possible missing link. In 1886 he again discussed his experiments in the December issue of the *American Chemical Journal,* stating:

> To what extent the attested acquisition of atmospheric nitrogen is a function of plant alone, in how far it may be dependent upon the action of electricity, microbes or other agency induced by the plant, by what species of plants and under what conditions it is accomplished are of course matters for future study. . . .
>
> I am aware of no observed fact to imply that these (ignited sand and nutrient solution) separately or together are able to fix nitrogen by aid of electricity, microorganisms, or any other means. In the present state of our knowledge, therefore the balance of probability seems to decidedly favor the assumption that the plants themselves must be factors in the acquisition of atmospheric nitrogen. . . .
>
> But whatever may be the plants that acquire atmospheric nitrogen, the ways by which they acquire it or the form in which it comes, the fact of its acquisition seems well established.

Atwater clearly recognized the possibility that both plants and bacteria may be factors in the fixation process but was confused by the other alternatives, notably electricity, and thereby missed the solution of the problem. Prophetically he adds:

> And late research leads us to hope that the explanation of the process by which the nitro-

gen is acquired may be found, perhaps in the near future.

The hope had already been realized. In 1883, Hellriegel and Wilfarth, two rather obscure German plant chemists, had begun a series of experiments which were destined to supply the key to the puzzle. Initially they had planned only to study the influence of the quantity of combined nitrogen on the yield of crops, seemingly a somewhat obvious and simple problem. At the end of two years of work, Hellriegel and Wilfarth could enumerate three facts, well established but unexplained. Cereal crops grown in sand cultures, supplied with a nutrient solution complete except for the element nitrogen, grew in proportion to the quantity of combined nitrogen added, but the growth of a leguminous crop apparently was independent of added nitrate. Moreover, chemical analyses demonstrated that the leguminous plant, peas, often showed gains in nitrogen which must have come from a source other than the sand or nutrients; no such gains were observed with the cereals. Finally, the pea plants in the *control* pots, *i.e.*, pots to which no nitrate was added, developed in a most confusing and inconsistent manner. Duplicate pots would be quite at variance with each other; the peas in one would be tall with dark-green foliage, in the other, stunted and yellow.

In a manner not unlike that of the detective in mystery fiction, these two methodical German investigators searched for a clue in the mass of evidence which would lead to a rational explanation of the seemingly irrational data. Four hypotheses, advanced by other workers to explain the differential response of cereals and leguminous plants to nitrogen nutrition, were carefully applied to the data, and each in turn rejected. Elimination of these hypotheses was in the main due to the unexplained vagaries of the control pots; four "beautiful hypotheses were slain by one ugly fact." But what at first appeared to be only a major tragedy of science proved to be in the end the long-sought-for clue. The curious, variable behavior of the peas in the control cultures could be interpreted as the random distribution of some unknown factor, a factor which entered the cultures from the outside and which enabled leguminous plants to fix nitrogen. If this were the case, one would expect the fixation to be unpredictable, since whether or not this factor reached a given culture might well be governed by the laws of chance. To construct an adequate hypothesis on the basis of this idea was the next step toward the solution. Hellriegel knew that the air contained microorganisms, that certain species of microorganisms were able to fix atmospheric nitrogen and that some plants lived symbiotically with lower fungi (mycorrhiza). Reports in the literature had offered strong evidence that the protuberances on the roots of the *Leguminosae,* the nodules, were filled with bacteria. These observations were combined into a simple yet complete explanation of the facts: bacteria from the air fell into the open sand cultures, infected the peas, forming nodules which enabled the plant to use free nitrogen; the bacteria were without effect on the cereal crops. Although this hypothesis brought order into the confused results of previous experiments, publication was delayed until it could be critically tested.

In March, 1886, Hellriegel and Wilfarth began the crucial experiment. In one series, seeds, sand, pots and nutrient solution were carefully sterilized and the sand protected from chance infection by sterile cotton; the second series was identical, except that the pots were deliberately infected with an extract of a rich garden soil; in the third series nothing was sterilized and infection left to chance. This experiment was a complete failure—in their anxiety to eliminate microorganisms a sand was obtained which had been strongly ignited; the ignition had formed an ash so alkaline that none of the plants grew. The experiment was discarded and a second started in May; Hellriegel's sole comment on what must have been a bitter disappointment was that the duplicate experiment was begun "late, but as the following will show, not too late." Seldom has a critical test given such perfect results. Peas grown in sterilized sand behaved as did the cereals, *viz.*, the growth was dependent on added nitrate, but in the sterilized sand plus soil extract all cultures of peas grew luxuriantly and fixed quantities of free nitrogen. In the non-sterile sand the development of the peas was erratic, as in the former experiments. At harvest the verification was complete; peas grown in the sterilized sand were free of nodules, but those in the sand plus soil extract were covered with the characteristic tubercules. In the non-sterile series only those plants which were tall and thrifty were infected.

Hellriegel appeared before the agricultural experiments section of the Versammlung Deutscher Naturforscher und Aertze in September, 1886, dramatically exhibited typical pots of leguminous plants and cereals and made his report. His discussion of the results was brief, but the demonstration of actual plant cultures met all arguments. Ironically, this meeting in Berlin was presided over by Henry Gilbert of Rothamsted, whose painstaking experiments of thirty years previous were thought to have settled the question, but whose conclusions were now shown to be in error—an error arising from too much care! Undiscouraged, Gilbert returned to England and soon published an account of experiments which are masterpieces of technique and which completely confirmed the findings of Hellriegel and Wilfarth.

The Period of Development (1886–1914)

The discovery of Hellriegel and Wilfarth immediately stimulated research on nitrogen fixation; reference to the figure shows that a real bull market resulted during the next few years. Scientists of many nations vied with one another in turning out copy at a terrific pace. Germany dominated the field, but France was also prolific; the British Empire produced at a more conservative rate and America made modest contributions through the efforts of Atwater and collaborators. Many of the papers during this period of accelerated development were less than ten pages in length and were often published in series. An author would publish six or seven articles bearing the same title in a single volume of a journal, each article dealing with a detail of one major experiment. It is probable that the modern editor would not accept such "squibs," at least on this subject, and this must be kept in mind when comparing the output during this Golden Age of production with later periods. Subject-matter of most of the contributions is monotonously similar—the experiments of Hellriegel and Wilfarth have been confirmed. It is a tribute to the genius and careful work of these two men that few dissenting votes are noted among the many checks on their work.

Interest in the question of whether or not all plants can use free nitrogen was revived, and during the period 1890–1895 the last mass attack was made on this question. The old experiments were repeated and new ones devised, but the results with plants other than members of the *Leguminosae* were uniformly negative. Since 1900 investigations on this subject have dwindled to the sporadic efforts of the more intrepid workers.

One important contribution made during this period must not be overlooked, *viz.*, the isolation in pure culture of the causal organism. Hellriegel and Wilfarth were plant chemists and were uninterested in the finer points of the biology inherent in their work. They employed for inoculation crude culture of soil or extracts of soil. In 1888 Beijerinck isolated pure cultures of the organism from the nodules of various leguminous plants and laid the foundations for the extensive bacteriological studies necessary for final solution of the problem.

About 1895 the zeal to verify the experiments of Hellriegel and Wilfarth had largely subsided and the rate of production fell off decidedly. Simultaneously the content of the papers changed; editors probably began to reject what an unkind critic might term "pot-boilers," and attack on some of the more complex features of the symbiosis became necessary. With the formulation of the complicated aspects of the problem, there was a sudden loss of interest which caused a sharp dip in the curve of production. Nevertheless, during the period of this "low," some of the most suggestive and provocative contributions were made, notably by the Germans, Frank, Nobbe and Hiltner, and by the Hollander, Beijerinck. A few of the questions raised by these investigators, many of which have not received final solution after 40 years of research, include:

(1) Does one species of bacteria attack all species of *Leguminosae*, or does each species of plant require a special type of bacteria?

(2) Are the bacteria capable of fixing nitrogen apart from the proper host plant?

(3) What is the explanation of strain variation, *i.e.*, why does one strain of the organism benefit the plant, whereas another strain of the same species proves non-beneficial?

(4) What is the best manner to apply artificial inoculation to crops? Under what conditions should artificial inoculation be used?

(5) What is the chemistry of the process?

(6) What members of the *Leguminosae* are especially suited for use as green manures? How should these be handled?

(7) What is the order of magnitude of nitrogen returned to field by various leguminous crops when used as a green manure? When the crop is removed?

From 1895 until 1915 there is a gradual but steady increase in the rate of production of research papers. In these 20 years the influence of German science is manifest; without a doubt, the Germans were the leaders in this field with respect to both quality and quantity of contributions. At the beginning of the century the number of papers by American investigators rose rapidly and soon were threatening the German dominance. Part of this was due to the efforts of Moore at the U. S. Department of Agriculture, who succeeded in popularizing this field of research among the investigators at experiment stations. Likewise the expansion of experiment stations in numerous states aided in swelling the total of publications from the United States. A large number of the American contributions at this time were in the form of station bulletins and dealt primarily with practical problems, *e.g.*, the type of leguminous plants to use in a given locality or with the benefits of artificial inoculation. While the publications from the United States increased rapidly, those from the British Empire exhibited a more restrained development and those from France declined. It is noteworthy that research on this problem in countries other than the "Big Four" was definitely established during the period from 1895 to 1915.

Approaching Maturity (1915—)

Even before the world war, there was evidence that the United States was to assume leadership in the production of research concerned with nitrogen fixation by the *Leguminosae*. With the start of the conflict this occurred. Among the European nations the quantity of research fell off sharply during the period from 1914 to 1919, while an actual expansion took place in the United States. The explanation of the increase in the productivity of America during the war probably rests on the fact that food production for man and beast was the feature of our participation. Research in the field under consideration accordingly would be stimulated by the national crisis; the increase does not imply non-participation in war activities by the American scientists as might be superficially inferred.

One of the most important of the practical contributions made during this period was the development of reliable methods for supplying pure cultures of the bacteria for artificial inoculation of leguminous seed. Such cultures made unnecessary the use of the laborious and haphazard soil transfer method previously used to provide the proper organisms. The majority of previous attempts to furnish pure cultures had proved disappointing and frequently disastrous mainly because lurid, exaggerated claims made to sell the cultures were substituted for scientific control in their production. Research at several experiment stations, notably the Soil Bureau at the U. S. Department of Agriculture, Wisconsin and Cornell, resulted in the development of a technique which insured successful production of dependable cultures of the various legume bacteria. Because of the previous exploitation by unscrupulous purveyors of "inoculum," farmers viewed with skepticism if not suspicion the new product. Gradually through demonstration and education they were not only convinced of the advantages of artificial inoculation for leguminous crops but also appreciated the limits of the benefits to be expected under various conditions.

At the termination of the war, the contributions from the United States showed a definite decrease, a reaction common to many of the wartime industries. This decrease continued into the early twenties, but at the same time research was revived in Europe so that the curve of production for the entire world began to rise. About the period from 1922 to 1923 the rate of production climbed in all countries, but especially in the States. The upturn in this country was so precipitous that about 1928 there is evident a sharp peak in the curve of production for the United States, a peak that carried the curve for the entire world into dizzy heights. This period of pronounced inflation is rather disconcerting to one endeavoring to find law and order in the growth of a literature, but it is believed the following explanation accounts for the sensational rise and subsequent fall.

In all countries, a political philosophy of self-sufficiency gained varying degrees of support. This political ideal of ability to provide all necessities within the national border would certainly focus attention on the fact that an intensive agriculture depends on a supply of cheap nitrogen. As an organism seeks to make the correct response to a changed environment, so would the state seek methods to furnish this requirement for a nationalistic existence. Under these circumstances impetus to research concerned with fixation of atmospheric nitrogen by biological or artificial means would be expected. Another factor that probably contributed to the rapid development during the twenties was the preeminent position of all natural sciences. Scientific achievements of the war had so appealed to the mass mind that they turned hopefully to the scientists for the means to rescue them from the complex political, social and economical difficulties which ushered in peace. Popular support for all research was to be had even in those countries whose financial status was precarious. Later, disappointed with scientific leadership, the mood changed and scientific programs suffered deflation.

In America the factor of economic prosperity, absent in other nations, undoubtedly played a rôle. One result of this prosperity was an expansion in the budgets of institutions devoted to research—it is probable that many experiment stations and universities undertook work in fields that ordinarily would be classed as "marginal." With the onset of the depression came tightened purse-strings and administrative frowns on research concerned with production aspects of national economy. The effect on the literature curve of nitrogen fixation by leguminous plants is unmistakable; the similarity of the curve with that of 50 representative stocks on the New York exchange for the period from 1923 to 1933 is readily apparent.

Although the caliber of much of the work reported during the period of prosperity must await future evaluation, the stimulus to production resulted in at least one desirable feature, *viz.,* the broadening of research programs. The practical problems which dominated the war period were stressed comparatively less and some of the more fundamental aspects were attacked. These included a complete reinvestigation of the strain variation problem and of nitrogen fixation by the bacteria apart from the host plant; new fields tentatively explored were detailed studies on the biology of the organism and on the biochemistry of the fixation process.

As an example of the manner in which theoretical studies often lead to conclusions of great practical significance may be cited the progress of the so-called "strain variation" problem. About 1920, work was started at the Wisconsin experiment station on a fundamental study of physiological differences in strains of the organisms isolated from different plants of the same cross-inoculation group. The research eventually demonstrated that mere inoculation with organisms of the proper group did not insure benefit to the host plant, since ability of different strains to fix nitrogen after invasion of the plant varied widely, some strains being definitely parasitic on the plant. This fact provides added reason for the use of pure cultures of known efficiency in order to insure maximum returns and at the same time emphasizes the need for exact control in the distribution of artificial inoculation.

The discussion has been restricted to *number* of papers which may not be the best measure of production. Because of differences in editorial policies or in methods of publication, the size of papers in various countries may vary considerably, hence the number of papers per year may be a fictitious criterion of research capacity. A plot was made of the mean size of papers since 1883; the data showed that on the average 60 per cent. of the papers are less than 10 pages in length, 20 per cent. are from 10 to 25 pages; from 10 to 15 per cent. are from 25 to 50, and the remainder are over 50 pages. The percentages did not vary considerably over a period of 50 years, although a preponderance of small papers featured production during the decade from 1885 to 1895. Since many of these were from Germany, a plot similar to Fig. 1 was made in which the output in number of pages per nationality was used as the ordinate. The resulting graph was little different from that given in Fig. 1; the peak during the period from 1885 to 1895 was not quite so pronounced and the German dominance from 1885 to 1910 was less marked.

The Extended Population Curve

The treatment so far has been a more or less empirical description of the growth of a body of literature together with observations regarding possible forces which influenced the growth. It is of interest to determine whether or not a quantitative expression of this development can be obtained. The mathematical curve that might be expected to describe the growth would be a representative of the logistic, or population, family. These curves describe phenomena which behave in the following manner: (1) The rate of production at a given time is proportional to total production (or change in rate of production is proportional to the latter); (2)

there is some upper limit to the rate of production.

A priori these considerations would appear to apply to the development of the literature of a given research, *i.e.*, the production during a given year should be roughly proportional to the total produced, but a limited rate of production would be eventually reached. The first of these follows from the fact that the more publications which appear, the larger is the audience that is likely to become interested in the problem; likewise the tendency to follow the crowd is evident, even among scientists. The second condition, *i.e.*, the attainment of a limit, takes place when the field becomes saturated; new contributors are no longer readily attracted because of either dearth of problems or difficulties in publication.

Mathematically[2] these conditions are expressed:

$$\frac{d^2N}{dt^2} = K\frac{dN}{dt}\left(A - \frac{dN}{dt}\right)$$ which integrates into

$$\frac{dN}{dt} = A - \frac{A\,(A-\alpha)}{A-\alpha+\alpha\, e^{kat}}$$

A—limiting rate of production
N—total number at time t
α—constant representing production at $t=0$
K—proportionality constant

Tamiya[2] has shown that the literature devoted to research on the fungi *Aspergillus* follows quite closely a curve of this type.

The data of Fig. 1 were replotted and by trial and error the constants evaluated for the arbitrary origin t equals 0 at 1860 and α equals unity. Since even a casual inspection shows that in no case will the fit of the points be close to a smooth logistic curve, no attempt was made to secure the "best" curve in the least square sense. The population curve that appears to best fit the data is shown in Fig. 2. It is apparent from a study of this figure that the points follow the indicated curve in a general way, but that the fit leaves much to be desired. The worst departures from "theory" occur at: (1) Following Hellriegel and Wilfarth experiment; (2) the world war; (3) 1928–1929. The explanation of these peaks and valleys has been already indicated.

It appears that the subject of nitrogen fixation by leguminous plants is so intimately connected with the business of living that the research in this field is rather sensitive to upheavals in the political and economical world. The subject-matter is so closely intertwined with the larger problem of production that support of research at a given period is a function of current economic

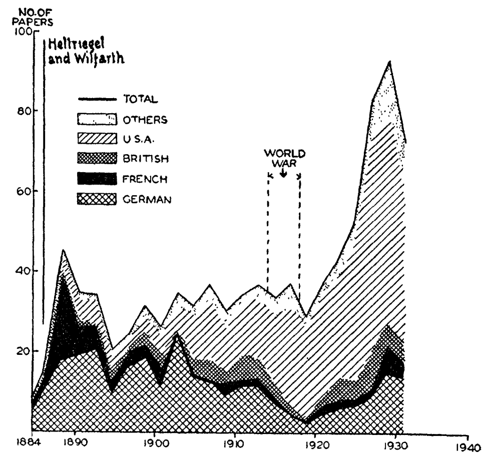

FIG. 1. THE DISTRIBUTION OF PUBLICATIONS ON NITROGEN FIXATION BY *Leguminosae* SINCE 1884.

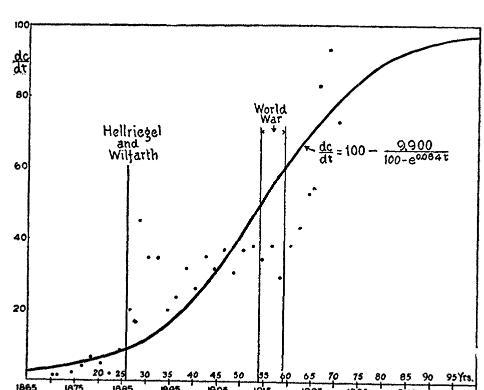

FIG. 2. GROWTH CURVE OF THE LITERATURE ON NITROGEN FIXATION BY LEGUMINOUS PLANTS.

[2] *Botanical Magazine*, 45: No. 530, 1931.

or political theories as well as the "natural" inclination of a given scientist to work in this field. In the theoretical development of the equation it was postulated that this inclination was conditioned only by magnitude of previous work and by proximity to the ultimate limit. Because of the effect of superimposition of external on the internal factors governing development, the growth appears to be more lawless than is really the case.

On the other hand, the literature of research with *Aspergillus*, an organism used mainly in physiological and anatomical studies, reflects academic interest primarily and is much less responsive to affairs of the world outside the laboratory. In such a case the fit of the observed with the "theoretical" might be fairly close, as was actually found.

Another influence which has caused occasional departures from the smooth curve in the case of the literature of nitrogen fixation is the ability of certain leaders to stimulate research. Attention has been called to Hellriegel and Wilfarth in Germany and to Atwater and later Moore in the United States. In recent years Virtanen of Finland has done much to accelerate the research in eastern Europe, with the result that the work of *Others* has become an appreciable part of the total. However, the impetus given to research in the Union of Socialist Soviet Republics since the revolution, a political event, has also been a factor in increasing the output of the unclassified nations.

In conclusion, it appears from the "smoothed" data that the research student of the future can look forward to an annual production of approximately 100 publications a year in this field. This limit of production seems likely to occur about 1965 to 1970. Likewise, the total number of pages to be mastered each year will be from 1,500 to 1,600 before the harassed student may look for relief from an ever-increasing annual load.

It is highly desirable to apply proper correction factors to the foregoing estimates in order to compensate for the apparent powerful influences on the research exerted by wars, depressions, revolutions and individual leadership. Until the effect of the external environment can be mathematically expressed in our equation of growth, at least as a first approximation, predictions based on a theoretical curve may or may not be more reliable than those found in a daily racing form.