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THE FACTOR OF SAFETY IN RESEARCH[1]

ONCE in the drear dead days unfortunately still fresh in memory the head of a great institution for the aid of education wrote, with reference to research, these words: "In the last two decades more sins have been committed in its name against good teaching than we are likely to atone for in the next generation." Evidently the time of reformation had not arrived when this disparagement was uttered, for some ten years later the same pen recorded history as follows: "Much of that which has gone on in American universities under the name of research is in truth only an imitation of research."

To some of you, more than commonly zealous in support of investigation and with a background of rural experience, these words may come with memories of the odor of new mown hay and visions of waving yellow fields and the reflection that excessive heat sometimes causes mental aberrations. For the quoted passages could have come only from an annual report, naturally written just after the end of the fiscal year; but unfortunately for this simple explanation, the fiscal year of the institution in question does not end in June, and the derogation of research was conceived in the cool gray days of autumn. Moreover no charge of alienation of reason could be brought against the author of these rebukes that would not lodge with equal justice in other quarters. The chief executive of another great institution which has done and is doing as much in the field of research as any of its kind in America voiced a similar sentiment thus: "Quite too much attention is paid to those who when they make some slight addition to their own stock of information fancy that the world's store of

[1] Address of the President of the Michigan Academy of Science, Arts and Letters, March 29, 1922.

knowledge is thereby increased by a new discovery." It is not fair in this case to remove the quoted words from their context, which was really a commendation of research as a university function. But in view of the wariness with which such views are expressed in public, the slight stricture here permitted to appear is probably only the weather-worn outcropping of a stratum much more extensive.

Indeed, it is not an uncommon idea even among men actively engaged in investigation that there is a fearful waste of energy upon research that might as well be left undone. Has it not been pointed out in every discussion of cooperation in scientific work how much better would the world be served, could the labor now being frittered away upon unimportant matters be organized under the direction of some one capable of separating the wheat from the chaff? A few years ago Edwin Linton, in an appreciation of Spencer Fullerton Baird, who must have been a practical person to have deserved the commendation bestowed upon him, wrote: "I am led to wonder if the failure of science to influence legislation in the interests of the people is not to be charged to the propensity on the part of these leaders to shun the practical." Likewise the energetic chief of the Federal Bureau of Entomology, on looking through a collection of doctor's theses with the interests of his own bureau in mind, finds "that only a very small percentage of this output represents work which can be of the slightest use to humanity in its immediate problems regarding the insect world." "At present most of the best men are working away in their laboratories practically heedless of . . . the tremendous necessity for the most intense work by the very best minds on the problem of overcoming and controlling our strongest rivals on this planet." Had Dr. Howard been a physicist, or engineer, or metallurgist, he could no doubt have changed a word here and there and made the same statement with equal vigor. Those of you who every May have scanned a series of doctor's theses may wish to explain in some other way the fine despair with which he exclaims, "And how can we emphasize the prime importance of devoting our earliest attention to those problems which most immediately concern our well being."

These are not the only investigators who have views that substantially agree in regarding much research as wasted, and the number who are willing to express such views in public is presumably but a fraction of the number who hold them in private.

Research is not, it is true, alone in including much which is of no direct value. How much of literature could not be suppressed, to the advantage of author, publisher and reading public? Is there not much of so-called art which might never have been born, and leave the world happier for its non-existence? However, to recite the ills of sister lines of endeavor is not to cure or even excuse our own.

But are they ills? Is it to be in any wise deplored that the research by which John Brown wins a degree, or Professor Jones keeps his mind fertile while teaching, is not of the sort that promises to lighten the burdens of human society or increase its means of pleasure? At least, is it to be deplored to the extent that Student Brown and Professor Jones should have refrained from research if they were unable to fix upon a more practical subject of investigation? I believe it is not only *not* to be regretted that some pieces of research must seem trifling, but that the system under which we now operate, in which unimportant or perhaps in themselves valueless contributions are sometimes made, accomplishes a greater result than any system that could be devised under which such insignificant researches would be excluded.

Let me disillusion at once those who imagine that I am about to defend research on the ideal ground that truth for truth's sake is indeed practical, and that therefore any investigation which discovers a grain of that precious commodity is an economic gain. I would be willing to make such a defense if it were desirable to pitch the combat on so high a plane. But it is no time to wrestle with the angels above the clouds while the forces of evil are unvanquished in the valley. I have no intention of discussing at length what is practical. Perhaps one should regard general knowledge as the most practical kind. The elimination of *ennui* and of the loose habits formed in the periods of mental vacuity to which the ignorant and the merely technically trained are frequently subjected may result in improved health and increased longevity, and so be a highly practical matter. An engineer or a physician seldom reaches the economic heights of his profession without the engaging or compelling mental exterior which only a general education can develop to the full. One need not admit that only those things are practical which look practical to the world at large. One need not even admit that a thing is useless even if it could be known—as it can not—that no better food supply, or no decrease of hardship would ever come out of it. The conception of the practical which makes it include general knowledge is capable of strong support, but I shall not avail myself of it. I propose to accept, for the purpose of this address, the definition of the practical as given by the man in the street.

Nor have I any intention of deciding whether a practical education is the best one. So far as I can see, the whole science of astronomy might be forgotten, and my present daily life would go on about as before. I would enjoy the sunlight and profit by its energy as I do now. The seasons would follow one another in the same order if we were ignorant of the causes of their succession. The eugenic effects of moonlit and starlit nights would be as great as at present. Should the navigator who brings me comforts from distant parts of the globe get into trouble, that difficulty could soon be obviated. But one element of vastness in the thoughts of men would be gone forever, and it is not unlikely that experiences of other kinds would shrink in proportion. What physical advantage, gained by devoting to some applied science the energy now devoted to the planets, could compensate for such a loss?

It is not my purpose, however, to address myself to idealists, though no doubt that is the character of this audience. Arguments on a lower level are now much more urgent. We are all familiar with the investigations which have been undertaken for the simple purpose of discovering scientific truth, but which afterwards have led to results of the highest practical importance. Biologists know by heart the story of Professor Harrison who by painstaking experiments devised a means of keeping alive a small group of cells after removal from the body of the animal to which they belonged, and of watching them grow under the microscope. He was seeking to demonstrate a principle of morphology and development, and he succeeded to the satisfaction of himself and his coworkers. Neither he nor they considered the possibility that his method of tissue culture would ever be used to the obvious advantage of man. But this method was later used by a great surgeon, who kept tissues alive for years and who pointed out the possibilities which the method contained of disclosing the causes of death and thereby of prolonging human life. Thus the experiments first used to settle a disputed question of biology promise, in the opinion of many, to bring us nearer to that oldest of human goals, the fountain of eternal youth. The possibility of practical advantage to be derived from these culture methods weighed heavily in the allocation of the Nobel prize in medicine which was bestowed upon the surgeon. There should be no detraction from the credit due to one who has the vision to discover new uses for old methods, but rather increase of credit to the original discoverer. Professor Harrison would be the last to ask that the Nobel prize be transferred to him. All he would ask is that research in general be supported in a broad way which will occasionally make possible further practical applications.

Perhaps less generally known is the recent improvement of submarine cables, whereby their capacity is increased five-fold. Experiments extending over a long period of time had as their aim improved insulation which would prevent or reduce leakage. These efforts succeeded to a marked degree only after another worker, with the mere advance of scientific knowledge in mind, invented a new alloy having the desired insulating properties. The field of physics is full of such examples. The work of Maxwell on the electromagnetic wave theory led to wireless telegraphy; Roentgen's rays were discovered in the course of a piece of pure research; and, indeed, all of the early work on electricity was done in a spirit of investigation having no other object than to discover the truth.

What might have happened to these discoveries had they promised to emerge in an atmosphere in which it was considered that "much that has gone on in the name of research is in truth only an imitation of research," can only be surmised. Whether they would have been made in the laboratories of an institution where they stood a chance of being regarded as "sins against good teaching" so heinous as to demand expiation for a generation, may be doubted. Even if their worst prospective reception had been that of being regarded as slight additions to the discoverers' own stock of information and not an increase of the world's store of knowledge, their origin could hardly have been inspiriting to their author. One pauses to meditate upon the reasons for the long delay in the appreciation of the work of the Abbé Mendel in hybridization of garden peas, and of that of Willard Gibbs on the "phase rule," and to wonder whether even then men in positions of influence were convinced that they could "spot" in advance those things which were worth doing.

It would be hazardous to assert that cases as striking as the foregoing are common. Less spectacular examples are, however, not rare. Although many of the economically valuable applications of science to practical ends are directly made by investigators who are consciously striving to make those applications, it is probably in every case true that their success has depended upon previous discoveries not made with a practical aim in view. Some one has gone so far as to say that every discovery of science which has proven of economic use was first made as a contribution to pure science.

Justification of research along lines that promise no amelioration of man's condition must not, however, lie only in the possibility that the amelioration will result even without the promise. Some investigations must be carried on purely for the training of investigators. Until, by use of tissue cultures or an analogous procedure discovered by the pure scientist and then applied by others, some means of indefinitely prolonging life is discovered, new investigators must be developed to replace the old. New investigators are developed only by practice, and in practice they must solve problems. For this educational work a problem of small value often serves as well as a weightier one. Indeed, since first attempts often show the hand of the novice, it may be doing a real service to science to withhold the more serious problem for a second or later investigation. It is not reasonable, therefore, and perhaps it is not wise to insist that even the training of research students shall all be done on subjects that are in themselves of high value either directly or indirectly. Objecting to our system of traininig in research by means of small investigations that are not in themselves important is like proposing to abandon the study of arithmetic by means of problems on the ground that no one ever bought seven gallons of vinegar at twenty cents a quart, and that therefore it is a waste of time to discover how much the liquid cost.

To convince ourselves that the rearing of young investigators on a diet of insignificant problems is not inevitably fatal, and that it may even be beneficial, it is only necessary to look backward instead of forward, and gather assurance concerning the future from what has happened in the past. Did Pasteur, for example, learn the art of investigation on a problem that he foresaw was to be a lasting boon and cause of untold happiness to men? This being a presidential address, I will probably exhibit no greater degree of ignorance than is to be expected if I inquire whether the solution of a puzzling problem relating to the isomeric tartaric acids was by any one at that time held to be full of economic promise. Molecular structure we may regard to-day as of high importance, perhaps in some instances even in a practical way, but hardly in Pasteur's early manhood. That his researches were considered by his contemporaries futile, even from the pure science viewpoint, is plain; for when Pasteur's reputation had been established, when he was professor of chemistry, even when he was dean of his faculty, than which no higher honor presumably could come to a man of science, he was advised by Biot and Dumas, veteran chemists, not to waste more time on the subjects which were then uppermost in his mind. These investigations led, however, through the chemistry of fermentation to the bacteria of fermentation, and thence to the organisms of disease, and to-day the appreciation of the practical value of Pasteur's work is universal.

We may be told that Pasteur could have started midway in his career if some one had put him there at the outset by advice; and if we reply that there was no such person to advise him, we may be reminded that there are plenty of advisers to-day. These advisers are precisely the foundation on which those who decry the uselessness of many present investigations propose to build a system in which only useful and important projects are undertaken. Granted an abundance of omniscient advisers, their plan should work; but if these foundation stones prove defective, the structure resting on them will fall. How readily such advisers may be discovered and drafted into service is perhaps capable of computation. No doubt each person who proposes to eliminate uselessness in research has in mind at least one who is able and willing to undertake the task of elimination. Otherwise the proposal would hardly be made. One need, therefore, only count the number of those who would dispense with impractical investigation to determine the minimum number of advisers with which the system might start. Probably there are others having ability, but also modesty, who can not be immediately discovered. So far as I know no one has attempted to determine how much leadership a federation to prevent uselessness in research might count upon.

There is danger in this connection that the controlling factor of a career be misjudged. Careers are only occasionally guided by advice; for the rest, they are the product of evolution. Each step depends on what has gone before, and determines what shall come after. Granted the characteristics with which Pasteur's parents endowed him, his life proceeded naturally from one thing to another. One need not be a fatalist to conceive that the only way for him to end with proof of the germ theory of disease was to start with isomerism in tartaric acids. Had he been artificially set down at some mile-post on the way, without having traversed the preceding distances, it is questionable whether he could have been made to follow the same road, even with the help of advice from those who believed they were qualified to give it. Without the abiding faith that he was on the right road, which only his own previous work, not the suggestions of his elders, could give him, it is scarcely likely he would have persevered through the long periods of discouragement. To him who asserts that Pasteur could have been put upon the problem of pathogenic organisms in his early days and have reached the goal of his maturity at an earlier date, the only suitable reply seems to be the verse which might prove to have apostolic origin if the Scriptures recorded everything, "Verily, optimism hath its own reward."

Had Pasteur's hypothetical early start on pathogenic organisms failed to lead him to the present conception of the etiology of disease, what would have been the damage? Would the world simply have lost Pasteur, and never been the wiser, in the same manner as it has probably lost many another genius, perhaps through mistaken advice coming from those who were supposed to know? Could humanity have counted on a substitute for Pasteur, arising at an equally early date and arriving, either with or without advice from superiors, at the same conclusions as Pasteur reached? It is not likely. Failure to discover the truth by Pasteur would have been a calamity. His work would have been careful, painstaking. Everyone watching his later career would have recognized that his work on the theory of pathogenic organisms must have been thorough. But, owing to immaturity, or want of perseverance because he lacked the faith in his own hypotheses which only gradual development of them could insure, it had demonstrated nothing, its results were negative. Surely this would not have been an encouraging fact for any one else who conceived the germ theory of disease and contemplated efforts to prove its correctness. The oligarchy set up to guide research in useful directions would hardly have advised young men, or others, to enter that field. The fact that careful work by an able investigator, even if then young, had failed to find any proof of the bacterial origin of disease, could easily have damned the truth to a generation or more of undiscovery.

If any comfort is to be taken in the gloomy picture of what would have happened if Pasteur had, at the behest of some supervising agency, undertaken as a first problem something else than the isomerism of tartaric acids, and thereby missed the germ theory of disease, it must lie in the belief that a man of Pasteur's timber would have done great things in another field. But such a consideration does not answer the argument that his early work, practical or not, was a necessary training in order that his maturer work might be valuable.

Doubtless the case of Pasteur can be duplicated by that of other eminent scientists whose first research seemed to bear no relation to their later high attainments. Perhaps that is regularly true, except in the small number of cases in which by the laws of chance it is to be expected that preliminary work and eventual important discoveries shall lie in the same field. The fields in which the accomplishments of great investigators lie may thus appear to be matters of accident; but then, an accident is but the inevitable consequence of other accidents that have gone before.

If it is not fatal, but sometimes even useful, to start the new investigator on his way with a problem whose solution promises no practical improvements in human affairs, what is to be said of those who are mature in research? Probably most of these trained workers would be better satisfied with their showing to their fellow men, even if not more content with themselves, if they could be perpetually engaged on practical projects. Even if it be granted, as has been done in the introductory remarks of this address for the sake of limiting the discussion, that practically useful investigations are the only ones desirable, is it possible to maintain a system of research in which only practical things are attempted, and make it work? For various reasons the practical problem that suggests itself to an investigator may be one which he can not undertake. Lack of facilities readily accounts for many such cases, geographical position for others. The problem that seems most feasible may not seem highly important even from the pure science point of view. What is the investigator to do under these circumstances? Refrain from undertaking a problem which he feels sure is not of great value? Even if that means doing no research at all? Perhaps. But if he decides to keep on working, he may take comfort in the story of the foolish virgins, and reflect that in his small way he is keeping his lamp trimmed and burning even at the cost of some oil which seems wasted, until the bridegroom cometh with a problem that is more worth while. For nothing is so quickly fatal to research as interruption of it. This university furnished, for valuable war work, some investigators whose previous work was regarded even by themselves as of small value. I am not speaking of any of you here present. The gentlemen to whom I refer are in their laboratories to-night. They find the labors to which the great conflict introduced them so pleasurable, nay, even enthralling, that they have no time to listen to mere presidential addresses. The life of any eminent scientist of the present generation would probably furnish a further example of the *ad interim* value of unimportant research. At least this is true of those in my own field upon whom I have taken the trouble to reflect. They have engaged in continuous investigation, the continuity being due in part, in every case, to insignificant productions. It is very seldom, and then only under unusual circumstances, that a serious interruption is followed by a return to high productivity.

Nor must it be forgotten that many men who are engaged in research of minor value are the trainers of new investigators who may be more "lucky" than themselves. I think with profound respect of the professor of physics in a small western college who keeps working in a small way, who has never made a striking contribution, practical or otherwise, to his science, but who every year or two sends to a great eastern university a graduate student. Although these students are most of them still young men, they have done creditable things, some of them practical. Is it likely that the professor in the small college could thus inspire his students to a career of learning without the stimulus that comes from his own research? You may answer this question to your own liking, as I am doing. In a vicarious way, this man seems likely to exert upon his science an influence out of all proportion to the immediate significance of his own investigations.

My challenge to the critics of the present system of research to produce anything better does not rest on the idealistic argument that truth for its own sake is the highest aim of the scholar. This argument might not appeal to those for whom this address is intended, who, while not present in this audience, may yet receive the challenge. It rests on the demonstrated fact that many discoveries thought unimportant when made have proven to be valuable later, on the belief that new investigators are often as successfully prepared by unimportant practice problems as by more fundamental ones and with sometimes less danger to the progress of science, and on the assumption that the continuity of labor which problems of small value permit is conducive to aggregate high productivity. This is the system under which we now operate, a system which leaves the individual free, and which does not chide him too severely if he sometimes engages in insignificant labor. It is a system which provides for the doing of many services in order that some of them may prove valuable. Can it be improved upon? Quite possibly. Can it be improved upon by attempting to suppress all efforts that seem to have no significance? I think not. The principle of this method is one which has been widely adopted in other affairs of life and has been found good. Firing a whole cartridge full of shot in order that one ball may bring down the game is a recognized principle of the huntsman. Is the remaining shot wasted? It is. Is the system which uses cartridges of shot, most of which is wasted, an uneconomical one? Any hunter will tell you it is not. The bullets of a machine gun are mostly wasted, but the system as a whole insures hitting the mark. Drilling wells that never yield oil is wasteful; but the system of drilling numerous wells where there is a chance of striking reservoirs is a profitable one. Casting bread upon the waters, to return again sevenfold in the form of flesh of fish, would be much more profitable if all the bread, instead of being cast at random, could be put into the mouths of those fishes that were afterwards going to be caught, and denied to those that would later escape the net. But could such individual feeding be carried out? Not economically; not even at all. Casting bread upon the waters is the easiest and least wasteful way of obtaining a return. Hundreds of inventions are made for every one that fills an important place in human economy. Numerous excursions, genuine or spurious, were necessary before the north pole was discovered. Business concerns by the hundred are established and succeed or fail, but by only a few of them is economic progress made. Thousands of students must be gathered into colleges, so that a few scholars may be produced. Even presidential addresses are subject to the same rule. In order that a few of distinction may be produced, many that fall short of the goal must be written and heard. If presidential addresses must be had, trial and error is the only way to secure quality.

The factor of safety has been employed for æons in animals, which waste millions of eggs and spermatozoa to insure continuity of the species. Professor Jennings, in one of the brilliant presidential addresses to which reference has been made, pictured himself as the accidental product of union of one among thousands of eggs and one among millions of sperms, and congratulated himself on being with us. We congratulate ourselves on having him with us. Along with Jennings, it is true, we have to accept a lot of inferior persons. We even have to take those who decry research because much of it is useless. But these disadvantages, these wasted combinations, are what insure such as Jennings. Only a small percentage of seeds ever germinate, and fewer still ever mature. The entire struggle for existence is based on the principle that security and advancement are best secured through wasteful over-production.

So in research. To find radium, we must permit scores of fruitless efforts in chemistry. To invent the wireless telephone, there must be numerous investigations that concern humanity little or not at all. To discover the mechanism of heredity, some one must be permitted to do much that has little or no bearing either upon that or upon anything else worth while. The great advances of the theory and practical employment of electricity, of industrial chemistry, of immunity, of surgery, all have been made at the cost of much plodding and puttering. It is doubtful whether they could have been made in any other way.

The foregoing defense of the present freedom of the investigator is not to be regarded as a recommendation of still further freedom. It is not proposed that young investigators shall be delivered from all advisers. No muzzle is to be placed upon those who have comments to make upon the value of the work of their colleagues. Restrictions laid upon advice and criticism are likely to be as dangerous as restrictions imposed upon problems for investigation. All that is insisted upon is that no such advice or criticism shall carry with it any weight that is not inherent in the advice or criticism itself. Those in whose hands lies the power to make or mar the career of investigators should be exceedingly cautious how they create an atmosphere that seems in any way to discourage or limit the freedom of research. I have referred in my introductory remarks to several instances in which responsible officials have, in my opinion, transgressed in this regard. They are not the only ones, and there are other ways of committing the same sin. One of these ways is the appointment of an investigator to a position for the purpose of studying a certain problem. There comes to my mind one such appointment in a research institution. The appointee was, in his own words, "brought down here to study ———" —but to name the specific problem would be to name the institution. He did not feel free to attack another problem until that one was solved. It made no difference that he had come vaguely to feel that the problem would never be solved, or that other investigations would yield greater returns. By the terms of his appointment, his energy could be directed into other channels only with the permission of his superior officer. Such direction from above could be justified only in the case of an assistant or an investigator on temporary appointment, not in the case of a permanent colleague. Research in a general field may legitimately be the aim of an institution in the appointment of an investigator, and the appointee would naturally be one who had demonstrated an abiding interest in that field; but even in such cases, the progress of science demands that he be free from restraint.

Very different from such interference is the friendly advice of a teacher or the criticism of a colleague. Advice and criticism carry no concealed weapons. They are sometimes good, and to repress them eliminates the good with the bad. Indeed, good advice is more easily frowned down than is the bad. If my argument were regarded as against the giving of advice, and were taken seriously, those whose advice is best would be the most restrained by it. The greatest freedom of suggestion from all sources is advantageous, for advice is sometimes good, and to get what is good one must also hear the worthless. That is the reason for this address—and this statement may be interpreted in any way you choose.

To sum up, a successful system of research, even when the practical is the ultimate aim, demands the greatest freedom of the investigator. While direction from superiors may effect gains in limited fields, the losses entailed in the whole system are probably invariably greater. Great industrial concerns maintain staffs of workers whose tasks are assigned to them, and such startling achievements as the wireless telephone have resulted from their directed energies; but the responsible heads of these enterprises recognize that untrammeled research in pure science must precede and build the foundation for their labors, and some of these industrial institutions are now deliberately maintaining research workers in fields which promise at present no practical results whatever. The freedom which is insisted upon for the investigator will, it is expected, often lead him to problems that have no practical value, or even no great scientific value. But a system in which such liberty is a cornerstone insures a continuous output and a wide range of results. Among these results are most certain to be some, perhaps many, of practical value. Any interference with this system which would limit investigations to those of supposed importance would interrupt their continuity, limit the output, restrict the variety, and defeat its own purpose. The development of a scientific foundation is an evolutionary process. Man has never yet interfered very successfully with the great scheme of organic evolution, and there is no reason to suppose that he can propose a superior substitute for the evolutionary process in the development of science. Selectionists have practically abandoned the belief that they can create new things at will, and are content now to discover, preserve, and combine what already exists or what may come into existence without their aid. Practical scientists may well take their cue from the selectionists, permit investigation to take its own course, and choose from among its products such as seem capable of application.

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WHAT BECOMES OF THE FUR SEALS

THE census of Alaska fur seals in 1921 as computed by Mr. Edward C. Johnston, of the U. S. Bureau of Fisheries, amounted to a total of 581,457 animals, exclusive of 22,546 surplus males which were killed for commercial purposes. This is a low but substantial increase of 5.2 per cent. over the figures for 1920. The annual percentages of increase of the class of breeding cows since 1912 have been as follows:

1913	12.54
1914	1.06
1915	11.02
1916	12.99
1917	9.44
1918	11.63
1919	9.97
1920	6.59
1921	5.22

Since it is this class which is the controlling element of the herd it will be instructive to examine these figures with considerable care. In the first place, the great variation from year to year in the rate of increase is most noticeable; but it is no greater than that which is found to exist on the several rookeries, as an examination of the complete reports published by the Bureau of Fisheries will show.

To some persons the above figures may appear satisfactory. Every year since the cessation of pelagic sealing in 1911 a gain has been shown, whereas a loss was sustained from 1886 to that date. It was during this last period that uncontrolled slaughter of the females developed and threatened the very existence of the species before it could be checked through diplomatic channels.

Others will doubtless ask, "Why have the increases been so low?" A species of animal the female of which brings forth one young each year and approximately ten in a lifetime should increase annually more than 8.98 per cent. on the average. But that is all that an average of the above percentages will show.

Several facts have been learned the past few years which throw some light on this important subject. For instance, it has been found in several successive years that only one half of the females which are born live to be three years old. The loss of the class on the islands before the pups learn to swim is about one per cent. It varies from three fourths of one per cent. to one and one half, depending entirely upon how many bulls more than necessary are present on the rookeries. The annual loss of females through actual killing on the islands does not exceed 75, or less than five hundredths of one per cent.; all such deaths are purely accidental and largely unavoidable in the conduct of commercial work.

Therefore, the loss can take place in but one other place and that is in the sea. The figure of 50 per cent. loss the first three years was obtained in the following manner: The loss of breeding females, due to old age, is about 10 per cent. each year because the average breeding age is about 10 years. If this 10 per cent. be deducted from the number of breeding females in any year, say 1915, the remainder will represent the breeders of that year which remained alive in 1916. If this be taken from the total number of breeders in 1916, the last remainder will represent the increment of new three-year-old cows that year because the first young are born the third year. In several seasons this increment has been only about 50 per cent. of the number of female pups born three years previously. In other words, the loss amounts to one fourth the total number of births in any one year. Out of the females born during the last nine years, therefore, the following losses have been suffered: