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THE DISCOVERY OF FORCING

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1. Introduction. I would like to begin by thanking the organizers of this conference for inviting me, and especially my friend Adolf Mader, whom I first met on a semester visit to Hawaii nine years ago. My knowledge of abelian groups is very limited, but since it has developed that forcing has played a role in the subject, there is most likely some interest in what I shall relate. I do remember quite well my first contact with abelian groups. This was through the monograph of Irving Kaplansky on infinite abelian groups which appeared while I was a graduate student at the University of Chicago, in the mid fifties. I recall reading the book rather cursorily, and even being surprised by the role that ordinal numbers played in Ulm's theorem. Kaplansky was an enormously lively and forceful influence in Chicago at that time, and he certainly represented algebra very ably to us students. In my first year or so, I studied many subjects avidly including algebra, mostly ring theory and algebraic number theory. I have now learned that Reinhold Baer was one of the pioneers of the abelian group theory, and I can relate that I recall seeing him when he would come up from Urbana seminars. Algebra was in the air at Chicago, perhaps more so than analysis, and before eventually deciding to return to my earlier interest in analysis by choosing Antoni Zygmund as my advisor, I was much taken by the beauty of algebra. The University of Chicago of that period has often been described as having its Golden Age, which fortunately for me coincided with my stay there, and an absolutely essential component of the excitement of my student days was generated by the enthusiasm of Kaplansky and of his many seminars and the resulting notes and monographs that arose from them. I should also mention, just to make a point of contact with Peter Hilton's beautiful talk on the birth of homological algebra, that there was also a good deal of ferment around the courses of Saunders MacLane, in topology, the $K(\pi, n)$ spaces, and the visit of Henri Cartan when he lectured on the calculation of the homological structure of these spaces. So I hope I have established some small credentials for

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participating in a conference on algebra.

During this conference, I have certainly been impressed by the vitality of abelian groups and also by what I have not often observed in such meetings, a strong spirit of collegiality and friendship among the participants, no doubt enhanced by the natural beauty of Hawaii, and by the hard work of the organizers.

I understand my task to be to indicate the background leading up to my work in aet theory, and to explain in broad outline the general method of forcing, which I introduced in order to establish various independence results. The evolution of forcing methods has been so rapid and extensive that I am no longer competent to give any broad survey. It is, of course, gratifying to learn that it has left its mark even on abelian groups, in particular on the Whitehead conjecture. Paul Eklof will speak in detail how set theory has impinged on this field.

Set theory is a subject which inspires two conflicting emotions in most mathematicians. On the one hand, everyone is familiar with the basic concepts, so that no technical preparation is necessary. Further, every one has his own personal view about the nature of sets, to what degree they feel comfortable with constructive or non-constructive methods, etc. They may feel that the "official" exposition of set theory, i.e., all of mathematics, using formal systems and particular axiom systems, has little relevance to their work as research mathematicians. On the other hand, the existence of a whole series of surprising results has to some extent shattered the complacency of many mathematicians, and there is an unjustified aura of mystery and awe that tends to surround the subject. In particular, the existence of *many* possible models of mathematics is difficult to accept upon first encounter, so that a possible reaction may very well be that somehow axiomatic set theory does not correspond to an intuitive picture of the mathematical universe, and that these results are not really part of normal mathematics. In these lectures I will try to clear up some of these confusions and convince you that indeed these results are easily accessible, even to a nonspecialist. I can assure that, in my own work, one of the most difficult parts of proving independence results was to overcome the psychological fear of thinking about the existence of various models of set theory as being natural objects in mathematics about which one could use natural mathematical intuition.

If one reviews the development of logic and set theory, there are some rather clear demarcations. There is first a period of purely philosophical thinking about Logic, which I can say belongs to the area of pre-mathematical thinking, and ends somewhere in the middle nineteenth century, with the realization by Boole, and others that there is a mathematics of truth and falsity, which today we call Boolean algebra. The next important step, shared by several people, but which we can, without straying too far, ascribe to Gottlieb Frege who realized that mathematical thinking involves variables, predicates or relations, and quantifiers, i.e., there exists and for all, which range over the variables. In his Begriffsschrift he stated how these symbols are manipulated, and by examples presumably convinced himself and others that this notation was all that was needed to express all mathematical thinking. Today we would say that he gave precise rules for the predicate calculus.

In a parallel development, Georg Cantor was developing the theory of sets, in particular his theory of cardinal numbers and perhaps even more significantly his theory of ordinal numbers. Although aware that his new creation was of a radical different nature than previous mathematics, since he asked questions about sets much larger than had ever occurred naturally before in mathematics, he probably regarded his theorems as correct theorems exactly in the same spirit as other results. Thus, I think it was correct to say that Cantor was certainly not a logician. The cornerstone of his theory was the notion of cardinals, and by using his well-ordering principle, he showed that all the cardinals were arranged in increasing size, for which he used the first letter of the Hebrew alphabet, $\aleph$, the whole sequence comprising the mathematical "universe" for which he used the last letter tav. As we all know, his joy in the discovery of this universe was marred by his inability to answer the first question which naturally suggested itself, that is, where did the continuum, C, fit in the sequence of alephs. His continuum hypothesis was that it was $\aleph_1$, the first uncountable cardinal.

Today we know that the continuum hypothesis, CH, is undecidable from the usual axioms of mathematics, the so-called Zermelo-Frankel system. To make this statement precise, one needs two ingredients. One, the formalization of mathematics in the strict sense achieved by Frege, and secondly, the statement of what are the axioms of set theory. In 1908, Zermelo published in a somewhat sketchy form, the commonly used axioms. However, he did not speak of a formal system, in the sense of Frege, but in an informal way described what he observed were the basic axioms which it seemed to him all mathematicians used in their normal research. Actually, even by the standards of an informal presentation, his paper contained a grave defect which, amazingly, did not attract sufficient attention. Namely, he used the word "property", and his key separation axiom stated that every property, for each set A, determined a subset of A having that property. Of course, property and sets are both undefined terms, so that there is a sense of vagueness here. Weyl sought to correct this, as did Frankel, and even more cogently Skolem. They said that Zermelo's one axiom of separation was actually an infinite scheme of axioms, and property meant every formula that can be written using the symbols of the predicate calculus, and the one undefined relation, $\in$, or membership. Already the first surprise had surfaced, namely, that the axioms of set theory are actually infinite in number, although they are generated by a simple recursive procedure.

With some modifications, due to Frankel and Skolem, these have remained the commonly accepted axioms for set theory, and hence all of mathematics.

When one reads these papers, particularly those of Skolem, one is struck by the fact that they do not differ in their general appearance and tone from papers in other fields. So for those who might wish to penetrate more deeply into the subject, let me give this encouragement: The attempts to formalize mathematics and make precise what the axioms are, were never thought of as attempts to "explain" the rules of logic, but rather to write down those rules and axioms which appeared to correspond to what the contemporary mathematicians were using. An unnatural tendency to investigate, for the most part, trivial minutiae of the formalism has unfortunately given the subject a reputation for abstruseness that it does not deserve.

In 1915, a landmark paper of Löwenheim appeared in which he presented a theorem of mathematical interest, not at all obvious, concerning the notion of formal system and the predicate calculus taken in the form given it by Frege or any equivalent version which one may prefer. This paper was rather difficult to decipher, and it was Skolem who simplified the presentation and extended the result. The so-called Löwenheim-Skolem theorem refers to formal languages and to *models* of these formal languages in which certain statements, axioms if you will, hold. I do not think that the importance of the notion of a model of a formal system had been emphasized, so that at the risk of slighting someone, I would be inclined to say that Frege must share the honor of founding our subject with Löwenheim, who might be called the father of model theory. There are several ways of looking at the Skolen-Löwenheim (S-L) theorem. The most often quoted version is that any model of a finite language contains a submodel which is countable and which has precisely the same true sentences. What was shocking, as Skolem pointed out, was that this theorem when applied to any axioms for set theory implies that the universe of all sets contains a countable subset, M, such that if one restricts one's attention to M and disregards all other sets, the axioms of set theory hold. This appeared to be paradoxical, since we know that uncountable sets exist. The paradox vanishes when we realize that, to say that a set is uncountable, is to say that there is no enumeration of the set. So the set in M which plays the role of an uncountable set in M, although countable, is uncountable when considered in M since M lacks any enumeration of that set. The other interpretation of S-L theorem is a bit controversial. To quote Skolem ([**4**] or [**3**]), " ... In volume 76 of Mathematische Annalen, Löwenheim proved an interesting and very remarkable theorem on what are called "first-order expressions". The theorem states that every first-order expression is either "contradictory or already satisfiable in a denumerably infinite domain".

The controversial aspect occurs because, if read in the most direct fashion, this is exactly the statement of the Gödel completeness theorem published ten years later. Now Kurt Gödel is one of my heroes, and I do not feel the necessity of defending him from the fact that his work was anticipated by Skolem. The question was put to Gödel, whether he was aware of Skolem's work. If various personal accounts are to be believed, as recounted in Gödel's collected works, it appears that he was not sufficiently aware of Skolem's paper which was published in a Norwegian journal in the German language. As you may have gathered, Skolem is also one of my heroes, and I will later mention some remarks of his which were amazingly prescient in foreseeing the possibility of obtaining results of the type which I eventually did obtain.

Skolem published a later paper in which, influenced by the fact that S-L precludes the possibility of axiom systems having only *one* model, so-called categoricity, he expressed a certain degree of pessimism. In view of the present day discussion of whether CH is actually true or false, I think it is interesting to reflect on these thoughts written in 1922, presented to the Scandinavian Mathematical Congress ([**4**] or [**2**]):

> The most important result above is that set-theoretic notions are relative. I had already communicated it orally to Bernstein in Göttingen in the winter of 1915–16. There are two reasons why I have not published anything about it until now: first, I have in the meantime been occupied with other problems; second, I believed that it was so clear that axiomatization in terms of sets was not a satisfactory ultimate foundation of mathematics that mathematicians would, for the most part, not be very much concerned with it. But, in recent times, I have seen to my surprise that so many mathematicians think that these axioms of set theory provide the idea foundation for mathematics; therefore, it seemed to me that the time has come to publish a critique.

Since the Skolem-Löwenheim "paradox", namely, that a countable model of set theory exists which is representative of the stumbling blocks that a nonspecialist encounters, I would like to briefly indicate how it is proved. What we are looking for is a countable set M of sets, such that if we ignore all other sets in the universe, a statement in M is true precisely if the same statement is true in the true universe of all sets. After some preliminary manipulation, it is possible to show that all statements can be regarded as starting with a sequence of quantifiers, for all, there exists, etc. The set of all statements can be enumerated, say A_n. We go through the list and every statement which begins with "there exists" *and* is true in the universe, we pick out one set in the universe which makes it true. Since there are only countably many statements, we have chosen only countably many elements and we place them in M. Next we form all statements using these sets and again only look at those which begin with "there exists". If they are true in the universe, we pick out one set which makes them true and adjoin these to M. We repeat this process countably many times. The resulting collection of all sets so chosen is clearly countable. Now it is easy to see that the true statements of M are exactly the true statements in the universe. This is proved by induction on the number of quantifiers appearing at the beginning of the statement. If there are none, then the statement simply is composed of finitely many statements of the form

"x is a member of y", connected by the Boolean operators. This clearly is true in M if and only if it is true in the universe. Now consider a statement with one quantifier. By considering its negation, if necessary, we may assume it begins with "there exists". Now our choosing process clearly guarantees that the statement is true in M if and only if it is true in the universe. The proof now proceeds by a simple induction on the number of quantifiers.

You may feel that this argument is too simple to be correct, but I assure you that this is the entire argument, needing only a very simple argument to show that one can always bring the quantifiers to the front of the statement. I might add that the underlying reason the argument is so simple is because it applies to any system whatsoever, as long as we have only finitely many predicates (even countably many will work the same way) so that the number of statements that can be formed is countable. This theorem is perhaps a typical example of how a fundamental result which has such wide application must of necessity be simple.

For the sake of completeness, I will enumerate all the symbols that are used in set theory.

1. propositional connectives, 'and', 'or', 'not', 'implies'
2. parentheses, left and right, and the equal sign
3. the symbol $\in$ for membership
4. variable symbols, which we can take as the letter x followed by a subscript which is a binary numeral, hence using the symbols 0 and 1
5. the quantifiers, 'for all', 'there exists'

Having said this, it follows that all mathematics can be reduced to a machine language using the above symbols, and with precise rules, so that a machine can verify proofs. To complete the picture we must state the axioms. As Zermelo gave them there were seven, but here I shall only mention the two most important. One is the separation axiom which says that if $P(x)$ is a formula, perhaps involving fixed sets, for every set A, there is a subset B of A consisting of all the elements x of A which satisfy $P(x)$. This is an infinite set of axioms since we must enumerate all formulas $P(x)$. The second axiom we mention is the power set axiom which says that for all A, there is a set B consisting of all subsets of A. Thus I expect you to believe that we have completely precise rules for manipulating the formulas of set theory. This is all we shall have to say about the formalization of set theory.

However, in all honesty, I must say that one must essentially forget that all proofs are eventually transcribed in this formal language. In order to think productively, one must use all the intuitive and informal methods of reasoning at one's disposal. At the very end one must check that no errors have been committed; but in practice set theory is treated as any other branch of mathematics. The reason we can do this is that we will never speak about proofs but only about models. So, therefore, let us return to what is sometimes called "naive set theory" and speak about the development by Cantor of his two principal discoveries, the notions of cardinal and ordinal. Cantor defined two sets as having the same cardinality if there was a one-to-one correspondence between them. He proved by means of the famous diagonal method that the power set of any set has a greater (in an obvious sense) cardinality than the original set. The next logical step is to show that the infinite cardinalities can be arranged in an order. Here the Cantor-Bernstein theorem, proved originally by Dedekind, asserts that if A and B are cardinalities such that A is less than or equal to B, and B less than or equal to A, then A and B are equal. What remains is to show that given two sets A and B, one is less than or equal to the other.

If one thinks of this problem for two "arbitrary" sets, one sees the hopelessness of trying to actually define a map from one into the other. I believe that almost anyone would have a feeling of unease about this problem; namely that, since nothing is given about the sets, it is impossible to begin to define a specific mapping. This intuition is, of course, what lies behind the fact that it is unprovable in the usual Zermelo-Frankel set theory. Cantor suggested a method of proving it. It depended on the notion of a well-ordering, i.e., an ordering of a set A in which every nonempty subset has a least element. If A and B have well-orderings, it is not hard to show that either there is a unique order preserving map of A onto an initial segment of B, or vice versa. So if A and B are well-ordered, we can define a unique map which shows that the cardinality of A is less than or equal to that of B, or conversely. By an ordinal we simply mean an equivalence class of well-orderings. It follows that the ordinals are themselves well-ordered. Now if one assumes the well-ordering principle, that all sets have a well-ordering, it follows that all the cardinal numbers have at least one ordinal of that cardinality. Thus, we have in the "sequence" of all ordinals, particular ordinals which we now call cardinals, which are defined as ordinals whose cardinality is greater than that of any of its predecessors. These, being ordinals themselves, are easily seen to be a well-ordered set, each with different cardinality. These are the $\aleph$'s. In particular, $\aleph_1$ is the first uncountable ordinal. It can be viewed as the set of all countable ordinals. The continuum hypothesis says that $\aleph_1$ is the cardinality of the power set of $\aleph_0$, i.e., the set of all real numbers, the continuum. The generalized continuum hypothesis says that, for any cardinal A, the power set of A, or 2^A, has the cardinality of the first cardinal after A.

The story of how Cantor struggled with the continuum hypothesis, and how it may have contributed to his mental disturbance is well known. Clearly, Hilbert, who was keenly interested in foundational questions, attached great importance to them. He himself also attempted a proof, and it would seem believed he had the essential outline of a proof. However, the world was not persuaded, and we now know such a proof was impossible. Hilbert spoke of Cantor's set theory as one of the most beautiful creations of the human intellect, and the Continuum Hypothesis as one of the most fundamental questions in mathematics. However, it should be added that it has little contact with the vast body of mathematics that came before it, and was largely ignored by most mathematicians. Of course, the well-ordering principle was also a fundamental question, although it seems that Cantor may have been more willing to merely accept it. Hilbert mentioned these questions as his first problem in his famous address of 1900. Two important correct results did appear in the early 1900's. The first was Zermelo's proof of the well-ordering principle from the axiom of choice. The second was a paper by König, who showed that the continuum C could not be the cardinal $\aleph_\varpi$, the first cardinal greater than all the $\aleph_n$. Indeed, C cannot be the sum of countably many smaller cardinals. Both these results are relatively simple, yet they represent important contributions. The axiom of choice has found wider acceptance than the well-ordering principle, and many present-day textbooks hardly mention the well-ordering principle. König's result is now known to be best possible, since the independence results show that any $\aleph$ which is not a countable sum of smaller $\aleph$'s can be made to be C, in some suitable model.

Before we begin what might be considered the second half of this lecture, the independence results of Gödel and myself, I would like to say a few words about methodology. The early years of the twentieth century were marked by a good deal of polemics among prominent mathematicians about the foundations of mathematics. These were greatly concerned with methods of proof, and particular formalizations of mathematics. It seemed that various people thought that this was a matter of great interest, to show how various branches of conventional mathematics could be reduced to particular formal systems, or to investigate the limitations of certain methods of reasoning. All this was to illustrate, or convince one of, the correctness of a particular philosophical viewpoint. Thus, Russell and Whitehead, probably influenced by what appeared to be the very real threat of contradictions, developed painstakingly in their very long work, *Principa Mathematica*, a theory of "types" and then did much of basic mathematics in their particular formal system. The result is of course totally unreadable, and in my opinion, of very little interest. Similarly, I think most mathematicians, as distinct from philosophers, will not find much interest in the various polemical publications of even prominent mathematicians. My personal opinion is that this is a kind of "religious debate". One can state one's belief but, with rare exceptions, there are few cases of conversion.

2. The work of CH and AC. The statements of the main results are that certain propositions, CH, AC, etc., are independent of the axioms of Zermelo-Frankel set theory. In this form they refer to proofs, i.e., strings of sentences derived from the axioms using the rules of predicate calculus. However, this is a bit misleading. In practice, the only way to do this is to exhibit a model in which the axioms hold, and in addition, certain other statements, depending on what one is interested in. This is certainly the case for Euclidean and non-Euclidean geometry. In my own work on CH, I never was able to successfully analyze proofs as a combinatorial "game" played with symbols on paper.

Therefore, I begin with a few words about models of set theory. Clearly a model for ZF, i.e., a set with a certain $\in$ relation, cannot be shown in ZF, for this implies the consistency of ZF and this in turn violates the incompleteness theorem. Conversely, consistency of

ZF implies the existence of a model by virtue of the completeness theorem. However, again I would claim that this is not really natural or satisfying. Our models should consist of actual sets, not some combinatorial scheme. Therefore, one speaks about *standard models*. These are models where the objects are sets and the membership relation is the usual one. That is, they are *submodels* of the universe.

The axiom of standard models, i.e., that there is a standard model, is slightly stronger than the consistency of the system. Nevertheless, I feel that one must work with standard models if one is to have any kind of reasonable intuitive understanding.

Now there is another point of view, slightly different, which avoids explicit mention of standard models but in essence achieves the same goal. That is, one might find a particular property $P(x)$ such that when one restricts to x satisfying P, all the axioms hold. One avoids speaking of the *set* of all x satisfying P, since the axioms do not allow such a construction involving a quantifying over all x, but asserts instead the following: If one looks at any axiom and adds the condition that all variables are assumed to satisfy P, one can prove the new *relativized* axiom. So one is now speaking about proofs in contrast to models, but the effect is the same as showing that the (fictitious) set of all x satisfying P is a model. Such a method is called the method of *inner models*. Indeed, this is the method of non-Euclidean geometry where one looks at all objects, say great circles on the sphere, satisfying a certain property. One might even think at first encounter that this is the only way to proceed. However, I shall later point out that such a method is impossible for the independence of the continuum hypothesis.

In 1937, Gödel showed that AC and CH were consistent with ZF. He did this by constructing a model of sets which he called *constructible sets*. Before sketching his work, I would like to say a few words about the background of his work. To my knowledge, the first interesting theorem about models of set theory is due to von Neumann in 1929 (his work was to some extent anticipated by Skolem and Zermelo). He was concerned with what may appear as a somewhat pathological aspect of the axioms. The usual axioms of Z do not exclude the possibility of a set x, such that x is a member of x. Now one may say that such monstrosities even if they do exist clearly play absolutely no role in the development of mathematics. For, one usually starts with the integers then considers sets of integers, or reals, then sets of these, etc. So this kind of circularity never occurs. Nevertheless, one might wish to rigorously exclude this possibility. Therefore, von Neumann introduced a new axiom, the axiom of regularity, which says roughly that all sets must eventually be based on the "primitive" elements which in the usual development is the empty set, but which in other formulations might be taken to be the integers. One version might say that there is no infinite sequence x_n where each x_{n+1} is a member of x_n. A better form, the one that is usually taken, is that given any set (of sets) there is one set which contains no members in that set. That is, it is minimal with respect to membership.

Now, von Neumann showed that even if these so-called "monstrosities" exist, it is possible to ignore them. He did this in the following way:

A set x is said to be of rank α, if α is the least upper bound of the rank of all its members. (One should really say, of rank α and *well founded*, if all its members are *well founded* and etc.). He then proved that the *well founded* sets form a model of set theory, and the axiom of regularity holds for them. Thus the inner model of the well-founded sets establishes the consistency of the axiom of regularity. The proof is not at all difficult. However, one draws two conclusions. First that the ordinals play a fundamental role in these axiomatic questions just as they did for Cantor when he tried to define the sequence of $\aleph$'s. The ordinals remain a kind of mystery in that we do not know how "far" they extend, but we must allow all of them if we are to assign ranks to sets. The second conclusion is one which I do not know if it was actually drawn by von Neumann or Gödel. It is a kind of pseudo-history in which I reconstruct what would seem to be the plausible route in discovering new concepts or proofs. This conclusion is that in set theory when dealing with fundamental questions, one often has a kind of philosophical basis or conviction, rooted in intuition, which will suggest the technical development of theorems. In this case the intuition is that one must only allow sets which are built up or "constructed" from previous sets. This general point of view is associated with the "predicative" philosophy which was the object of much debate. The essential idea is that even if one adopts the naive view that all properties define sets, it is important that in defining "new" sets, the sets that the property speaks about have already been defined, or constructed. For example, suppose one defines a real number by a certain property of integers, but in this property one speaks about all real numbers. Thus, in order to verify that an integer n belongs in the set, one must ask a question about this set itself. This could cause concern. If one takes the view that the real numbers exist, then asking questions about them in order to pick out a particular real number causes no difficulty. If one feels that real numbers should be "constructed", then in trying to define a real number one should not ask a question about reals which may include the very real that one is trying to construct.

The method of Gödel is to restrict attention to those sets which can indeed by defined predicatively. To quote Gödel [**1**], his method of construction as we shall soon give, "is to be understood in the semi-intuitionistic sense which excludes impredicative procedures. This means 'constructible' sets are defined to be those sets which can be obtained by Russell's ramified hierarchy of types, if extended to include transfinite orders." My personal reaction to the above is one of a little surprise since he does not refer to von Neumann's theory of rank which does this very construction using arbitrary ordinals. But Gödel often expressed his ideas in rather convoluted ways and was concerned with philosophical nuances, which I in all honesty have never found interesting. Since von Neumann and Gödel were rather close mathematically, and von Neumann with his famous quickness understood Gödel's work very early and easily, I conclude in my pseudo-history that von Neumann's construction of well-founded sets had a very strong influence on Gödel.

For a set A, consider any formula with one free variable $P(x)$, using as constants particular elements of A, and where it is understood that all the bound variables of P range only over A. Then we can form the set of all elements of A satisfying P. These sets may be said to be "predicatively defined from A". The collection of all such sets we adjoin to A form a set that we denote by A'. This is the basic idea in Gödel's construction. If the reader wishes to penetrate more deeply, he should pause at this point to ponder the significance of this simple, yet absolutely fundamental definition. I do not know whether this idea of "construction" appeared explicitly before Gödel. If I may engage in some more pseudo-history, I would say that if it did not appear previously, I would find it somewhat surprising in view of the extent of the debates about what constituted constructive methods. It certainly is very much in the spirit of the predicative point of view espoused by writers such as Poincaré and Weyl.

Now we come to the second component of Gödel's construction, which is to extend the procedure to transfinite orders. The definition is very simple using transfinite induction. We define M_0 as being the set whose only member is the empty set, and for each α, $M_\alpha = A'$, where A is the union of all M_β, and β ranges over all ordinals less than α. Finally a set is called constructible if it lies in some M_α. The constructible "universe" is often denoted by L, so x in L means simply x is constructible. Thus, to recapitulate, Gödel's construction is a synthesis of the idea of predicative definition, combined with the idea of rank of sets and excluding the ordinals from any restriction of "predicativity". Since this work deals with set theory, it is of course not surprising that one cannot make any serious use of a restrictive notion of predicativity, which might not allow sufficiently many sets.

Let us pause here and contemplate the constructible sets. Each x in L has a "name", i.e., the formula which defines it, but it also is necessary to give the ordinal α at which it is being constructed. It is almost obvious that at each α, the ordinal α is constructed so that ordinals are *automatically* in L. It is this which distinguishes the constructible sets from other concepts which are strictly constructive. The lesson that L teaches us is that ordinals must not be questioned, but using ordinals we can construct other sets. I shall return to this theme when I discuss my own work. Intuitively, one might say things like, the constructible sets are the only sets one really needs, etc., but the question of whether they form a model for set theory obviously requires a rigorous proof. Probably this was the first question which Gödel attacked. The proof of this offers no real difficulty. The reason for this is that the two major axioms of ZF, the Power set axiom and the axiom of separation or replacement, assert precisely that certain sets exist consisting of all sets with a certain property. Since the construction of L allows all properties in the transition from A to A', it is not too difficult to see that the axioms hold in L. Of course, one must use very heavily the fact that all ordinals are allowed in the construction. The next step in Gödel's proof is to show that the axiom of choice holds in L. The axiom of choice says in one form that given any set X, there is a function which assigns to each nonempty element y of X, an element of y. Now in the construction of L, each element is constructed at a *least* ordinal. Furthermore, at each ordinal there are only countably many formulas into which may be inserted particular constants that have already been constructed at a previous ordinal. It is not difficult to say that, assuming we have well-ordered all the previously constructed sets, we obtain a well-ordering of whatever new sets are constructed at a given ordinal. Putting these altogether, we see that there is a definable well-ordering of L. This clearly gives a *definable* choice function by simply defining the choice function as the element which appears first in the well-ordering. At this point we have shown that "models" exist in which the axiom of choice is true, and hence we know that it is impossible to prove AC false from the axioms of ZF. This clearly is a momentous achievement. Nevertheless, viewed 65 years later, the proof has very little flavor of a mathematical character. Rather, it is an achievement of definitions and of a point of view. It reminds one somewhat of Cantor's original definition of cardinality and his proof of the nondenumerability of the continuum. There, too, there are only very slight mathematical complexities, but, especially in his definition of well-ordering, there was a point of view which was quite original in its day. The question of whether CH could be decided in L was more difficult. According to Dawson's biography of Gödel, the above results were obtained by 1935. From Gödel's notebooks, the proof that CH holds in L was obtained June 14, 1937, and he informed von Neumann of his success on July 13.

At least one author conjectures that the severe depression he suffered during much of those two years was due to the strain of working on the proof. In any event, an announcement was published in 1938 which gives almost no specifics, and a sketch of the proof which is actually complete in all respects was published in 1939. What was it about these problems which caused Gödel such anxiety and difficulty? The proof in a modern text such as Jech's encyclopedic work takes three pages of not very dense material, yet it involves a new step. It seems that every advance in logic of this era seems in retrospect to be almost of a philosophical nature, yet caused great difficulties in both discovering and even understanding. The answer to the question lies in the result which I have already described as the first nontrivial result of logic: the theorem of Löwenheim-Skolem.

The key lemma in the proof is the fact that every real number in L is constructed by a *countable* ordinal. Since the number of countable ordinals is, almost by definition, $\aleph_1$, it will follow that $C = \aleph_1$. Now to prove the lemma. Suppose that an ordinal α constructs a set x which is a set of integers. We construct a submodel, say N of L, containing α, containing the integers and containing x such that in N, "α constructs x" is still true. Since we are starting with only countably many objects, i.e., α, x and the integers, L-S says that there is a countable submodel. (Actually L-S says that we can even find a countable submodel in which every statement of L is true in N, but this could not be carried out in L itself. This is an example of the kind of minefield of confusions which no doubt Gödel faced.) Since N is countable, a simple argument shows that α is a countable ordinal inside N, and it easily follows that this countable ordinal also constructs x. For those familiar with the proof, I am omitting what Gödel called absoluteness, namely that the construction inside N is the same as that inside L. This is almost obvious but in a formal exposition, it does make the proof a little longer.

To show that the generalized continuum hypothesis holds, one starts with an $\aleph$ we call A and shows that in L, every subset of A is constructed by an ordinal of cardinality A. The proof is the same as above, except in the initial set used to construct the submodel N, we must use all the elements of A, hence the cardinality of A is seen to be that of A. With this we end the exposition of Gödel's famous result.

3. Independence of CH. After the publication of Gödel's result in 1939 and the appearance about a year later of a detailed exposition as lecture notes, there were a total of four papers to my knowledge which in any way dealt with his construction, until my own work in 1963. One of these, actually a series of three papers, was used by Shepherdson who showed that the method of inner models used by Gödel and von Neumann could never show the consistency of the negation of AC or CH. This result evidently received insufficient attention because, when I rediscovered them in 1962, I was urged to publish them despite some reservations I had. The other papers dealt with constructions of one set from another, i.e., relative constructibility. Furthermore, there was little mention of the problem of showing the consistency of the negation of CH. One reference was an expository article of Gödel, in which he refers to it as a likely outcome, but hardly seems to refer to it as a pressing problem for research. Why was this the case?

Firstly, although the first note of Gödel was a very good sketch of his results, the publication of the formal exposition in his usual fastidious style gave the impression of a very technical, even partially philosophical, result. Of course, it was a perfectly good mathematical result with a relatively straightforward proof. Let me give some impressions that I had obtained before actually reading the Princeton monograph but after a cursory inspection. Firstly, it did not actually construct a model, the traditional method, but gave a concept, namely constructibility, to construct an *inner* model. Secondly, it had an exaggerated emphasis on relatively minor points, in particular, the notion of *absoluteness*, which somehow seemed to be a new philosophical concept. From general impressions I had of the proof, there was a finality to it, an impression that somehow Gödel had mathematicized a philosophical concept, i.e., constructibility, and there seemed no possibility of doing this again, especially because the negation of CH and AC were regarded as pathological. I repeat that these hazy and even self-contradictory impressions I had were strictly my own, but, nevertheless, I think that it is very possible that others had similar impressions. For example, as a graduate student I had looked at Kleene's large book, and there was very little emphasis or even discussion of the entire matter. In a word, it was in a corner by itself, majestic and untouchable. Finally, there was a personal dimension in the matter. A rumor had circulated, very well known in all circles of logicians, that Gödel had actually partially solved the problem, specifically as I heard it, for AC and only for the theory of types (years later, after my own proof of the independence of CH, AC, etc., I asked Gödel directly about this and he confirmed that he had found such a method, specifically contradicted the idea that type theory was involved, but would tell me absolutely nothing of what he had done). The aura surrounding Gödel, for many purposes the founder of modern logic, was of course heightened by his almost total withdrawal and inaccessibility. Today we know more about Gödel's own activity due to the publication of various materials, and I can refer the reader to the very complete biography of Gödel by Dawson. In a letter to Menger, December 15, 1937, we learn that he was working on the independence of CH, "but don't know yet whether I will succeed with it." It seems that from 1941 to 1946 he devoted himself to attempts to prove the independence. In 1967 in a letter he wrote that he had indeed obtained some results in 1942 but could only reconstruct the proof of the independence of the axiom of constructibility, not that of AC, and in type theory (contradicting what he had told me in 1966). After 1946 he seems to have devoted himself entirely to philosophy. What strikes me as strange is that, although Gödel regarded the independence of CH as a most important problem, there seems to be no indication that anyone else was working on the problem.

Now in 1962 I began to think about proving independence. This arose from certain discussions that I had with Sol Feferman and Halsey Royden at Stanford about how one should view the foundations of mathematics. Feferman had spent his entire career on the program of proof theory which was begun by Hilbert. In our informal discussion I was advocating a kind of mixture of formalist and realist view which I thought meant that, in some sense, there was an intuitively very convincing way of looking at mathematics and convincing oneself that it was consistent. For some reason, I began to feel sufficiently challenged to give some lectures. After two or three I became discouraged that I had sunk into the same sort of polemics as had beset the mathematicians of 50 years before. However, I was convinced that I had a valuable way of looking at things, and at some point I decided to work on the independence problem. Since the axiom of choice plays a bigger role in conventional mathematics than does CH, I thought it simpler to think about AC. Also some work, namely the Frankel-Mostowski method, had been done on AC, although since it constructs totally artificial models of set theory I felt it was not relevant. At first I tried various devices which would attempt to construct 'indistinguishable' elements in a more natural way, but soon I found myself enmeshed in thinking about the structure of proofs. At this point I was not thinking about models, but rather syntactically. Also, I had not read Gödel's monograph for reasons that I mentioned above. Strangely enough, I leaned to the view that the consistency of the negation of AC would not in any way be related to the consistency of AC, since the latter was a "natural" result, and the former a "counterexample". I eventually came to several conclusions. One, there was no device of the type of Frankel-Mostowski or similar "tricks" which would give the result. Two, one would have to eventually analyze all possible proofs in some way and show that there was an inductive procedure to show that no proof is bringing one substantially closer to having a method of choosing one element from each set. Three, although there would have to be a semantic analysis in some sense, eventually one would have to construct a *standard* model. This third conclusion was to remain in the background, but for the moment I concentrated on the idea that by analyzing proofs one could by some kind of induction show that any proof of CH could be shortened to give a shorter proof, in some sense, and thus show that no such proof existed. But how to do this? I seemed to be in the same kind of circle that proof theory is in when it tries to show that a proof of a contradiction yields a shorter contradiction. Perhaps I should say that the notion of length of proof is not to be though of in a precise sense, but, in my thinking, I would feel that a given line of the proof might be questioned as to whether it makes any essential progress, or can be eliminated in some way.

The question I faced was this: How to perform any kind of "induction" on the length of a proof. It seemed some kind of inductive hypothesis might work, whereby if I showed that no "progress" was made in a choice function up to a certain point, then the next step would also not make any progress. It was at this point that I realized the connection with the models, specifically standard models. Instead of thinking about proofs, I would think about the formulas that defined sets, these formulas might involve other sets previously defined, etc. So if one thinks about sets, one sees that the induction is on the rank, and I am assuming that every set is defined by a formula. At this point I decided to look at Gödel's monograph, and I realized that this is exactly what the definition of constructibility does. I now had a firm foothold on a method, namely, to do the Gödel construction but obviously not exactly in the same way. In the Zermelo-Frankel method, one introduces artificial "atoms" or "Urlemente" at the lowest level and builds up from there. This, of course, makes the resulting model violate the fundamental principal of extensionality in that these atoms are all empty yet are not equal. So, something must be done about this, but the idea that one must build up in some sense seemed absolutely clear.

Now, at this point, I felt elated yet also very discouraged. Basically, all I had accomplished was to see that I would have to make a point of contact with the existing work, yet I had no new idea how to modify things. Nevertheless the feeling of elation was that I had eliminated many wrong possibilities by totally deserting the proof-theoretic approach. I was back in mathematics, not in philosophy. I still was not thinking about CH yet, so I was concentrating on trying to construct something like atoms. But, in mathematics the integers are the atoms, so to speak, and there is no way to introduce artificial integers. It would have to be at the next level, sets of integers. Now things became still clearer, I would introduce new sets of integers to an existing model. Thus I assumed immediately that I had a standard model of set theory, which fact although "obvious" cannot be proved in ZF, since it violates the fact that the consistency of ZF cannot be proved in ZF. I felt I had to leave ZF, even if by ever so little, since the existence of a standard model says a little more than consistency, but not much. Now the construction of Gödel does of course construct a model, but I soon realized that it does not actually correspond to the kind of proof analysis that I had in mind. Namely, it is not specifically tailored to the axioms of ZF, but gives a very generous definition of a "construction". Therefore, I modified his definition of the construction A to A' mentioned above, to be that where only those subsets of A which are required to exist by the axioms of ZF. For example, we do NOT require that the set consisting of all elements of A be put in A'. This would correspond to demanding that a set of all sets exists. I do not recall what I thought would emerge from this new construction, but after a brief interval it became clear that one constructs the *minimal model* (standard) for ZF. Further, it is countable, as a quick application of Löwenheim-Skolem shows. Most importantly, since it is minimal, no definition of an inner model by means of a formula could yield another model; hence it follows that there is no inner model in which the negation of CH, or AC, or the axiom of constructibility, exists. I was happy with these results as they represented the first concrete progress I had made. As mentioned earlier it developed that I had been anticipated by Shepherdson about ten years before. I did find it strange that it had not been pointed out even sooner, perhaps by Gödel in his review article, and I did feel a certain confidence, that even though I was an outsider, I had good intuition. I was thus given a clue that countable models would play an important role and that my dream of examining every possible formula individually must be close to the truth. There was another negative result, equally simple, that remained unnoticed until after my proof was completed. This says one cannot prove the existence of any *uncountable* standard model in which AC holds, and CH is false (this does not mean that in the universe CH is true, merely that one cannot prove the existence of such a model even granting the existence of standard models, or even any of the higher axioms of infinity). The proof is as follows: If M is an uncountable standard model in which AC holds, it is easy to see that M contains all countable ordinals. If the axiom of constructibility is assumed, this means that all the real numbers are in M and constructible in M. Hence CH holds. I only saw this after I was asked at a lecture why I only worked with countable models, whereupon the above proof occurred to me. Again, this result shows how little serious work was being done in the field after 1937. For those who know some rudiments of Model theory, it is a theorem that a consistent theory which has an infinite model has models of any cardinality. The above result refers to *standard* models only. This is another indication of how the decision to restrict myself only to *standard* models was justified by intuition.

So we are starting with a countable standard model M, and we wish to adjoin new elements and still obtain a model. An important decision is that no new ordinals are to be created. Just as Gödel did not remove any ordinals in the constructible universe, a kind of converse decision is made not to add any new ordinals. The simplest adjunction that one can make is to adjoin a single set of integers. (Incidentally, this problem of how to adjoin a single set of integers to a model was pointed out by Skolem in his general remarks about how the axioms fail to characterize the universe of sets uniquely.) Now one can trivially adjoin an element already in M. To test the intuition, one should try to adjoin to M an element which enjoys no "specific" property to M, i.e., something akin to a variable adjunction to a field. I called such an element a "generic" element. Now the problem is to make precise this notion of a generic element. If one can manage to adjoin one such element, then one would have a method to adjoin many and thus create many different models with various properties. Thus the essence of the problem has become to give a precise definition of a generic set. Also I had the hope that, because a set, say a, was generic, it should imply that when one adjoins a to the model M by means of the analog of the Gödel construction, the resulting object $M(a)$ *would still be a model for ZF*. This last point cannot be truly justified except by a detailed examination of the proof. Let me give some heuristic motivations.

Suppose M were a countable model. Up until now we have not discussed the role countability might play. This means that all the sets of M are countable, although the enumeration of some sets of M does not exist in M. The simplest example would be the uncountable ordinals in M. These of course are actually countable ordinals, and hence there is an ordinal I, not in M, which is countable, and which is larger than all the ordinals of M. Since I is countable, it can be expressed as a relation on the integers and hence coded as a set a of integers. Now if by misfortune we try to adjoin this a to M, the result cannot possibly be a model for ZF. For if it were, the ordinal I as coded by I would have to appear in $M(a)$. However, we also made the rigid assumption that we were going to add *no new ordinals.* This is a contradiction, so that $M(a)$ cannot be a model. From this example, we learn of the extreme danger in allowing new sets to exist. Yet a itself is a new set. How then can we satisfy these two conflicting demands?

There are certainly moments in any mathematical discovery when the resolution of a problem takes place at such a subconscious level that, in retrospect, it seems impossible to dissect it and explain its origin. Rather, the entire idea presents itself at once, often perhaps in a vague form, but gradually becomes more precise. Since the entire new "model" $M(a)$ is constructed by transfinite induction on ordinals, the definition of what is meant by saying a is generic must also be given by a transfinite induction. Yet a, as a set of integers, occurs very early in the rank hierarchy of sets, so there can be no question of building a by means of a transfinite induction. The answer is this: the set a will not be determined completely, yet properties of a will be completely determined on the basis of very incomplete information about a. I would like to pause and ask the reader to contemplate the seeming contradiction in the above. This idea as it presented itself to me, appeared so different from any normal way of thinking, that I felt it could have enormous consequences. On the other hand, it seemed to skirt the possibility of contradiction in a very perilous manner. Of course, a new generation has arisen who imbibe this idea with their first serious exposure to set theory, and for them, presumably, it does not have the mystical quality that it had for me when I first thought of it. How could one decide whether a statement about a is true, before we have a? In a somewhat exaggerated sense, it seemed that I would have to examine the very meaning of truth and think about it in a new way. Now the definition of truth is obvious. It is done by induction on the number of quantifiers. Thus, if a statement "there exists x, $A(x)$" is examined, we decide whether it is true by looking at $A(x)$ for every possible x. Thus the number of quantifiers is reduced by 1. So the definition of truth would proceed by induction on the number of quantifiers and on the rank of the sets being looked at.

There are some statements, called elementary statements, that cannot possibly be reduced, neither by lowering the rank of the sets it involves, nor by removing quantifiers. These are statements of the form n is in a, where n is an integer. Since there is no way they can be deduced, they must be taken as given. An elementary statement (or forcing condition) is a finite number of statements of the form n in a, or n not in a, which are not contradictory. It is plausible to conjecture that, whatever the definition of truth is, it can be decided by our inductive definition from the knowledge of a finite number of elementary statements. This is the notion of *forcing*. If we denote the elementary conditions by P, we must now define the notion "P forces a statement S". The name, *forcing*, was chosen so as to draw the analogy with the usual concept of implication, but in a new sense. How shall we define *forcing* by transfinite induction?

Again, the generation which grew up with forcing cannot easily imagine the uncertainty with which I faced giving a precise definition. It seemed that it might be too much to ask to hope that a finite number of conditions on a would be enough to decide everything. Furthermore, there was this large question looming. Even if one could systematically decide what one would like to be true, what would actually make it true in the final model. I do not recall the precise sequence of events, but my best guess is that this point I answered first. Namely, if one assumes the model M is countable, then one can ask every question in sequence, deciding every one. But here again another danger lurked. If one did this, then the enumeration would be done outside the model M, and so one had to be sure that there was no contradiction in both working in and out of the model. This was the price that had to be paid if one leaves the security of inner models. For the moment, let us ignore the question of what the final definition of the set a will be, and try to develop a notion of an elementary condition P forcing a statement S.

Let us only deal with statements S which have a *rank*. This means that all variables, and all constants occurring in the statement, deal with sets whose rank is bounded by some ordinal. For the moment we are not allowing statements which have variables ranging over the entire model. All our sets and variables are actually functions of the "generic" set a. So in analogy with field theory, we are actually dealing with the space of all (rational) functions of a, not actual sets. Clear the elementary statements P force statements n in a, or n not in a, precisely when these are contained in P. Now we have a formal definition: an elementary statement is a finite set of statements n in a, or not n in a which are not contradictory. Suppose a statement begins with "there exists" a set x of rank less than α, such that $A(x)$ holds. If we have an example of a set (actually a function of a) such that P does force $A(x)$, clearly we have no choice (forced) to say that P forces "there exists … ". *Emphatically not.* For it may very well be that we shall later find an elementary condition which does force the existence of such an x. So we must treat the two quantifiers a bit differently. Now we must reexamine something about our elementary conditions. If P is such, we must allow the possibility that we shall later make further assumptions about the set a, which *must be consistent with* P. This means that we are using a natural partial ordering among these conditions. We say $P < Q$, if all the conditions of P are contained in Q. That is, Q is further along in determining the final a.

This leads to a formal definition of forcing which I give here in a somewhat abbreviated form:

(a) P forces "there exists x, $A(x)$" if, for some x with the required rank, P forces $A(x)$.

(b) P forces "for all x, $A(x)$" if no $Q > P$ is such that Q forces the negation, i.e., for some y, Q forces not $A(y)$.

The reader may feel that these requirements are clearly warranted, but are they sufficient to completely construct a? The answer is not entirely obvious. By themselves they seem to leave most questions about a unresolved. However, we need another definition.

A sequence $P_q, P_2, \ldots$ is called complete if it is an increasing sequence in the sense of the partial ordering of the P, and if *every* statement S is forced by some P_k.

Actually, we have only defined forcing for statements which have rank, but since a is completely determined by statements n in a, this will determine a. Now we have a series of quick lemmas which establish that forcing is a good notion of truth. First, two obvious ones:

Lemma 1. *P forces a statement S and its negation.*

Lemma 2. *If P forces S, then for every Q, $Q > P$, Q forces S.*

Then a surprising little lemma, which is crucial.

Lemma 3. *For all P and S, there is an extension Q of P such that Q forces either S or Q forces not S.*

All these lemmas follow almost immediately from the definitions. It follows that a complete sequence exists. Now if P_n is a complete sequence, for each integer k the statement k in a, or k not in a, must be forced by some P_n. Thus it is easy to see that a is determined by P_n. Finally we have a truth lemma.

Lemma 4. *Let P_n be a complete sequence. A statement S is true in $M(a)$ if and only if some P_n forces S.*

With this we complete all the elementary lemmas of forcing. Now one must show that $M(a)$ is a model. Here one encounters basic differences with the Gödel result. Let us look at the power set axiom. If x is a set in the model, it occurs at a certain ordinal α. One must show that all the subsets of x occur before some ordinal β. This argument must be carried out in M since we are dealing with ordinals of M. Yet a is not in M, so we cannot discuss x as a set, but only as designated by the ordinal α, a function of a. To work inside M, we consider the set of P which forces a given set to lie in a or not lie in a. Because forcing is defined in M, we can look at all possibilities of assigning sets of P, which force the members of x to lie in an arbitrary y. This set is the "truth value" of the statement. So, in ordinary set theory a subset of x is determined by a two-valued function on the members of x. In our situation, a subset is determined by a function taking its values in the subset of the elementary conditions. *These values are all in the model M.* Thus we can quantify over all possible truth values and, by a simple argument, show that any subset of x occurs before some ordinal β which is in M. The other axioms are proved in essentially the same manner.

Now we have a method for constructing interesting new models. How do we know that a is a "new" set not already contained in M? A simple argument shows that, for any a' in M and P, we can force a to be not equal to a' by choosing any n, not already determined by P, and simply extending P by adding n in a, or n not in a, to prevent a from being equal to a'. In this way we see that a is not constructible, and so we have a model with a nonconstructible set of integers.

This follows essentially my original presentation. Now Solovay pointed out that the subsets of P which determine the truth or falsity of each statement are essentially elements of a Boolean algebra. If one takes this approach, one need never actually choose a complete sequence, but instead say that we have a Boolean valued model and the basic lemmas imply that this behaves sufficiently similar to ordinary truth, that we can see that since the *all* P essentially force a to be nonconstructible, then we cannot prove that a is constructible.

To make the analogy with Boolean algebras more precise, one can perfect the notion of forcing by saying P forces a statement S, if no extension of P forces the negation of S. This remark was made by Scott, and allows one to say that a necessary and sufficient condition on P, such that every complete sequence with P occurring forces S, is exactly that P forces S in this slightly stronger sense.

The connection between Boolean algebras and our elementary conditions is as follows: A Boolean algebra has as its canonical model a set of subsets of a given set X, closed under intersection, union, and complementation. The elementary conditions merely have a transitive partial ordering. If B is a Boolean algebra, then we can use the relation of subset as a partial ordering. That is, we say $p \leq q$, if $p \cap q = p$. Conversely, every partially ordered (p.o.) set gives rise to a Boolean algebra as follows.

Let P be a p.o. set. We define a subset U of P to be a 'regular cut,' if

i) $p \leq q$ and q in U implies p is in U.

ii) If p is not in U, then there is a $q \leq p$ such that no $r \leq q$ has r in U. One can now show that the set of regular cuts forms a Boolean algebra in a suitable sense and even a complete Boolean algebra. We do not give the details, but the analogy is clear. Note, however, that the partial order used here is the reverse of what we were using above. As mentioned, using the language of Boolean algebras brings our technique of forcing closer to standard usages.

Having shown how to adjoin one "generic" element, I show the rest of the basic theory develops rather smoothly. For example, suppose one wishes to violate the continuum hypothesis. The obvious way is to adjoin sets of integers a_i where i ranges over all the ordinals less than $\aleph_2$. Because of the general properties of generic sets, it will be clear that all these a_i will be distinct. The intuition behind this and behind almost all the results proved by forcing is that a relationship between the generic sets will not hold, unless the elementary conditions more or less are designed to make that happen. However, there is a complication. Namely, the $\aleph_2$ that we are using is that ordinal in the original model M. But the statement of CH is that the cardinality of the continuum is the first uncountable cardinal. It is therefore necessary to show that $\aleph_2$ in M is the second uncountable cardinal in the new model. When I came across this point, it was completely unexpected. Indeed, given the rumors that had circulated that Gödel was unable to handle CH, I experienced a certain degree of unease at this moment. However, the intuition I referred to above would indicate that, since there is no reference to ordinals in the elementary conditions, there is no reason, a priori, to think that the relations among the ordinals will be changed. Indeed this is the case. One can show that if two ordinals have different cardinality in M, they will have different cardinality in the new model. There is an important fact about the elementary conditions which is responsible for it. This is the *countable chain condition*. A p.o. set is said to satisfy the c.c.c. if every set of mutually incompatible elements is countable (p and q are incompatible if there is no r which extends both of them). The proof of this offers no particular difficulty. One other point remains and this caused me even more difficulty. Having adjoined $\aleph_2$ elements to the continuum, we can only say that C is at least $\aleph_2$. If one examines the above proof of the power set axiom, it is relatively easy to see that the number of subset of the integers is at most the number of countable sequences taking values in the Boolean algebra. A simple-minded analysis shows that the cardinality of the Boolean algebra is at most $\aleph_3$ since we are dealing with subsets of elementary conditions. So, for a period I could only show that in the model there are at least $\aleph_2$ elements in the continuum and at most $\aleph_3$. Again a closer examination shows that there are really only $\aleph_2$ elements. This is essentially the same proof as the countable chain condition.

One can repeat the argument with any ordinal and show that the continuum is greater than any given ordinal in M. However there is one old result about CH due to König. Namely, C cannot be the countable sum of smaller cardinals. This rules out, for example, $\aleph_\varpi$. How does the above analysis take this into account? Very nicely, once one sees that when we try to adjoin, say B elements, where B is a cardinal in M, the cardinality of the continuum becomes the number of countable subsets of M. Using the method of König this will be B, if B is not a countable sum of smaller cardinals, but will be the first cardinal greater than B if it is. Thus, the method of forcing produces, in some sense, best possible results.

The number and variety of results that were soon proved after the discovery of forcing makes it impossible to give any kind of reasonable summary. The axiom of choice has so many different formulations that it produced many questions. Cantor's original interest in it was in showing the existence of a well-ordering of the reals. The negation of this can be produced in a suitable model. One of the most striking results is that even the axiom of choice for a countable set of pairs can be negated. Clearly, from a pair of reals, one can always choose the smaller. The first possible place a counterexample can occur is thus with sets of real numbers. Indeed a model with the impossibility of such a choice function can be shown to exist.

Let me mention one more type of model which leads to completely new phenomena. It is possible to change cardinalities in the new model. That is, cardinals which in M are not equal can be made equal in the new model. If we make $\aleph_1$ countable, say, then all the cardinals shift down by one. In the new model there will only be countable constructible real numbers. This requires a more complicated definition of elementary conditions. In the simplest models described above, one dealt with finitely many statements of the general form n is in a, where n is an integer, a a generic set of integers. In order to change cardinals, one must consider functions defined on a finite subset of an ordinal α into an ordinal β. Still other models require elementary conditions which have infinitely many statements. These are necessary when one wishes to control cardinal arithmetic for several cardinals simultaneously. I will not give details but simply say that at this point there is very little set theory left, there are only combinatorial problems associated with the particular p.o. set used as the forcing conditions.

4. Some observations of a more subjective nature. For most mathematicians it is almost irresistible to ask the question, despite the independence results, is CH true or false? Therefore, I think I would be shirking my responsibility if I did not add my opinion about this question. Let me begin by repeating what I have already said about religious debate. I have no desire to convert anyone to my view, nor do I pretend that my own view is without problems and contradictions. I have found that in such conversations the one who seems to be the apparent victor is not at all the one who has the best understanding but rather one who is skillful in arguing and in projecting his personality. It is a skill that I am woefully deficient in. Let me merely say a few words about my own attitude toward set theory.

Everyone agrees that, whether or not one believes that set theory refers to an existing reality, there is a beauty in its simplicity and in its scope. Someone who rejects that sets exist as "completed wholes" swimming in an ethereal fluid beyond all direct human experience has the formidable task of explaining from whence this beauty derives. On the other hand, how can one assert that something like the continuum exists when there is no way one could even in principle search it, or even worse, search the set of all subsets, to see if there was a set of intermediate cardinality? Faced with these two choices, I choose the first. The only reality we truly comprehend is that of our own experience. But we have a wonderful ability to extrapolate. The laws of the infinite are extrapolations of our experience with the finite. If there is something infinite, perhaps it is the wonderful intuition we have which allows us to sense what axioms will lead to a consistent and beautiful system such as our contemporary set theory. The ultimate response to CH must be looked at in human, almost sociological terms. We will debate, experiment, prove and conjecture until some picture emerges that satisfies this wonderful taskmaster that is our intuition. As I said in my monograph some years ago, I think the consensus will be that CH is false. The intuition that pleases me most strongly is the following: The axiom of separation, or replacement, and the axiom of the power set are in some sense orthogonal to each other. No process of describing a cardinal by a property of the type used in the replacement axiom (here I must be vague) can adequately describe the size of the continuum. Thus I feel that C is greater than $\aleph_2$, etc.

Curiously enough, I must say that this attitude has a counterpart in the thinking of a strong realist such as Gödel himself. He told me that it was unthinkable that our intuition would not eventually discover an axiom that would resolve CH. In both our viewpoints there is an ultimate reliance upon an internal arbiter to decide the issue, by means of criteria which we do not yet know. For Gödel, this would be some kind of absolute, sudden appearance of a grand new idea. For me, perhaps, an evolutionary point of view that develops in the body politic of mathematics as a whole. Again, I must repeat that I do not take my own view completely seriously, at least not to the extent of defending it at all costs.

For me, it is the aesthetics which may very well be the final arbiter. I agree with Hilbert that Cantor created a paradise for us. For Hilbert, I think it was a paradise because it put the mathematics he loved beyond all criticism, gave it a foundation that would withstand all criticism. For me, it is rather a paradise of beautiful results, in the end only dealing with the finite but living in the infinity of our own minds.

Finally a personal remark. I cannot say that I was a friend of Kurt Gödel. We met relatively few times and there was a gulf of age and background that I found difficult to bridge. Yet, my meetings with him were charged with an emotion that was intense, yet difficult to describe. We each traversed journeys that had much in common. I would like to dedicate this talk to his memory.

REFERENCES

1. Kurt Gödel, *Consistency of the axiom of choice and the generalized continuum hypothesis*, Proc. Nat. Acad. Sciences **24** (1938), 556–557.

2. Jean van Heijenoort, *From Frege to Gödel*, Harvard Univ. Press, Cambridge, MA, 1967.

3. Thoralf Skolem, *Logisch-kombinatorische Untersuchungen über die Erfüllbarkeit oder Beweisbarkeit mathematischer Sätze nebst einem Theorem über dichte Mengen*, Videnskapsselskapets skrifter, I. Matematisk-naturvidenskabelig klasse, 1920.

4. ———, *Einige Bemerkungen zur axiomatischen Begründung der Mengenlehre*, Matematikerkongressen i Helsingfors den 4–7 Juli 1922 Den femte skandinaviska matematikerkongressen, Redogoerelse, 1922.

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