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WHAT IS STATISTICS?[1]

WHEN one assembles the phrase: "What is Statistics?" he must expect first to defend his grammar. Should he not ask: "What are statistics?" We do not say "What are Mathematics?" or "What are Kinematics?" Statistics in the original and plural sense means collections of data, first data of the state and by derivation any kind of data. In this sense a statistician is any person who collects such material or who discusses it by any method or without method. It is as though by mathematics we meant collections of mathematical tables or formularies and by a mathematician one who gets the material together. Such usage is justified by custom for statistics and statisticians but not for mathematics or mathematicians. It represents a wide difference of attitude toward the two subjects and the workers in them.

Another difference is this. We see many college books appearing with titles such as "Elements of Statistical Methods," written chiefly by economists. We do not find the book catalogs flooded with texts on "Elements of Mathematical Method" or introduction to biological method. Why? May it not be that mathematicians and biologists, though interested in the technique of their fields of study and instruction, regard those subjects primarily as sciences in which the more important element is really the scientific viewpoint, the principles of the science in question, whereas teachers and writers of texts on statistical method believe that their subject is not a science but a technique? Which group is the wiser, or are mathematics and biology on the one hand and statistics on the other such contrasting entities that each group is wise in its own conceits?

If a great institution like the Rockefeller Institute for Medical Research desires to make outstanding contributions to biology it perforce incorporates with its staff one or more outstanding biologists. It does not consider adequate the purchase of a few mediocre texts on biological technique, to which a staff not especially trained in biology may turn to find the biological method and apply them in its work of advancing medical science. In such institutions we find also trained chemists, not merely reference texts to chemical methods. And this even though every investigator in medical science has a not inconsiderable training in both chemistry and biology. Despite such training, and perhaps because of it, he does not trust himself unaided to extract from the manuals of those

[1] Read at the Rockefeller Institute, April 8, 1927.

sciences and to apply the methods which he needs in his work.

Yet in the past and perhaps even to-day many leading institutions do seem to consider that the statistician is not necessary to them for the discussion of their statistical problems and appear content to have the members of their staff dip into some statistical manual, extract what they may of some ill-comprehended statistical method or technique and believe or hope themselves competent to elicit the proper inferences and avoid the improper ones by the routine application to their data of these miraculous extracts. That is why I ask: What is statistics, not what are statistics, nor yet what is statistical method? And I venture to suggest in a tentative, undogmatic sort of way that it is largely because of lack of knowledge of what statistics is that the person untrained in it trusts himself with a tool quite as dangerous as any he may pick out from the whole armamentarium of scientific methodology.

By the use of statistics in the singular, like mathematics or kinetics or physics, I intend precisely to direct the emphasis toward the principles of the science, toward its significance for human thought, toward its meaning. And I shall ask you to think of statistics as dealing with realities like physics, not merely as a refined system of logic like modern abstract mathematics. Statistics has its abstract side in the theory of probability, just as physics has in its higher mathematical reaches. But as the significance of physics can be well discussed with a very simple mathematical technique, so statistics may be understood without any of the higher branches of the theory of probability, without any mathematics beyond precollegiate algebra, but it can not be understood without thought.

Thinking is apparently a painful process. We do not naturally like even to ponder our definitions. Some of us seem but dimly to recognize that there are such things as definitions. We use words in a vague sense by habit and often in some ignorance of their denotations and in complete ignorance of their connotations. Yet not statistics nor any other science can replace or even exist without the science of language and of logic. Newspaper headlines are not a safe guide to a scientific glossary. You pick up a paper and read: "December wetter than usual." What does that mean? What is the true significance of the word usual? Should not the headliner have written "December wetter than the average"? What is the difference between the usual and the average? What is meant by an average, anyhow? This simple headline contains a deal of statistics. Again we read: "Coolidge likely to run for a third term." What does third mean? And likely—this seems a statement relative to probability, or did the writer really mean liable?

Next in importance to an appreciation of the meaning of words and of the significance of ordinary logical processes, the statistician should place the understanding of the simple notions of probability. The reason for this is that he desires to pass from the particular to the general. Now a general or universal proposition may be postulated but can never be completely proved. This means that unless we take a universal as a tenet of faith we can consider it as established only to a presumably high degree of probability and only within certain more or less restricted limits. If we say that the specific gravity of urine is 1.020 we make a universal statement, but it is not true. If we say that the specific gravity of urine lies between 1.015 and 1.025 we again make a universal statement, which again is not true in fact, though it is less false. The reason we need the knowledge of probability is to have a better way to state our universals or generalizations and a more precise understanding of what they mean.

To persons brought up as most of us are to take our knowledge chiefly on authority rather than to get it by inquiry it is at first a great shock to learn that a statement involving probabilities is more true and more precise, in brief more significant, than a categorical statement. But without such an understanding we are unable as scientists, other than as scientific cataloguers to commence to make answer to Pilate's question: What is truth? I may illustrate by an imagined example. Suppose there is an infection among a population of animals. The incidence is partial. We wish experimentally to determine what that incidence is. How do we proceed?

First, we may conceivably test all the animals. If we do this we come to a definite statement of the percentage affected (errors and omissions of observation excepted). The result is a particular fact, it is not a generalization. As we have used up, by hypothesis, our total universe of animals there is no need of generalization. But if the universe is large it would be impracticable to test all the animals. We must resort to sampling. Suppose we select at random a dozen animals and find eight infected and four free of infection. Then our particular is just that, namely, of that sample of twelve there are eight infected and four not. Shall we pass to the conclusion that two thirds of the animals are infected? Assuredly not. We could not expect all samples to run exactly alike. Suppose that we make the tentative assumption that the incidence is exactly two thirds in the total population. What would be the results of repeated selection and observation of random samples of twelve?

We are back to the simplest type of probability-problem. If $p = 2/3$ be the chance of infection and $q = 1/3$ be the chance of freedom therefrom, then $p \cdot p = p^2$ will be the chance of infection of both of two animals A and B, $p \cdot q$ the chance that A is infected and B is free, $q \cdot p$ the chance that A is free but B is infected, $q \cdot q = q^2$ the chance that both are free. The analysis may be made for twelve instead of two. There are thirteen possibilities; all twelve may be infected, eleven may be infected and one free, ten infected and two free, and so on down to none infected but all free. Each of these possibilities has its own probability. The respective values are:

Selection	Chance	Chance	Chance	Relative
12– 0	p^{12}	$\frac{4096}{531441}$	.0077	.03
11– 1	$12p^{11}q$	$\frac{24576}{531441}$	.0462	.20
10– 2	$66p^{10}q^2$	$\frac{67584}{531441}$	.1272	.53
9– 3	$220p^9q^3$	$\frac{112640}{531441}$	.2120	.89
8– 4	$495p^8q^4$	$\frac{126720}{531441}$	.2384	1.00
7– 5	$792p^7q^5$	$\frac{101376}{531441}$	.1908	.80
6– 6	$924p^6q^6$	$\frac{59136}{531441}$	.1113	.47
5– 7	$792p^5q^7$	$\frac{25344}{531441}$	.0477	.20
4– 8	$495p^4q^8$	$\frac{7920}{531441}$	.0149	.06
3– 9	$220p^3q^9$	$\frac{1760}{531441}$	.0033	.01
2–10	$66p^2q^{10}$	$\frac{264}{531441}$	.0005	.00
1–11	$12pq^{11}$	$\frac{24}{531441}$	.0000	.00
0–12	q^{12}	$\frac{1}{531441}$	.0000	.00

The details of the calculation may appear complicated, but the results at least are simple and typical. The actual chance of finding two thirds infected in a sample of twelve taken from a universe in which the chance of infection is two thirds is only .2384, less than one in four. You have three chances in four of not realizing in any particular sample the proportion in the universe; your chances are almost as good to realize a division 9–3 or 7–5 as 8–4, and you have about half as good chance to realize either 10–2 or 6–6 in the sample. With correspondingly harder luck you may get complexions in the sample still further from that in the universe.

Clearly this hypothetical case may be reversed. Instead of postulating a division in the universe with twice as many infected as free, and calculating the chances of samples of twelve of different complexions, you may admit the observation of eight infected and four free in the sample and ask what may reasonably be the proportions in the universe if this observation be not too unreasonably rare? The answer requires the solution of a quadratic equation, a problem in precollegiate algebra, and the formula for the mean error (standard deviation) of sampling $\sigma_p = \sqrt{pq/n}$, which may be proved by elementary algebra. The work will not be given here, the result is that there is a probability of about 1/2 that the proportion in the universe will lie between 16/28 and 21/28, *i. e.,* between $4/7 = .57$ and $3/4 = .75$ and a probability of about 2/3 that the proportion of the universe lies between .52 and .78.

This type of statement of the inference assumes that the experience realized in the sample is not too anomalous. The inference might better be put thus: If the proportion of infection in the universe lie between .52 and .78 then the experience $2/3 = .667$ in a random sample of twelve is not so unusual as to be realized less than twice out of three trials, or if the incidence of infection in the universe is less than 52 per cent. or more than 78 per cent., then realization of a random sample of twelve will be rarer than one chance in three. The enigmatical little word random deserves attention. We mean that the sample of twelve is merely picked out by chance. There is nothing to prevent any particular random sample from having a very rare complexion. In fact the very idea of randomness implies that once in a while the sample will be far from typical. One does have hard luck. When one generalizes from a sample he simply has to assume that his luck has not been too bad, that his sample is really fairly typical.

Now in actual life sampling is not an entirely random process. Suppose we are dealing with plague-infested rats and want to estimate how widespread their plague is. We go out and get twelve rats. How do we know that it may not be far easier to get sick rats than well rats or contrariwise far easier to get well rats than sick ones? We do not know. The processes of getting the sample may involve systematic errors. Quite in addition to the difficulties of ensuring randomness. The sample may accidentally be atypical or it may be systematically unsound by virtue of the physical limitations imposed by realizing the sample. What any statistician has to affirm before he may draw his inference on the universe is that his sample is neither atypical nor unsound. No amount of theory will ensure this, but only an adequate experience or sense of the eternal fitness of things whereby he may judge that in fact he has a fair randomness in his method of selection and has not had hard luck in the selection.

I have spoken of the necessity of knowing the meaning of words and appreciating the value of logic. Then, next, of the importance of understanding the elements of the theory of probability and realizing the ways in which chance may fortuitously or sampling may systematically mislead. I will give a further illustration of a probability calculation. Suppose that we are sampling plague-infested rats and pick out twenty-four rats. Let us turn them over to a technician to examine and assume that for some reason or none the technician examines them by twos instead of singly, and finds eight of the twelve sets of two infected, but four free. What is the inference? As four pairs are free, at least eight individual rats are free. As eight pairs are infected, at least eight single rats must have been infected. The incidence must be between one-third and two-thirds inclusive in this particular sample of twenty-four. Can we say more? Let q be the chance for freedom of infection in the universe. Then q^2 is the chance that both of a pair be infected. The experience gives $q^2 = 1/3$ or $q = .58$ and $p = .42$. The probability is that in this sample the infectivity which must lie between one-third and two-thirds is actually much nearer the former figure, namely, $p = .42$, and that ten of the twenty-four rats were infected. We may further superpose on such a calculation as this a discussion of variations of sampling, as above, and thus elicit the maximum information with respect to the constitution of the universe.

But there is one other consideration of very great importance with which the statistician should be familiar, namely, the analysis of association of attributes. We are always asking about the connection of this defect with that. Do men tend more than women to schizophrenia? Do cardiac and renal disorders tend to come together? Does like tend to marry like? Are there anthropologic types which run to certain disorders? These are more complex universes than the simple universe of infection or freedom from it. We have here a double dichotomy or an even more complicated situation. Let us take the usual case, the simplest, of a universe of double characters A and B where for each individual of the universe we may say that he has the character A or fails to have it and has the character B or fails to have it. There are now four possible kinds of individuals in our universe.

1° Those with both characters A and B, say (AB)
2° Those with A but not B, say $(A\beta)$
3° Those with B but not A, say (αB)
4° Those with neither A nor B, say $(\alpha\beta)$

The logical analysis of the universe can not be made, because the constitution of the universe can not even be specified, without a knowledge of four numbers, *i. e.*, the numbers in each of the four classes of the doubly dichotomized universe.

Time and time again people bring me material for statistical discussion where no discussion is possible but to point out that they have not all four of the numbers necessary to determine an association; often they have three of them, sometimes only two, and once in a while only one. No amount of statistical technique will close this logical hiatus. A vast amount of material avails little if it be not of the sort needed. A statistical clerk may assemble data, but a real statistician, well in advance of making any statistical analysis, must discern what data will be necessary to suit his inferential purposes. Any one can make a survey, but few seem able to lay out one that will be fruitful. Hindsight is better than foresight; it is not easy in advance to bethink oneself of what he may need. Thinking, like loving, is a painful thrill. I could extend the discussion of association with illustrative examples and amplify it by calculations of sampling errors to determine whether or not, in the particular examples, the associations were significant; but I will content myself with referring to the treatment in Yule's "Statistics" and with the statement that most of our texts on statistics say little or nothing about this most fundamental matter.

As in my course in vital statistics at the Harvard School of Public Health, so here in my brief hour I am trying to lay the stress on basic ideas without which detailed calculations become but empty, if impressive, virtuosity. In my own work I try not to permit myself the ease of routine until the figures themselves have been critically examined. Often relatively simple methods are finally adequate, and if so, so much the better. It does not pay to build too high on doubtful facts. Some years ago I became interested in the problem of computing life tables for native and foreign-born whites in city and rural districts. The Census Bureau kindly supplied the specific rates at five-year periods. I did not like the looks of the material. Consider it for a moment. The rural native white males have lower death-rates at all ages than those in the city until we reach the tenth decade where the figures become somewhat erratic, due perhaps in part to small numbers and in part to poor age reporting. Likewise the foreign whites do better in rural than city districts. These observations are both as might be expected. But the foreign city whites under forty-five years of age have lower death-rates than the native whites; whereas the rural foreign whites have higher rates than the rural native whites. The facts as supplied are probably correct, but are they significant for a life table calculation? What is a life table? It is a complicated mathematical function of the specific death-rates. And what is it for? It is to give the expectation of life at any age and to represent the vital condition in a stationary population.

What is the importance of the conception of a stationary population? It is to get at the biologic facts about a population independent of the actual age distribution of the population. Thus the average age at death in New York State other than New York City in 1923 was fifty-one years and the general death rate was 14.8 per 1,000. Now if the deaths just replaced the births in numbers and if each age group were just replaced by the lower age group less its deaths so that the population were stationary, then at a rate of 14.8 per 1,000 per annum it would take $1{,}000 \div 14.8 = 67\frac{1}{2}$ years on the average for a given group of births to die out. In a stationary population a death rate of 14.8 means an average age at death of $67\frac{1}{2}$ years, not 51 years as was actually the case in New York State in 1923 with its non-stationary population. Conversely, an average age at death of 51 years in a stationary population would mean a death-rate of $1{,}00 \div 51 = 19.6$ instead of 14.8. There is thus a great difference between figures in a stationary and in an unstationary population. The differences would be far more striking in New York City than in the rest of the state. Now the complete expectation of life at birth, which may be obtained from the life table, purports to be the biologic average age at death independent of population distribution and 1,000 divided by that age the true biologic death-rate, and there can be little doubt that the life table does improve on the average age of death or the general death-rate as an index of length of life.

That which I wish especially to point out is the fact that the life table does not actually give a true measure of the biologic situation. In constructing the table we use as of a certain date the specific death-rates by age. We may consider the rates which are found in the state report for New York, 1923. The age group 60 to 64 has a death-rate of 41 for the city, 31 for the rest of the state and 35 1/2 for the whole state. Except as the persons now in that group have moved into the state they have lived through in their youth the very high infant mortalities, the high child mortalities and the high mortalities of early adult life which prevailed from 1865 to 1890, times when the infant mortality was three times as high as at present, the death-rate from tuberculosis as high as three to four per thousand per annum (four times the present figures), those from diphtheria and scarlet fever over one per thousand (ten times the present figures) and typhoid fever perhaps one per thousand. If there has been a natural selection of the death-rate in man during this period, the weaklings have been weeded out and the age group 60 to 64 should be a stronger group than if its weaker members had not been so decimated in early life. On the other hand, if these diseases of early life which swept the population now in this age group and took so many of its numbers, wounded and crippled many of those that were not killed and left them with serious sequelae, the present survivors in the age group 60 to 64 may be weaker than if they had not passed through the fire. Whether the net result is in favor of the natural selection of the death-rate or opposed to it I have no idea, and in the absence of knowledge on this point we can not maintain, even though we do tacitly assume, that the death-rate of 35 1/2 in this group is compatible with the low specific death-rates which we now find in early life. What our life table calculation does is to use the age specific death-rates as though they were those in a stationary population and upon that assumption to calculate the characteristics of that stationary population. It is important to realize the existence of all these tacit assumptions even if we believe that they do not have a material effect upon the result and upon the validity of our practical conclusions.

We may now return to the specific death-rates of the rural and urban native and foreign white populations and ask whether a calculation of life tables would be *a priori* worth while. The city rates for natives are higher than for rural; the same is true for foreign. We have considerable experience to indicate that really urban death-rates are, or until very recently have been, higher than rural death-rates, and that they are higher all along the line from infancy to old age, so that the net effect of the less favorable urban environment would seem to be more crippling than selective. Furthermore, we find that the foreign rural whites do worse than the native rural whites. This does not seem unreasonable; they may have difficulties of acclimatization, whether economic or meteorologic, and they may have been subjected to less favorable conditions in earlier life. To offset this we have to recognize that our immigrants may be a favorably selected lot, more robust than the compatriots they left behind them and possibly to be expected to excel our natives with whom they are compared. When we turn to the foreign city whites in contrast to the natives we find that to the age of forty-five they do better than the natives. Is this a true biologic finding of the sort we should desire to incorporate in a stationary population or is it some accident of selection? This is the primary statistical question. So long as the specific death-rates are let stand for what they are, any doubt with respect to the proper answer to this question remains a doubt and as such subject to discussion. Once these specific death-rates have been incorporated into a life table we have added to them a whole handful, I will not say a whole brainful, of tacit assumptions, possibly ill-realized and now easily overlooked, and we may get out of our life table more the consequences of those assumptions than those of the original biologic facts.

It seems to be a curious fact that careful logical thinking, and even the appreciation of the meaning of words or the significance of definitions, is considered to be mathematics and that very careful attention to what one means is rated as higher mathematics irrespective of whether any formulas are used. Thus if we teach a person the arithmetical routine of calculating a life table from age specific death-rates we are not forcing him to indulge in mathematics other than the most elementary, but if we ask him what he really is doing or what a life table really is and whether by doing what he is doing he is really getting what he purports to be getting, then we are submitting him to that particular cruel sort of torture known as higher mathematics. This attitude of mind was perhaps illustrated in an earlier terminology whereby the physical sciences were called natural philosophy and the biological science natural history. The latter were the story of nature; there were voyages to go, things to see and romances to relate. This was not higher mathematics and was balm to the soul. The physical sciences were, however, not the story of nature but the wisdom thereof; there was thinking to be done, analyses to make and proofs to give. This was higher mathematics and was a test of the intellect. We know better now. Physics and astronomy have become metaphysics and romance; biology has become hard-boiled. Physics consists of schizophrenic phantasy or manic ecstasy with a maximal obfuscation of complicated mathematical technique; biology means close thinking, with a minimum of formal mathematics. Statistics is higher mathematics—let it go at that—and the statistician is a fellow who has to find out within practical inferential limits something of real significance. This requires some aptitude and some training, and of course it is not easy to supply either; both must grow, and that takes time. For these reasons organizations like this are coming to realize the importance for their work of adding the statistician to the biologist and chemist.

If time still permits I should like still to amplify and to emphasize the use that a statistician may be to you in experimental medicine. I have spoken for the most part of the statistical thinking to be done on observations already made. In the brief reference to association of attributes there was mention of the difficulty of foreseeing those elements which logically must be known if we shall be put in a good position for inference. This applies also to the layout of an experimental program and because we do not have either the time or the resources to assemble by the experimental method the mass of material that may be collected in the general observational field, it is all the more important that we plan our work so as to get the most from the fewest experiments. As we are usually limited to small samples we have to be particularly circumspect. On this subject the best reference seems to be R. A. Fisher's "Statistical Methods for Research Workers." The title should not mislead you into feeling that Fisher is really interested in technique of a routine sort, nor divert you from concentrating your attention especially upon such trenchant paragraphs as that on page 111, where the author states: "The need for duplicate experiments is sufficiently widely recognized; it is not so widely understood that in some cases, when it is desired to place a high degree of confidence on the results, triplicate experiments will enable us to detect with confidence differences as small as one seventh of those which, with a duplicate experiment, would justify the same degree of confidence." This statement is very important. Many of you know that the precision of statistical data increases only with the square root of the number of observations, and from that a careless thinker might conclude that triplicate experiment would increase the precision only as $\sqrt{3}$ is to $\sqrt{2}$ or less than 25 per cent., whereas Fisher's statement shows that with proper planning the increase may sometimes run to several hundred per cent. He reverts to this matter on p. 224 in a section upon the technique of plot experimentation, but the problem is the same as that of experimental medicine. It is this pregnant philosophy of experimentation which is the most significant part of Fisher's book for your purposes and which you may easily overlook or fail to grasp. True meaning is always hard to grasp, but once understood it stands by long after detail has vanished.

Logic and definitions, probability and universals, association of attributes and multiple universals, real significance *versus* arithmetical operations, planning experimental procedure, these are five considerations which have to be borne in mind in the kind of work you are doing and the last is perhaps both the most difficult and the most helpful, the one which would most repay in its saving of time any expenditure you might make to understand it. May I close with the suggestion that, as you now have on your staff in your department at Princeton one who thoroughly comprehends planning experiments, you get him some time to follow up these generalities of mine with an intensive discussion of that special statistical subject?

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WHAT SHOULD WE DO WITH A HARVEY OR A LAENNEC?[1]

DURING the greater part of the year most of the members of our society are integral parts of various medical schools. Our chief interests are devoted to academic medicine. To-day, however, we meet in a city which is far from all medical schools and our thoughts properly turn to the larger problems connected with research in clinical medicine. The American Society for Clinical Investigation is composed of clinicians who have been selected because they have shown ability in the fields of research. We have as our chief concern and as our chief responsibility the perpetuation of the research spirit in clinical medicine. Therefore it is our duty to sound a note of warning whenever we believe this spirit to be in danger.

With the tremendous growth of our medical clinics and medical schools we are in danger of being swamped in this process of expansion. There is a great demand for young men to fill the various positions in these large clinics and the demand falls chiefly on the membership of this society. Almost every man here is planning a career of academic medicine in which he hopes to be able to continue that scientific work which alone qualified him for membership. What are his chances of success?

Suppose we had in this country a young William Harvey and a young Réné Theophile Laënnec. What would we do with them? We would certainly give them a good training in their medical schools and hospitals and we would provide them with fellowships or minor teaching positions which would allow them to devote most of their time to research work until they reached their early thirties. Then would begin the critical period. If they showed aptitude for executive work they would be given increasing responsibilities in the management of the clinic and medical school. If they showed aptitude for teaching they would have to spend more and more time with the students. If they were successful as clinicians they would have to look after not only their ward patients but also an increasing number of friends, nurses, and doctors' wives. They might in addition have private patients. Having already attained distinction in research they would be consulted by younger men seeking advice and many visitors seeking instruction.

At this stage Harvey and Laënnec would be put in charge of departments of medicine. With it would come an increased amount of paper work, budgets, schedules, and numerous committee meetings. I do not wish to discuss the question of full time versus part time, but it is quite possible that the university might stop their private practices which brought them in contact with instructive manifestations of disease in order that more time might be devoted to budgets and committee meetings.

From now on Harvey and Laënnec would be penalized for each new success in administrative, clinical or pedagogical ability. If they were really able administrators their departments would be increased in size and embellished with sub-departments until they were running an almost complete medical faculty with anatomists, physiologists, bacteriologists, etc., all under the department of medicine. If they were really distinguished clinicians great pressure would be brought to bear until they were taking care of many persons who had influence with the university. If they were public-spirited men desirous of advancing the cause of scientific medicine and public health they would be placed upon numberless committees in the county or national medical societies.

What would become of the discoveries that made Harvey the founder of modern physiology or the pathological studies that made Laënnec the founder of modern clinical medicine? How much time would be left for quiet work and contemplation?

You may answer that these men would have to take care of themselves and reserve a fair proportion of their time for their own work. How can a man best do this? A clever man would take care to make himself stupid at executive work and tardy at all committee meetings but what sensible man with the instincts of a doctor allows himself to be clever? A logical man would set aside a certain portion of each day and steel his heart against all interruptions; but what thoughtful doctor is ever logical with regard to his own time? Suppose he did reserve a morning for himself and in the midst of it a resident came to him saying that they were about to perform an autopsy on that interesting case with the big spleen, or an assistant blew in full of excitement with the result of his experimentum crucis or suppose there came with many bows that distinguished visitor from Germany or Japan.

The situation would not be serious if it only meant the loss of the original investigations of a few such men. There is a more serious loss; that is the gradual atrophy of the research spirit through disuse. The true research spirit can be maintained only by means

[1] President's address, American Society for Clinical Investigation, Atlantic City, May 2, 1927.