SOME SUGGESTIONS FOR RESEARCH IN MATHEMATICS.

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In going to the boundary of knowledge of one of the oldest of all the sciences it is to be expected that one must travel far; and, since the materials with which the mathematician works are largely symbolic and hence highly subjective, it is also to be expected that mathematical research will be difficult. To a great degree this is true, but nevertheless, in a growing subject like mathematics new lines of investigation, new points of view and new methods of attack are being developed so that it is possible under proper guidance for the novice in research to make a start. And as he acquires mathematical strength and maturity his power and enthusiasm should increase with corresponding gains in research output. In this discussion some suggestions will be offered which it is hoped will be of help to anyone who is truly desirous of doing research in mathematics.

There are different kinds of mathematical research and these I have roughly classified into three types. The first is *fundamental* research, that research which characterizes the work of the genius and revolutionizes a point of view or a method of attack or develops an entirely new field of mathematical endeavor. This kind of research is not to be expected of the novice although the youthful Pascal, Gauss, Galois and Abel are brilliant examples to the contrary. The second type of research I call *contributory* and it is well represented by most of the papers appearing in the mathematical journals. The great majority of research workers are doing this type of research, and their work serves as the foundation on which the master worker builds so securely. It is this type of work which the novice may hope and expect to do. The third type of research is that which contents itself with *clarifying, simplifying and extending* what is already known. Good illustrations of this are the simplifying of the proofs of known theorems, the proving of known theorems by new methods, and the correlation of known facts. This last requires mathematical perspective and maturity but not necessarily great power. It might be questioned whether this third type constitutes real reasearch, but it nevertheless passes as such in most universities.

To be successful in research one must have the spirit of research, the enthusiasm to do research, a highly developed imagination and curiosity, and the determination and patience that are necessary for the accomplishment of anything worthwhile. No matter how brilliant a student may be in the classroom if he has not the above characteristics, either active or latent, he will not become a research worker. This point will become clearer when the following facts are known. During the decade 1910-1920 between 1,200 and 1,300 research papers appeared

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in American mathematical journals. These were the work of 325 different persons of whom nearly one-half contributed one and only one paper. It is to be presumed that many of those who contributed only one paper were publishing their doctorate theses. When it is further realized that the average number of doctorates in mathematics granted annually by American universities is between 25 and 30, it becomes evident that there is something vitally lacking in the product turned out by our graduate schools. My contention is that if a man shows sufficient ability to produce a real research paper he should be able to continue his investigations along the lines on which he has already made a start. I realize that many will be ready to offer objections to my point of view and probably some of them are valid. However, I do not recognize the fact that a man is thrown into environment which is not conducive to research as being entirely valid as an objection. A far greater objection in my opinion would be the lack of research facilities, which in mathematics amounts to a good working library and the opportunity for frequent conference and discussion with others interested in mathematical research.

This situation is not nearly as bad as it seems, for with the French and German mathematical encyclopedias, the German Jahrbuch and the Revue Semestrielle accessible, all of which tend to bring the information on the latest developments in the various fields up to date, one need not be far behind the times nor without knowledge of the more important discoveries in any field. Furthermore, such works as Pascal's *Repertorium*, Dickson's *History of the Theory of Numbers*, Bromwich's *Theory of Infinite Series*, and Weber's *Lehrbuch der Algebra*, will give the beginner a fair knowledge of their respective fields and offer many points of starting for new investigations. As a matter of fact the range of mathematics is so broad and the number of workers in nearly all the leading languages is so great that it is practically impossible for even the highly skilled research worker to keep in touch with the latest developments in even his own field, without having recourse to the various sources mentioned which give titles, abstracts and reviews of what is appearing in the numerous mathematical journals, books and other publications.

One of the big problems in mathematical research is to produce something that is new. To the novice this appears most formidable and often will discourage a most promising worker. Mathematical history is full of rediscoveries, and where many workers are investigating along the same lines it is to be expected that duplications of results will often occur. The most notable examples of this are probably the invention of the calculus by Newton and Leibniz and the discoveries of Bolyai and Lobachevski in non-euclidean geometry. But as long as a discovery has been made honestly the joy and enthusiasm that follow such a discovery should be sufficient to offset the disappointment at finding that it is already known, and with the experience gained and the consciousness of power and self-confidence acquired, one should work on with the expectation of making new discoveries.

At the present time some of the fields of mathematics in which much that is comparatively elementary remains to be done, are group theory,

the theory of statistics and certain phases of geometry. American mathematicians at present are doing considerable work in the theory of statistics and mathematical physics. Certainly the field of general science, chemistry, physics, biology, engineering, etc., and education, are rapidly taking up mathematics and they afford great opportunities for joint research work. I believe it was Lord Kelvin who is said to have remarked that if one could state a scientific fact in numbers one really knew something about it, otherwise the knowledge was not very great. Modern science is rapidly coming to realize that fact.

In my own field of endeavor the workers are relatively few, probably because group theory has not been developed sufficiently far to offer practical applications such as are found in statistical theory and mathematical physics. However, it has practical applications in geometry and crystallography. To cite an illustration of research that requires no great mathematical maturity and yet is sufficiently worthwhile, interesting and suggestive, as an introduction to research methods, I mention the application of the permutations found in the symmetric group of order six to the parameters of curves which involve only three parameters in their equations, such as the circle or the special form of the parabola $y=ax^2+bx+c$. It is well known that if the homogeneous co-ordinates of a point in a plane are subjected to these permutations, in general six distinct points will be determined all of which are vertices of a Pascal hexagon and hence determine a conic section. But I have been unable to find any further development or extension of this idea along certain lines, such as the division of the plane into well defined regions such that if one of these points occurs in one of these regions all of the six points are in that region, and the conic determined is always the same kind for a given region. The extension of the idea to higher spaces is obvious. Take as another illustration the circle $x^2+y^2+ax+by+c=0$, and permute a, b, c. In general six distinct circles will be determined whose centers are on a conic, as is to be expected. They will be equal in pairs and have the same straight line as their common chord. Other interesting geometrical relations will also be found. The parabola $y=ax^2+bx+c$ treated in a similar manner yields very interesting results. While all this is elementary and only a special case of a more general theory, still it affords a starting point for the beginner, and strange as it may seem, very little has been done along these lines with the group theory interpretations. And the results are probably as valuable as much that passes as research. While it is tactless and indiscreet, as well as unkind, to designate any new result as trivial yet it can not be denied that much that is published today as research will probably never be of more than passing theoretic interest, if even that, and yet, if it leads to something that is worthwhile or adds a little more to the sum total of human knowledge, it probably justifies itself.

In conclusion then my advice to the beginner would be that he make a start in whatever field that may be of special interest to him, and that he look for all the information possible in this field in the encyclopedias, treatises and any other literature accessible to him. He

should associate whenever and wherever possible with those who are working along kindred lines. And he should keep everlastingly at it. While much that may be done will prove worthless, and much that is discovered will apparently almost melt away, so far as pages are concerned, when prepared for publication, it must be remembered that the value of a result is frequently not recognized until years after its discovery. Furthermore, the discovery that a certain fact is already known should act as an incentive to further effort, for the joy in finding the truths of mathematics should be just as great, and one has gained power and confidence through the effort. Moreover, to become a master in any field requires one to make constant study and application of what has already been discovered, and the investigator with a working knowledge of a field is well on the road to successful research.