Two addresses from the Memorial Symposium held at Indiana University, June 1993

ON MAX ZORN'S CONTRIBUTIONS TO MATHEMATICS

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It is a pleasure and an honor to have the opportunity to spend a few minutes describing some of Max Zorn's contributions to mathematics. It is essential that any memorial service for Max include such a discussion; Mathematics was, after all, a major part of Max's life and certainly the dominant aspect of his intellectual life. Those in the audience who are not mathematicians will therefore I hope forgive me if the discussion becomes at times somewhat technical. The language of Mathematics was Max's language and it is in that language that we must in part speak of him.

Max's interests were remarkably diverse. He published papers in algebra, number theory, logic, functional analysis and function theory. It is therefore impossible for me to speak competently on all of his contributions and so I have chosen instead to speak of his work in algebra and number theory. This is appropriate not only because of my own mathematical interests but also because it is in these areas that Max did his first important work.

I should also say at this point that I will not speak on his work on what came to be called Zorn's lemma. In fact one of my goals is to demonstrate that Zorn's lemma, although certainly Max's most famous mathematical contribution, was far from his only one.

However in this regard I cannot refrain from describing an incident involving the student newspaper, the Indiana Daily Student. In their article on Max, they referred to his result as "Zorn's Dilemma", as though it were something like

Russell's paradox. As someone in the department pointed out to me, no one would have enjoyed the error as much as Max himself.

Max obtained his degree in 1930, two months before his 24th birthday. His advisor was Emil Artin; in fact he was Artin's second Ph.D. student. His thesis was in abstract algebra and treated the theory of alternative algebras [*Theorie der Alternativen Ringe*, Abh. Math. Seminar Hamburg 8, 123-147 (1930)]. He went on to write four papers on this subject, the last in 1941. I want to try to put his results in perspective.

In 1843 William Hamilton introduced the algebra of quaternions, the first extension of the notion of number system past the system of complex numbers. Just as the complex numbers may be constructed by introducing operations on ordered pairs of real numbers, the quaternion numbers may be constructed by introducing similar rules for adding and multiplying ordered pairs of complex numbers (the duplication process). The resulting system is four dimensional over the reals and has many properties in common with its real or complex predecessors. In particular it is an example of what is now called a division algebra; you can divide any nonzero quaternion into any other quaternion, just as you can in the reals or the complexes. However unlike the passage from the reals to complexes, you lose something in going from complexes to quaternions: The algebra of quaternions is not commutative (there are quaternions $\mathbf{x}$, $\mathbf{y}$ such that $\mathbf{xy}$ does not equal $\mathbf{yx}$).

Almost immediately after Hamilton's discovery, Graves and Cayley (independently) discovered that there was yet a bigger number system, this time of dimension eight over the reals, in which once again one could always divide. Just as in going from the reals to the complexes or from the complexes to the quaternions, this new system can be constructed by defining certain addition and multiplication rules to ordered pairs of quaternions, that is by another application of the duplication process. What results is now usually referred to as the system of Cayley numbers (or octonions). However once again we have lost something in the process. The algebra of Cayley numbers is not only not commutative, it is not even associative:

There are octonians **x,y,z** such that **(xy)z** does not equal **x(yz)**. However the Cayley numbers do have a property that is close to, but weaker than, associativity: The expression **(xy)z-x(yz)** is an alternating function of its three arguments, i.e. the expression is zero when any two of the three arguments are the same. Thus any algebra satisfying this weaker form of associativity came to be known as an alternative algebra and it was this class of algebras that was the object of Max's study in the four papers.

To make this definition perhaps palatable to those more comfortable with associative objects let me say that another characterization of alternative algebras is this: an algebra is alternative if and only if the subalgebra generated by any two elements is associative.

Now between the 1840's and the 1930's much had happened. In particular the subject of abstract algebra had been born. Among its achievements had been so called "structure" theorems for classes of algebras, for example for the class of Lie algebras (work of Killing and Cartan) and the class of associative algebras (work of Wedderburn). Even though these two classes have very different looking properties, the pattern of the structure theory is remarkably similar. In each case one identifies an unpleasant subset of the algebra, usually referred to as the "radical". It is then shown that if that subset is as small as possible (that is equals zero) then the algebra (which in this case is called semisimple) can be decomposed into a finite sum of more understandable objects (called the simple objects of the class). Finally these simple objects are classified by showing that each one is the same as one that appears on a list of algebras in the class that you already know.

This, then, is what Max did for the class of alternative algebras: He determined what the unpleasant subset for alternative algebras is, he proved that if the alternative algebra is semisimple then the algebra decomposes into a finite direct sum of "simple" alternative algebras, and he then classified the simple alternative algebras. It should be noted that his results are for algebras over arbitrary fields with no restriction on the characteristic, that is definitely in the abstract algebra spirit.

This was a striking achievement and in fact Max received a University prize from Hamburg for his work. In the process or working out the structure theory he proved in particular that the only finite dimensional alternative division algebras over the reals are precisely the four algebras we have discussed, the reals, the complexes, the quaternions, and the octonions. This result is of sufficient importance to deserves its own paragraph.

After the discovery of the quaternions and the Cayley numbers it was thought by some that there must be a never-ending series of bigger and bigger number systems. In particular Graves is said to have conjectured the existence of such a system of dimension 2n for each n. However a series of results starting later in the nineteenth century and continuing into the second half of the twentieth century has shown that this is far from the case. In very brief form here is the history:

In 1877 Frobenius proved that the only real associative finite dimensional division algebras are the reals, the complexes and the quaternions.

In 1933 Zorn's theorem appeared [*Alternativkorper und Quadratische Systeme*, Abhandlung Math. Sem. Hamburg, 9, pp 395-402 (1933)].

In 1940 Heinz Hopf proved, with no associativity conditions assumptions of any kind —and this was a stunner— that every finite dimensional real division algebra has dimension of 2n.

In 1958 Milnor and Kervair (independently) proved, again with no associativity conditions, the famous result that every finite dimensional real division algebra has dimension 1,2,4, or 8.

So Max's theorem was a significant contribution to that line of results. He had good reason not to be able to figure out the theorems of Hopf and of Milnor and Kevair, because both of those results require methods of algebraic topology which were only then being developed.

I have described Max's research on alternative algebras. But I want to make two observations on his other work.

In his second published paper entitled *A Note on Analytic Hypercomplex Number Theory* [Abhandlung Math. Sem. Hamburg, 9, pp197-201 (1933)], Max proved that the fundamental theorems on the structure and classification of simple algebras over algebraic number fields could be derived from the functional equation of the zeta function of a division algebra. This became one of the standard approaches to the subject and, in fact, is the one used in Andre Weil's famous text *Basic Number Theory.*

Lastly here is a nice example of Max's mathematical insight. In 1937 a paper of his entitled *p-adic Analysis And Elementary Number Theory* appeared in the Annals of Mathematics [Vol. 38, pp. 451-464]. In it he provides a new proof of a generalization, first given by Schur, of Fermat's so-called little theorem. The proof uses the method of p adic analysis and Max makes the important remark that his aim is to demonstrate the desirability of a deeper investigation of special p adic functions and in particular of the possibility of analytic continuation of such functions. Well, suffice it to say that the analysis of special p adic functions has now become an important tool in the arsenal of modern number theory.

These things having been said, for most of us in this room Max's published work, as significant and substantial as it is, is not what we will remember him by. It is rather Max's life-long dedication to mathematics and his apparently endless curiosity about mathematical ideas that we remember and from which we draw inspiration. For me personally this dedication is symbolized by the image of Max with his cane walking purposefully along Third Street to his office, day after day. It is Max's spirit and very presence that we will all miss.

ZORN'S LEMMA

John Ewing

The 19th century was a troubled time for mathematicians. A subject that once seemed above the common squabbling of scientists and philosophers began to develop flaw s that even beginning students of calculus could see. Fierce arguments broke out about how to repair the damage. Slowly, those problems were corrected by carefully defining terms (continuity, convergence, integration), but mathematicians soon discovered that they must define evermore basic terms to do the job. The process had to end somewhere, and it was clear that it ended in a system of axioms --- simple statements that everyone would accept as true, and from which all the rest of mathematics was deduced using logic.

But what should those axioms be? Near the turn of the century, Zermelo and Fraenkel formulated a system of axioms for the most basic part of mathematics, set theory . But Zermelo soon ran into a problem: He needed to prove something about sets, and it didn't seem to follow from the axioms. Here's where the story of Zorn's Lemma begins.

Cantor (a major figure in the development of set theory) gave a prominent place to ordered sets. An ordering is a relation --- ordered pairs of elements, one above the other ---that provides a way to compare elements (like rank in the army or standing in the polls). It is a full ordering if it satisfies some common sense properties of a ranking (an element cannot be both above and below another element, and if **x** is above **y**, while **y** is above **z**, then **x** is above **z**. A set is well-ordered if it has a full ordering for which every (nonempty) subset has a smallest element. Example --- the counting numbers {1,2,3,4,5, ...}. A not-so-obvious example is the set of integers {..., -2,-1,0,1,2, ...} or the rationals. The "natural" ordering doesn't give a well-ordering (since there are subsets without a smallest element), but we can put an "unnatural" ordering on the set to make it well-ordered.

Why bother? Because Cantor discovered that for sets that are well-ordered, one can argue by induction --- exactly as one argues for the counting numbers. To prove a statement is true for every integer, we need only show it's true for the first and then show that when it's true for the first n, it must be true for n+1. That's induction. Using well-ordered sets allowed Cantor to do "transfinite" induction, which was a powerful new technique, especially in algebra.

Could every set be well-ordered? That was a key question for mathematicians trying to create firm foundations. Of course, what they really needed to know was whether the statement (or its negative) followed from the axioms of Zermelo and Fraenkel. In 1904, Zermelo "proved" his Well-ordering Theorem: Every set could be well-ordered. Unfortunately, he had to assume another axiom to do so, the Axiom of Choice. At first, the Axiom of Choice seems completely natural; it says that for any collection of set s we can form a new set by choosing exactly one element from each of the others. Faced with a basket of apples, a basket of oranges, a basket of grapes, etc. we can make a basket of fruit by choosing one apple and one orange and one grape and ... But there is a subtlety that one easily misses: We have to make the choices simultaneously, not one at a time. For a finite collection of finite sets, this can easily be done. For an infinite collection of infinite sets, it's not so obvious.

The Well-ordering Theorem therefore rested on shaky ground, following from the axioms of set theory and an uncertain Axiom of Choice. Nonetheless, transfinite induction and well-ordering were so useful that mathematicians used them freely during the early part of the twentieth century, building whole areas on top of what might be a shaky foundation. Algebra was particularly dependent on transfinite induction, and an entire area (field theory) was developed using the Well-ordering Theorem as its basis.

In 1934, Max Zorn presented a paper to the American Mathematical Society that tried to study those shaky foundations. It subsequently appeared as a research announcement in the Bulletin of the AMS (1935, 667-670). In that short paper, he showed that many of the theorems of field theory followed from the usual axioms of set theory together with a new axiom, which he called his maximum principle. The new axiom had to do with collections of sets. A chain in such a collection is a subcollection of nested sets, that is, in which one of any pair is contained in the other. A collection is closed if it contains the union of every chain (the set of all elements in every one of the sets in the chain). Here's Zorn's maximal principle: Whenever a collection of sets is closed, there must be a maximal set in the collection, that is, a set that is not contained as a proper subset of any other . Using that principle, rather than the Well-ordering theorem of Zermelo, he sketched the proofs of many basic theorems about transcendence and algebraic closure in field theory.

It was that maximal principle that came to be known (in slightly different form) as Zorn's Lemma. It was, of course, not a lemma at all in the sense that it was not proved but rather assumed. It was a replacement for Zermelo's Well-ordering Theorem, and made proving many of the results in algebra easier and less cumbersome.

Did it solve the problems of foundations? Not at all. In fact, Max Zorn him self mentions at the end of his 1935 paper that his maximal principle and the Well-ordering Theorem and the Axiom of Choice are all equivalent ; that is, one can prove any one of these three from any one of the others. He promises to provide a proof of this in another paper. The new axiom made it easier to prove theorems, but it did not resolve the question about foundations.

Was it a new idea? Not entirely. The early part of the twentieth century was a tumultuous time for mathematics. Many people studied foundations, and Kuratowski and Brouwer both considered maximal principles in other settings. But Zorn showed how to use a maximal principle to prove interesting and important mathematics. It was Zorn's work that people read and studied. And it was the name "Zorn's Lemma" that was attached to a particular maximal principle: Every partially ordered set for which every chain has an upper bound has a maximal element. This was such a useful formulation of the axiom that mathematicians often sought to convert problems into a form that made it easy to apply; one tried to "zornify" the problem. Using the Well-ordering Theorem in the form of Zorn's Lemma became a useful tool, used almost universally in twentieth century mathematics.

Was the assumption justified? That is one of the delightful surprises (and mysteries) of mathematics. On the one hand, Goedel showed that it was: Assuming the Zorn's Lemma along with the rest of the axioms of set theory would lead to no contradictions. On the other hand, Cohen showed that Zorn's Lemma was independent from those axioms: Assuming the negative was consistent as well. Mathematicians are therefore left with a free choice, to assume Zorn's Lemma or not. Most do, and that makes Zorn's Lemma indispensable.