THE BULLETIN OF SYMBOLIC LOGIC
Volume 14, Number 3, Sept. 2008

COHEN AND SET THEORY

AKIHIRO KANAMORI

Abstract. We discuss the work of Paul Cohen in set theory and its influence, especially the background, discovery, development of forcing.

Paul Joseph Cohen (1934–2007) in 1963 established the independence of the Axiom of Choice (AC) from ZF and the independence of the Continuum Hypothesis (CH) from ZFC. That is, he established that Con(ZF) implies Con(ZF + ¬AC) and Con(ZFC) implies Con(ZFC + ¬CH). Already prominent as an analyst, Cohen had ventured into set theory with fresh eyes and an open-mindedness about possibilities. These results delimited ZF and ZFC in terms of the two fundamental issues at the beginnings of set theory. But beyond that, Cohen's proofs were the inaugural examples of a new technique, *forcing*, which was to become a remarkably general and flexible method for extending models of set theory. Forcing has strong intuitive underpinnings and reinforces the notion of set as given by the first-order ZF axioms with conspicuous uses of Replacement and Foundation. If Gödel's construction of L had launched set theory as a distinctive field of mathematics, then Cohen's forcing began its transformation into a modern, sophisticated one.

The extent and breadth of the expansion of set theory henceforth dwarfed all that came before, both in terms of the numbers of people involved and the results established. With clear intimations of a new and concrete way of building models, set theorists rushed in and with forcing were soon establishing a cornucopia of relative consistency results, truths in a wider sense, with some illuminating classical problems of mathematics. Soon, ZFC became quite unlike Euclidean geometry and much like group theory, with a wide range of models of set theory being investigated for their own sake. Set theory had undergone a sea-change, and with the subject so enriched, it is difficult to convey the strangeness of it.

Received April 24, 2008.

This is the full text of an invited address given at the annual meeting of the Association for Symbolic Logic held at Irvine in March 2008. My thanks to Juliet Floyd, and, for helpful communications, to Andreas Blass, Solomon Feferman, Alexander Kechris, Elliott Mendelson, and Dana Scott.

1079-8986/08/1403-0003/$3.80

How did forcing come about? How did it develop into a general method? What is the extent of Cohen's achievement and its relation to subsequent events? How did Cohen himself view his work and mathematics in general at the time and late in his life? These are related questions that we keep in mind in what follows and reprise at the end, with an emphasis on the mathematical themes and details and the historical progression insofar as it draws out the mathematical development.[1]

§**1. Before.** Cohen was and is considered an analyst who made a transforming contribution to set theory. Because forcing is such a singular phenomenon, Cohen has at times been regarded as a bit of a brash carpetbagger, an opportunist who was fortunate and brilliant enough to have made a decisive breakthrough. Be that as it may, we try here to paint a more discriminating picture and to draw out some connections and continuities, both historical and mathematical, from a time when mathematics was simpler and not so Balkanized.

Cohen became a graduate student in 1953, at age 19, at the University of Chicago; received a master's degree a year later; and then his doctorate in 1958. This was a veritable golden age for the emerging university and its department of mathematics. Irving Kaplansky and Saunders MacLane made strong impressions on the young Cohen,[2] and they as well as Shiing-Shen Chern, Irving Segal, and André Weil were making crucial advances in their fields. As for foundations, Paul Halmos and MacLane had interests and Elliott Mendelson was an instructor for 1955–1956.

From early on Cohen had developed what was to be an abiding interest in number theory, an area of mathematics which he found attractive because of the simplicity of its statements and complexity and ingenuity of its proofs.[3] Cohen recalled working on a famous problem in diophantine approximation issuing from work of Axel Thue and Carl Siegel, loosely speaking whether an algebraic number can have only finitely "good" rational approximations, until one day the number theorist Swinnerton-Dyer knocked on his door and told him that the problem had been solved by Klaus Roth; for this Roth received the Fields Medal in 1958.[4] Whether it was from this formative period or later, Cohen, as can be seen in his writings, developed a conviction about the centrality of number theory in mathematics. Significantly, Cohen by this time also developed an interest in logic, this issuing from decidability

[1]Moore [44] is a detailed, historical account of the origins of forcing; Cohen [19] is an extended reminiscence of the discovery of forcing four decades later; and Albers–Alexanderson–Reid [1], pp. 43–58 is a portrait of Cohen based on an extended interview with him. We shall be referring to these in what follows. Yandell [64], pp. 59–83 is another account of Cohen and his work, largely based on [44], [1], and a phone conversation with Cohen.

[2]See [19], p. 1071.

[3]See [21].

[4]See [1], p. 49, 56.

questions in number theory.[5] He recalled spending time with future logicians, especially William Howard, Anil Nerode, Raymond Smullyan, and Stanley Tennenbaum, in this way picking up a good deal of logic, and also reading Kleene's *Introduction to Metamathematics*.[6] Also, one of his office-mates was Michael Morley, a soon-to-be prominent model-theorist.[7] Whatever the case, there was little in the way of possibilities for apprenticeship in number theory at that time in Chicago.

Moving to a different field, Cohen, toward his dissertation, worked in harmonic analysis with the well-known Antoni Zygmund of the Polish mathematical school, who had a large following and developed what then came to be known as the Chicago School of Analysis. Zygmund was among the next generation just after those who founded the Polish school, and, if we follow up one path of the genealogy tree, was himself a student of Aleksander Rajchman and Stefan Mazurkiewicz, the latter a student of Wacław Sierpiński.[8] The incipient historical connection with set theory extended to Cohen's dissertation, entitled *Topics in the Theory of Uniqueness of Trigonometrical Series*—on the very subject that Georg Cantor first established results in 1871 about derived sets using his "symbols of infinity" that eventually became the ordinal numbers. The main work of the dissertation would remain unpublished, with only a note [7] extracted which established an optimal form of Green's theorem. This initial publication of Cohen's did have a *mathematical* thread which we expand on forthwith.

There is one antecedent kind of mathematical construction that trickles from the the beginnings of set theory into topology and analysis and impinges on forcing—by way of category. Set theory was born on that day in December in 1873 when Cantor established that the continuum is not countable. In a proof in which the much later diagonal argument is arguably implicit, Cantor defined, given a countable sequence of reals, a sequence of nested intervals so that any real in their intersection will not be in the sequence. Twenty-five years later, with much the same proof idea, René Baire in his 1899 thesis established the Baire Category Theorem, which in a felicitous formulation for our context asserts that *the intersection of countably many dense open sets is dense*.[9]

The Baire Category Theorem would become widely applied by the Polish school and, through its dissemination of ideas, across topology and analysis. Applications of the theorem establish existence assertions in a topological space through successive approximations. Typically the process can start anywhere in the space and therefore produce a large class of witnesses. The

[5]See [1], p. 50.
[6]See [44], p. 154 and [1], p. 51.
[7]See [21].
[8]Incidentally, Cohen himself was the son of Jewish immigrants from Poland.
[9]This holds in abstract terms in any complete metric or locally compact space.

arguments can have an involved preamble having to do with the underlying topology and the dense open sets but then can be remarkably short. On the one hand, the existence assertion is not always buttressed by an "explicit construction", but on the other hand, there is the conclusion that apparent "pathologies" are almost everywhere.

In 1931 Stefan Banach [3] provided new sense to Weierstrass's classical construction of a continuous nowhere-differentiable function by devising a remarkably short proof via the Baire Category Theorem that in the space $C[0,1]$ of continuous functions on the unit interval with the uniform metric (i.e., using the sup norm) *the nowhere differentiable functions are co-meager*. With Banach's result achieving expository popularity, Cohen would presumably have been cognizant of it, especially in the imported Polish air of Chicago.

In any case, in that initial publication [7] of Cohen's drawn from his dissertation, there is an explicit appeal to the Baire Category theorem to attend to an issue about differentiability in a manner similar to Banach's.

A final connection, rather tenuous, has to do with stable dynamical systems. After Stephen Smale did his work on the Poincaré Conjecture for which he would get the Fields Medal in 1966, the same year that Cohen would, Smale in the mid-1960s initiated a program to determine the stable dynamical systems. For a compact differentiable manifold M and the space of diffeomorphisms $M \longrightarrow M$ with the uniform metric (all this possibly elaborated with higher derivatives), Smale in a well-known paper [51] defined a property of diffeomorphisms to be *generic* if the set of diffeomorphisms satisfying it is co-meager. Regarding diffeomorphisms as dynamical systems (via their iteration) Smale set out to show that the generic properties determine the stable dynamical systems. Smale often applied the Baire Category Theorem in various ways, but on the other hand he eventually came to a successful formulation of what a stable dynamical system is and moreover showed it to be distinct from genericity.

Back to Cohen. Before official receipt of his Ph.D. Cohen took up an instructorship at the University of Rochester for the academic year 1957–1958 and then an instructorship at the Massachusetts Institute of Technology for 1958–1959. At MIT Cohen came into contact with logicians Azriel Levy and Hartley Rogers, and among others, especially John Nash. Sylvia Nasar's well-known biography of Nash, *A Beautiful Mind* [45], candidly describes Cohen and his interactions with Nash. She wrote of Cohen (p. 237):

> He spoke several languages. He played the piano. His ambitions were seemingly unlimited and he spoke, from one moment to the next, of becoming a physicist, a composer, even a novelist. [Eli] Stein, who became a close friend of Cohen's, said: "What drives Cohen is that he's going to be better than any other guy. He's going solve the big problems. He looks down on mathematicians who do

mathematics for the sake of making incremental improvements in the field."

Several years before, Nash had done the work in game theory that would eventually lead to the Nobel Prize and had established his most important and difficult result on the embeddability of compact Riemannian manifolds into Euclidean space. Nasar described how Nash and Cohen had long and charged discussions about mathematical problems, especially the Riemann Hypothesis.[10]

Of concrete results, Cohen in 1958 had started to work on measures on locally compact abelian groups. Within a year he made a significant advance on a conjecture of Littlewood in Fourier analysis about the lower bound on a exponential sum[11] and with it characterized the idempotent measures on locally compact abelian groups. For this Cohen would in 1964 receive the prestigious Bôcher Memorial prize awarded by the American Mathematical Society "for a notable paper in analysis published during the preceding six years". The citations for this award are mostly about a body of esteemed work, but in Cohen's case it was indeed for "a notable paper", [8]. Cohen was able to made a considerable advance with concrete means, at one point (p. 196) using a lemma about finite integers that could have been derived via the Finite Ramsey Theorem. Notably, in a subsequent paper [9] Cohen continued his emphasis on concrete means by providing a method for eliminating appeals to the Axiom of Choice from several known applications of Banach algebras to classical analysis.

Cohen spent the years 1959–1961 at the Institute for Advanced Study at Princeton as a fellow and then became an assistant professor at Stanford University. Having put aside the Riemann Hypothesis and with his particular work on locally compact abelian groups having gone as far as it could, Cohen during this period turned to big problems of logic. Both at the Institute and at Stanford, he discussed issues with Solomon Feferman, and at Stanford to a lesser extent with Georg Kreisel. In 1961–1962 Cohen focused on the consistency of analysis and even conducted a seminar on his work, but abandoned his approach when it failed to get past arithmetic. Even then, his assimilation of and admiration for the consistency work in proof theory would play an important role in his later mathematical thinking. At the end of 1962 Cohen moved on to the independence of AC.[12]

What was the state of set theory at that time? In the axiomatic tradition Gödel's relative consistency result for AC and CH through the inner model L of constructibility had stood as an isolated monument for quite a

[10] See [45], p. 238. It is said that this association contributed to Nash's first psychiatric commitment for schizophrenia in 1959.

[11] This is not what is widely known as "Littlewood's conjecture", about lattice points in Diophantine approximation theory.

[12] See [44], p. 155.

number of years. To the extent that axiomatics and inner models were investigated at all, the papers were couched in the exacting formalism of Gödel's 1940 monograph [30], even to the reverent citation of the axioms through their groupings. Starting in the mid-1950s however, new model-theoretic initiatives informed the situation, and with Alfred Tarski established at the University of California at Berkeley a large part of the development would take place there. In the new terms, Gödel's main, CH argument for L was better understood as a direct Skolem hull and elementary substructure argument, something that had been obscured by [30].

The emergence of the ultraproduct construction for providing a concrete, algebraic means of building models led to a revitalization of the theory of large cardinals. Building on the work of Jerome Keisler, Dana Scott [47] in 1961 took an ultrapower of the set-theoretic universe V itself to establish that having a measurable cardinal contradicts Gödel's Axiom of Constructibility $V = L$. With the ultrapower set theory was brought to the point of entertaining elementary *embeddings* into well-founded models. It was soon to be transfigured by a new means for getting well-founded *extensions* of well-founded models. At the 1962 International Congress of Mathematicians at Stockholm, Scott presented his result about measurability and L, and Cohen [10], his work on idempotent measures on locally compact abelian groups. At the 1966 congress at Moscow, Cohen was awarded the Fields Medal for the independence of the Axiom of Choice and of the Continuum Hypothesis.

§2. The minimal model. Cohen's progress to his independence results would be by way of the minimal model of set theory. Importantly, overt proof-theoretic approaches would give way to increasingly semantic approaches. In the retrospective [19], Cohen recalled at length how he had come to forcing. As to why there had been little work on the problem of independence, Cohen adduced two reasons. The first pertained to the obtuseness of Gödel's monograph [30] as compared to his first announcement [29], and what Cohen wrote is interestingly revelatory (p. 1086 ff):

> ... 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 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.

As here, Cohen persistently regarded "philosophical" attitudes with their emphasis on concepts and proofs as having been detrimental and would stress how it was a "mathematical" approach with constructions and models that led to success. The other reason that Cohen adduced for the lack of progress on independence was that rumor had it that Gödel had partially solved the independence problem, at least for AC.

Cohen went on to describe his early, 1962 speculations about independence. He first focused on just the independence of AC as simpler and because some work had already been done on it, namely the Fraenkel–Mostowski model-building with urelements (atoms). He eventually came to several conclusions (p. 1088):

> One, there is 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 is an inductive procedure to show that no proof is bringing one substantially closer to having a method of choosing one element from each set. Third, although there would have to be a semantic analysis in some sense, eventually one would have to construct a *standard* model.

A *standard* model is also known as an $\in$-model, a set which together with the membership relation restricted to it is a model of set theory; it was clear to Cohen from the beginning that any well-founded model of set theory is isomorphic to a transitive standard model, and the assumption of transitivity for standard models is implicit in his work. We see here how Cohen focused on standard models and their concreteness from the beginning. Presumably with his proof-theoretic work on the consistency of analysis influencing him, Cohen initially worked on devising some kind of induction on length of proofs. He continued (p. 1089):

> It seemed some kind of inductive hypothesis would 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 previous 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.

Cohen then saw that Gödel's construction did not correspond to the kind of proof analysis that he had in mind. "Namely, it is not specifically tailored to the axioms of ZF, but gives a very generous definition of 'construction'." (p. 1090) From previous passages, it becomes obvious that this "generosity" is the incorporation of all the ordinals as impredicatively given. Cohen therefore modified the construction, and came up with his first, precursory result in set theory, that there is a minimal model of ZF, i.e., among all standard models there is one that is isomorphically embedded in every other (which of course must have transitive form L_γ for some countable $\gamma < \omega_1$). This result in particular implied that taking inner models cannot establish independence, and Cohen was happy with it, as it represented the first concrete progress he had made.[13]

Cohen recalled that both Kreisel and Scott urged that this result be published and he proceeded to do so though he was astounded that such a simple result was apparently unknown. Only later did Cohen become aware that he had duplicated a result of John Shepherdson published a decade earlier.[14]

The Shepherdson [48] and Cohen [12] papers are a study in contrasts that speaks to the historical distance and the coming breakthrough. While both papers are devoted to establishing essentially the same result, the former takes 20 pages and latter only 4. Shepherdson labors in the Gödelian formalism with its careful laying out of axioms and propositions in first-order logic, while Cohen proceeds informally and draws on mathematical experience. Shepherdson works out the relativization of formulas, worries about absoluteness and comes down to the minimal model, while Cohen takes an algebraic closure. As he writes (p. 537),

> We observe that the idea of a minimal collection of objects satisfying certain axioms is well known in mathematics, for example, in group theory one often considers the subgroup generated by a collection of elements and in measure theory we define the Borel sets as the smallest σ-algebra of sets containing the open sets.

Cohen constructs the minimal model through a recursive definition based on closing off under the ZF axioms. Proceeding in modern terms, for a set X, let $\Gamma(X)$ denote the collection of $\{x, y\}$ for $x, y \in X$ as well as $\bigcup x$, $\mathcal{P}(x) \cap X$, and $\{z \in X \mid \exists w \in x R(w, z)\}$ whenever $R(\cdot, \cdot)$ is a functional relation defined with quantifiers restricted to X. The latter, of course, arranges for Replacement. Let $T_0 = \omega + 1$, i.e., the set consisting of all the natural numbers and the set of all the natural numbers, and recursively, $T_\alpha = \Gamma(\bigcup_{\beta<\alpha} T_\beta)$. For $M = \bigcup_\alpha T_\alpha$ being a model of ZF Cohen merely

[13]See [19], p. 1090.

[14]See [44], p. 155.

refers to the Gödel monograph. As he pointed out, the only difference between L and his construction is that he does not demand that $X \in \Gamma(X)$—so that Replacement is being addressed while forestalling all the ordinals from getting into M. Cohen then argues that M is minimal and by the Löwenheim-Skolem theorem that M is countable; we would just note today that the closure ordinal γ, i.e., the least α such that $T_{\alpha+1} = T_\alpha$, is countable. In its way, there is an elegance to this construction in its concreteness and simplicity. Cohen here was working in a context where there are standard models of ZF, and though this would become an issue in forcing, he would henceforth work his ideas on the concrete backdrop provided by such models.

§3. **Forcing.** For his frontal assault on independence, Cohen concentrated first on starting with a countable (transitive) standard model M of ZF and adjoining just a set a of integers to get a minimal extension $M(a)$, with a firm decision made not to alter the ordinals. There is a remarkable audacity and hope here in his trying to do something so basic, given the high axiomatic tradition steeped in formal logic. Cohen continued his recollection ([19], p. 1091):

> 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.

By the middle of April 1963, everything came together for Cohen. He engagingly described the moment (p. 1092):

> There are certain 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* [my emphasis]. 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.

More informally, Cohen recalled: "What made it so exciting to me was how ideas which at first seemed merely philosophical could actually be made into precise mathematics. I went up to Berkeley to see Dana Scott and run the proof past him. I was very, very excited."[15] At Stanford Cohen described his argument to Feferman, lectured on it, and still in April, circulated a manuscript entitled "The independence of the Axiom of Choice".[16] In it Cohen presented four results by extending a countable (transitive) standard model of $V = L$:

(1) The consistency of ZFC + GCH + $V \neq L$ by adjoining what is now known as a Cohen real.
(2) The consistency of ZFC + ¬CH by adjoining $\aleph_2$ Cohen reals. He could not conclude at this time that $2^{\aleph_0}$ is exactly $\aleph_2$.
(3) The consistency of ZF + "There is no well-ordering of the reals" by adjoining countably many Cohen reals, the set consisting of them, but no enumeration—"the basic Cohen model".
(4) The consistency of ZF + "there is a countable sequence of pairs of sets of reals without a choice function" by adjoining a sequence $\langle \{A_i, B_i\} \mid i \in \omega \rangle$ where the A_i and B_i each consist of countably many Cohen reals—"the second Cohen model".

The thrust of Cohen's constructions, what gave them a crucial operational clarity, was to start with a (transitive) standard model of ZF and extend it to another without altering the ordinals. Cohen did appreciate that starting with a standard model of ZF is formally more substantial than assuming merely the consistency of ZF, and he indicated, as a separate matter, a syntactic way to pare down his arguments into formal relative consistency statements of the type Con(ZF) → Con(ZF + ¬AC).

The components of Cohen's forcing scheme as he first conceived it for extending a (transitive) standard model M of ZF to a generic extension $M(a)$ were as follows:

(a) In M, a *ramified language* together with *ranked terms* to denote sets in the extension is developed for the purpose of approaching satisfaction in the extension. The ramified language has quantifiers $\forall_\alpha$ and $\exists_\alpha$ indexed by the ordinals of M; at this time Cohen took the terms to

[15]See [1], p. 53. The first sentence here is noteworthy in juxtaposition with the first passage quoted in the previous section which had: "somehow Gödel had mathematicized a philosophical concept, i.e., constructibility, and there seemed no possibility of doing this again ... ".

[16]See [44], p. 156.

be certain F_α's, well-ordered in the style of Gödel's monograph and its eight generators for generating L.

(b) In M, a set of *conditions* each regarded as providing partial information toward an eventual generic object a is devised, ordered according to amount of information.

(c) In M, the *forcing relation* "p forces φ", between conditions p and formulas φ of the ramified language is defined to specify when the information of p secures assertion φ in the extension. At this time Cohen took formulas to be in prenex form and equality to be given by the Axiom of Extensionality.

(d) Assuming M is countable, a *complete sequence* of stronger and stronger conditions $p_0, p_1, p_2, \ldots$ is devised so that every formula or its negation is forced by some member of the sequence, and through this sequence a generic object a is arrived at having the desirable properties to establish independence. This is a Baire category argument carried out outside of M; the set of Cohen reals over M is co-meager.

(e) That the resulting $M(a)$ consisting of the interpretations of the terms as determined by a is a model of ZF is established, based on the complete sequence, the definability of the forcing relation in M, and M being a model of ZF.

The genie was out of the bottle. Despite his relative inexperience Cohen had gone a great distance and squarely addressed big problems of set theory. At this moment of impact his scheme immediately generated excitement and came under considerable scrutiny, with some fitful questions raised a measure of the upending in thinking that was being wrought. In the thick of the to and fro, Cohen gave his second lecture on forcing at Princeton on 3 May,[17] and soon afterward met with Gödel himself. Kreisel had written to Gödel already on 15 April about Cohen's work and encouraged Gödel to see Cohen, and Cohen himself had written him on 24 April.[18]

Cohen in a letter of 9 May to Gödel wrote:

> ... what I am trying to say is that only you, with your pre-eminent position in the field, can give the "stamp of approval" which I would so much desire. I hope very much that you can study the manuscript thoroughly and by next week-end be willing to discuss it in more detail. Perhaps, at that time you would consider communicating to the *Proceedings of the National Academy of Sciences* a short note.

Gödel after a few days did pronounce Cohen's work to be correct and moreover agreed to communicate it to the PNAS. Cohen in the letter had also written: "I feel under a great nervous strain"; Gödel in a letter of 20 June to Cohen on various issues ended with ([31], p. 383):

[17] This lecture was arranged by Nerode, at Cohen's request.

[18] See [44], p. 157 for more details here and on the interactions with Gödel described below.

> I hope you are not under some nervous strain which hampers you in your work. You have just achieved the most important progress in set theory since its axiomatization [so more important than Gödel's own work with L]. So you have every reason to be in high spirits.

Through the summer Cohen worked on his PNAS communication, fielding numerous suggestions from Gödel in correspondence, and it eventually appeared in two installments [11, 13] in the winter of 1963–1964.

§**4. First expansion.** From the beginning the potentialities of forcing were evident, and it began to be applied forthwith and, in the process, reworked for wide applicability, the technique soon to become general method. For Cohen, having taken formulas to be in prenex form the recursive definition of the forcing relation rested on the unraveling of the quantifiers, and the crucial clause was: p forces $\forall_\alpha x\varphi(x)$ *iff* for no q stronger than p does q force $\neg\varphi(t)$ for any term t of rank less than α. In May or June Scott had come up with the forcing symbol $\Vdash$ and had decoupled negation from Cohen's clause with: $p \Vdash \neg\varphi$ *iff* for no q stronger than p does $q \Vdash \varphi$.[19] Thus, the forcing relation could be defined for all formulas, and this clarifies how it is only the negation clause with its interaction of conditions that separates forcing from satisfaction. On the one hand, this negation clause is indicated by Cohen's formulation, but on the other hand, without it the general method cannot be developed nor can forcing arguments be made, as they are today, about assertions at large.

Feferman [26, 25] was the first after Cohen to get results with forcing, these both in set theory and in second-order arithmetic. Feferman had been working on whether Σ^1_1 AC could be shown independent via forcing from Δ^1_1 Comprehension for subsystems of second-order arithmetic, the idea being that the hyperarithmetic sets satisfy the latter and one might forcibly adjoin a counterexample to the former. He could not solve this problem, but in his efforts he came to understand forcing well, and working through May and June he established several results.[20]

Feferman relied on Scott's negation clause and of necessity used, instead of Cohen's Gödelian F_α's, abstraction terms corresponding directly to definitions in the ramified language—another felicitous move for the general reworking of forcing. In set theory he showed that adjoining one Cohen real entails that there is no definable well-ordering of the reals, initiating the use of forcing to study definability in ZFC. He also established a new $\neg$AC

[19]See [44], p. 160. Years later, Scott (in the foreword to [4]) recalled the emergence of the Kripke and Beth models for intuitionistic logic and wrote: " … after Cohen's original announcement, I pointed out the analogy with intuitionistic interpretations, and along these lines Cohen simplified his treatment of negation at my suggestion."

[20]See [44], p. 160. Only many years later did John Steel [60] use forcing to show that Δ^1_1 Comprehension does not imply Σ^1_1 AC; although ramified languages would soon be bypassed in general forcing, Steel's forcing re-established the specific importance of ramification for a careful hierarchical analysis.

consistency, that there is no non-principal ultrafilter over ω, in the forcing extension adjoining countably many Cohen reals but not the set consisting of them—"the Feferman model".

In second-order arithmetic Feferman developed, for Kleene's rendering of the hyperarithmetic sets as the ramified analytic hierarchy, a corresponding ramified language and, forcing with it, established that there are incomparable hyperdegrees. Clifford Spector [59] had first established this by measure-theoretic means. In retrospect, Feferman's argument can be seen as an application of the Baire Category Theorem to establish an outright result. As such, the result amounts to a category-theoretic analogue of Spector's, for it shows that the pairs $\langle a, b\rangle$ of reals that are hyperarithmetically incomparable is co-meager, just as Spector's argument had shown that it has measure one. One significant dividend of the specific definability of the forcing relation is that Feferman's incomparable hyperdegrees can be seen to be recursive in Kleene's $\mathcal{O}$.

With Scott's treatment of negation in hand Feferman also came up with *weak forcing* $p \Vdash^* \varphi$, given by: $p \Vdash \neg\neg\varphi$. $p \Vdash^* \varphi$ *iff* $M(a) \models \varphi$ for any generic a approximated by p, and so $p \Vdash^* \varphi$ and $\vdash \varphi \rightarrow \psi$ implies $p \Vdash^* \psi$. With this closer connection to semantics and deducibility, it would be through the lens of $\Vdash^*$ that forcing would henceforth be viewed.

Robert Solovay would above all epitomize this period of great expansion in set theory, with his mathematical sophistication and fundamental results about and with forcing, in large cardinals, and in descriptive set theory. Following initial graduate study, also at the University of Chicago, in differential topology Solovay focused his energies on set theory after attending Cohen's 3 May Princeton lecture, quickly absorbing forcing and making his first incursions. For his $\neg$CH model resulting from adjoining $\aleph_2$ Cohen reals, Cohen had come to the Delta-system Lemma *ab initio* and through the consequent countable chain condition deduced that cardinals are preserved, but he could not initially conclude that $2^{\aleph_0}$ is exactly $\aleph_2$. In June both Cohen and Solovay independently came to this conclusion[21] by appealing anew to the countable chain condition.

Solovay carried out the first exploration of possible spectra of powers of regular cardinals, introducing closure properties on the set of conditions and establishing the consistency of a finite conjunction of possibilities for the $\aleph_n$'s. Solovay subsequently gave seminar talks on forcing at the Institute for Advanced Study, and there in late Fall 1963 William Easton [23, 24] established his now well-known global result on powers of regular cardinals with class forcing.

Solovay's final advance in 1963 was a matter of technique. In his bootstrapping into a new context Cohen had relied on a version of Gödel's CH

[21] See [44], p. 159, 161–162. Cohen in a letter of 14 June to Gödel so confirmed the size of the continuum and also described how cardinals can be collapsed.

argument for L to establish that the Power Set Axiom holds in generic extensions. Solovay gave the now-standard sort of argument using Replacement, confirming that one does not have to start with a ground model satisfying $V = L$.

Cohen gave his third lecture on forcing on 4 July (Independence Day!) at a conference on model theory held at Berkeley in early summer 1963. Feferman and Solovay also presented their respective results with forcing. Cohen's paper [14] for the proceedings, judging from the results of others mentioned, was presumably written sometime in early 1964. The account, in its time frame, is notable in several respects: Cohen (p. 52) posed two problems: Is there a (necessarily ill-founded) model of set theory with an automorphism σ whose square σ^2 is the identity? Cohen [18] would himself eventually answer this question positively with forcing. Is it consistent to have ZF + Countable AC + "all sets of reals are Lebesgue measurable"? Cohen considered this a "very important problem", one "requiring perhaps some basic elaboration of the ideas of forcing and generic sets." Solovay [53, 55] would soon answer this question positively in 1964, as described below.

Finally, Cohen (pp. 53–54) separately from his argumentation with standard models detailed a specific approach for establishing formal relative consistency. In his PNAS communication [13] Cohen had proceeded semantically with standard models of finitely many ZF axioms in some enumeration, arguing that to get a standard model of the first p ZF axioms together with $2^{\aleph_0} = \aleph_2$ it suffices to start with a standard model of the first $f(p)$ axioms, for an arithmetical function f. Cohen now returned, interestingly, to what he had broached in his April manuscript, a syntactic approach about the forcing relation and logical deduction. He first replaced his standard model with the entire universe and proceeded to describe a series of primitive recursive functions e.g., "There is a primitive recursive function which assigns to the number of each axiom of Z-F a proof in Z-F that all [conditions] P force that axiom." Cohen's analysis exhibited his proof-theoretic experience and anticipated the relative consistency argument via the later Boolean-valued approach to forcing.

Actually, forcing can be carried out over any, not necessarily standard, model—so that formal relative consistency is immediate. Cohen himself would confirm this a decade later in his last paper [18] with mathematical results. The situation there required that he start with an ill-founded model; the "ordinals" remained unaltered in the forcing, but he had to argue separately for Foundation in the extension. Whatever is the case, standard models evidently played a central role in the discovery of forcing, and the simplifications and intuitive underpinnings afforded by them were crucial factors in the development of forcing as a general method.

Azriel Levy, visiting Berkeley, first heard the details of Cohen's results at the model theory conference, and later that summer fully assimilated forcing working with Feferman. In quick succession several abstracts appeared: Levy [39], Feferman–Levy [27], Levy [40, 41], and Halpern-Levy [34]. With this work Levy became one of the first after Cohen to exploit forcing in a sustained fashion to establish a series of significant results. These had to do with further AC independences and the limits of definability in set theory and freely exploited the idea of *collapsing* a cardinal, i.e., adjoining a generic bijection to a smaller ordinal, as first set forth by Cohen.[22]

Feferman–Levy [27] started with a model of ZFC + GCH and collapsed every $\aleph_n$ to $\aleph_0$—the "Feferman-Levy model." The former $\aleph_\omega$ becomes the new $\aleph_1$ so that it is singular, and the reals are a countable union of countable sets so that Countable AC fails in a drastic fashion.

Halpern–Levy [34] represents a line of work in set theory for which Cohen's epochal advance provided at least a semblance of continuity. James Halpern in his 1962 Berkeley dissertation[23] had shown that in a Fraenkel–Mostowski model with urelements the Boolean Prime Ideal Theorem (BPI), that every Boolean algebra has a prime ideal, holds, bringing a Ramsey-type partition theorem into play. Halpern–Levy [34] observed that the Ordering Principle (OP), that every set can be linearly ordered, holds in the basic Cohen model for the failure of AC. BPI was becoming a prominent choice principle, one which implied OP, and Levy saw the need for a strengthened "tree" partition theorem to "transfer" Halpern's result to a ZF forcing result. Halpern and Läuchli [33] duly established this theorem, and with it, Halpern and Levy [35] established that BPI also holds in the basic Cohen model. This Halpern–Läuchli–Levy collaboration was an important step forward in an emerging cottage industry of transferring results from Fraenkel–Mostowski models to ZF consistency results via forcing by correlating urelements with generic sets.

The results of Levy's abstracts [39, 40, 41], as eventually presented in expanded form in [42, 43], had to do with the limits of definability when successively, ordinal parameters and then real parameters are allowed. Extending the initial observation of Feferman [26], Levy [39, 42] showed that in Cohen's first model adjoining a Cohen real to a model of $V = L$, there is no definable well-ordering of the reals even if ground-model parameters are allowed in the definition. For this Levy first exploited the important *homogeneity* of the Cohen forcing conditions. Levy [39, 42] further showed that collapsing $\aleph_1$ entails moreover that every well-ordering of the reals definable with ground-model parameters is countable. Levy [40, 43] next established a delimitative result for descriptive set theory, that if to a model of $V = L$ one adjoins $\aleph_1$ Cohen reals, there is a Π^1_2 relation which cannot be uniformized by any projective relation. The relation is $\langle f, g\rangle \in R$ *iff* $g \notin L[f]$, and in the model it cannot be uniformized by any relation definable allowing ordinal

[22] See [14], p. 51.
[23] See [32].

and real parameters. Finally, Levy [41, 43] extended his results by collapsing every cardinal below an inaccessible cardinal to $\aleph_0$, rendering the cardinal itself the new $\aleph_1$. This entails that in the resulting model every well-ordering of the reals definable with ordinal *and* real parameters is countable, every new real having appeared in an early part of the collapsing. The "Levy collapse", thus devised to get at the limits of definability, would become a basic component of the investigation of large cardinals with forcing.

Still in the summer of 1963, Tennenbaum [61] established the independence of Souslin's Hypothesis from ZFC. This hypothesis is a classical proposition equivalent to the combinatorial assertion that there are no "Souslin trees", and Tennenbaum generically adjoined a Souslin tree. In having addressed a question from 1920, Tennenbaum's result is notable for having been the first after Cohen's for illuminating an outstanding classical problem and without involving Cohen's ways of adjoining reals and collapsing cardinals.

These various results of 1963 amounted to the first cresting of the wave created by Cohen. In the next several years, forcing, confirmed in its potency and applicability, was widely disseminated, leading to a great expansion of set theory as a field of mathematics, and advances with forcing at a higher plane were achieved, in large part by Solovay.

§**5. After.** Forcing, even in its first year, began to be disseminated through seminars and courses across a wide range of universities. Already in 1963, J. Barkley Rosser gave seminar talks on forcing at the University of Wisconsin at Madison, and Chen-Chung Chang and Nerode had a seminar at the Institute for Advanced Study at which Solovay gave talks on forcing. In 1964, Karel Prikry lectured on forcing in a Warsaw seminar and Yiannis Moschovakis at Harvard. Courses were given on forcing by Feferman at Stanford and by Levy at the Hebrew University. And at Berkeley a group of graduate students self-organized into a working seminar, a group including soon-to-be prominent set theorists James Baumgartner, Richard Laver, and William Mitchell.[24]

Cohen himself gave a comprehensive course at Harvard in the Spring of 1965 which resulted in a monograph [15]. This monograph would have served as a fine introduction to mathematical logic at the time, and as such it exhibits Cohen's by then impressive command of the subject.[25] The first third of the monograph is given over to the Gödel Incompleteness Theorems and recursive functions; Cohen's early interest in number theory comes through, and the culminating result presented is what is now known as Tennenbaum's theorem, that no countable non-standard model of Peano Arithmetic can

[24]See [44], pp. 160–162.

[25]Incidentally, among those whom Cohen credited for help in the preparation of the manuscript were David Pincus, Thomas Scanlon, and Jon Barwise, all who would make significant contributions to mathematical logic.

be recursively presented. The middle third of the monograph is an incisive account of axiomatic set theory through Gödel's work on L to the minimal model, an account shedding Gödel's formal style and pivoting to a modern presentation. The final third of the monograph is given over, of course, to forcing and independence results. Cohen incorporated the methodological changes made by others in 1963 and presents, or least cites, most of the results of that year.

On the subject of establishing formal relative consistency in view of his standard model assumption, Cohen outlined both his syntactic approach about how all conditions P force a statement, described in his [14], and the semantic approach about using standard models of only finitely many axioms, described in his [13]. As to the former, he wrote (pp. 147–148):

> Although this point of view may seem like a rather tedious way of avoiding models, it should be mentioned that in our original approach to forcing this syntactical point of view was the dominant point of view, and models were later introduced as they appeared to simplify the exposition. The peculiar role of the countability of M is here entirely avoided.

This stands in contrast to Cohen's early emphasis on the minimal model and his reminiscences ([19], p. 1092) quoted earlier about his discovery of forcing through the contemplation of adding a new set a of integers to a standard model. This tension between syntactic consistency and actual model building would play an important role in Cohen's thinking about mathematics, particularly his coming espousal of formalism while at the same time emphasizing the operational importance of working with mathematical objects. This tension would henceforth remain in set theory once forcing as method became embedded into its fabric.

In his conclusion, Cohen offered the following point of view about CH from an "idealist" perspective (p. 151):

> A point of view which the author feels may eventually come to be accepted is that CH is *obviously* false. The main reason one accepts the Axiom of Infinity is probably that we feel it absurd to think that the process of adding only one set at a time can exhaust the entire universe. Similarly with the higher axioms of infinity. Now $\aleph_1$ is the set of countable ordinals and this is merely a special and the simplest way of generating a higher cardinal. The set C [the continuum] is, in contrast, generated by a totally new and more powerful principle, namely the Power Set Axiom. It is unreasonable to expect that any description of a larger cardinal which attempts to build up that cardinal from ideas deriving from the Replacement Axiom can ever reach C. Thus C is greater than $\aleph_n$, $\aleph_\omega$, $\aleph_\alpha$ where $\alpha = \aleph_\omega$ etc. This point of view regards C is an incredibly rich set given to us by one bold new axiom, which can never be approached by any

> piecemeal process of construction. Perhaps later generations will see the problem more clearly and express themselves more eloquently.

Turning to the new work, the main achievements with forcing in the years immediately after 1963 would be due by Solovay. As we recede from the vortex of Cohen's initial advance, we give only a synopsis of these results, one which however hardly does justice to their full significance for the future development of set theory. Working on that Lebesgue measure problem of Cohen's, Solovay [53, 55] in 1964 established a result remarkable for its early sophistication and revelatory of what standard of argument is possible with forcing: In the extension resulting from the Levy collapse of an inaccessible cardinal, the inner model consisting of sets definable with ordinal and real parameters satisfies: The Principle of Dependent Choices and "all sets of reals are Lebesgue measurable".[26] The proof used Solovay's concept of a *random real* and the homogeneity and universality of the Levy collapse, themes significantly augmenting those from Levy's own work and still relevant today in the investigation of regularity properties of sets of reals.

By June 1965, Solovay and Tennenbaum [57] established the relative consistency of Souslin's Hypothesis. Forcing with a Souslin tree as an ordered set of conditions "kills" the tree, and they systematically killed all Souslin trees in the first genuine argument by iterated forcing.

In 1966 Solovay [54, 56] established the equiconsistency of there being a measurable cardinal and $2^{\aleph_0}$ being real-valued measurable. In the forward direction he adjoined many random reals and applied the Radon-Nikodym theorem of analysis, and in the converse direction he drew out the important concepts of *saturated ideal* and *generic ultrapower*, concepts that would become basic to the integration of forcing and large cardinals.

The development of forcing as method went hand in hand with this procession of central results, even at a basic level before iteration and integration with large cardinals. As early as 1963 Solovay realized that Cohen's framework can encompass arbitrary partial orders with the theory developed in terms of their incompatible members and dense sets. Later, Solovay brought in *generic filters*, a concept accredited to Levy,[27] loosening genericity from having a complete sequence and hence the countability of the ground model. While in Cohen's work sets of conditions were evidently concocted to yield generic objects directly witnessing existence assertions, in Levy's and Solovay's work various sets of conditions and their relation to genericity assumed a separate significance. For this a general formulation in terms of partial orders became desirable and incumbent.

When working on his Lebesgue measurability result, complications in describing when a formula holds over a range of generic extensions led

[26] See [44] for the vicissitudes in the development of Solovay's result.

[27] See [55], n. 5.

Solovay to the idea of assigning a value to a formula from a complete Boolean algebra. He initially assigned to each formula a pair of sets, consisting first of those conditions forcing the formula and second of those conditions forcing its negation. Solovay described this to Scott in September 1965, and Scott pointed out that Solovay's Boolean algebra was essentially the algebra of regular open sets of the underlying topology. Working independently, Solovay and Scott soon developed the idea of recasting forcing entirely in terms of *Boolean-valued models*.[28] This approach showed how Cohen's ramified languages can be replaced by a more direct induction on rank and made evident how a countable standard model was not needed. By establishing in ZFC that e.g., there is a complete Boolean algebra assigning $\neg$CH Boolean value one, a semantic construction was replaced by a syntactic one that directly secured relative consistency.

Scott popularized Boolean-valued models in lectures at a four-week set theory conference held in the summer of 1967 at the University of California at Los Angeles (UCLA). This was by all accounts one of those rare, highly exhilarating conferences featuring groundbreaking papers and lectures that both summarized the progress and focused the energy of a new field opening up, this in large part due to Cohen's work.

At the UCLA conference, Cohen [17] presented his own thoughts on the foundations of set theory. Taking the critical point to be the existence of infinite totalities, Cohen framed the issue in dichotomous terms as a choice between "Realism" and "Formalism". He repeated his [14] contention that from a Realist perspective the size of the continuum may come to be considered very large because of the potency of the power set. On the other hand, he opted for Formalism, a choice which for him carries with it as a heavy weight (p. 13) "the admission that CH, perhaps the first significant question about uncountable sets which can be asked, has no intrinsic meaning." In the subsequent discussion, Cohen did point out how among mathematicians "there is a natural tendency to replace discussion of methods and statements by discussion of suitable abstractions which are considered as 'objects'," and recalled how with the expansion of the concept of function Weierstrass's continuous nowhere differentiable function came to have as legitimate an existence as sin x. There is a quiet resonance here between Cohen's coming down on the Formalist side and his syntactic approach to formal relative consistency, described earlier, in light of his operational use of standard models. At the end Cohen put forward the view that "we do set theory because we have an informal consistency proof for it", and gave a sketch of how one might reduce the complexity of a putative contradiction à la Gentzen in proof theory.

[28] See [44], p. 163. It turned out that Petr Vopěnka [63] leading his Prague seminar had developed a similar approach, though his earlier papers did not have much impact partly because of an involved formalism.

Also at that UCLA conference Joseph Shoenfield [49] gave lucid lectures on "unramified forcing". Shoenfield both advanced the general partial order–generic filter formulation and eliminated the ramified language as in the Boolean-valued approach. Shoenfield had terms for denoting sets in the extension that were remarkably simple and clearly brought out how the only hierarchical dependence can be on the inherent well-foundedness of sets. Boolean-valued models, with their elegant algebraic trappings and seemingly more complete information, had held the promise of being the right approach to independence results. However, the view of forcing as a way of actually extending models with conditions held the reservoir of mathematical sense and the promise of new discovery, and bolstered by Shoenfield's simple formulation set theorists were soon proceeding in terms of partial orders and generic filters.

The theory and method of forcing stabilized by the early 1970s as follows:

(1) Forcing is a matter of partial orders and generic filters much as presented in [49], and as a heritage from Boolean-valued models, **1** denotes weakest condition and the direction $p < q$ is for p having more information than q.[29]

(2) V is typically construed as the ground model; a partial order $P \in V$ as a "notion of forcing" is specified to a purpose; a generic filter $G \notin V$ is posited; and an extension $V[G]$ taken, its properties argued for based on the combinatorial properties of P.

(3) Boolean algebras underscore the setting and are sometimes a necessary augmentation; most importantly, arguments about sub-extensions generated by terms and embeddings of extensions are sometimes best or necessarily cast in terms of Boolean algebras.

Item (2) is both representative and symptomatic of an underlying transformation of attitudes in and about set theory in large part brought on by the advent of forcing. Up to Gödel's work on L and just beyond, the focus of set theory was on V as *the* universe of sets and what sets *are*. Set theory had born a special ontological burden because it is a theory of extensions to which mathematics can be reduced. On the one hand, sets appear to be central to mathematics. On the other hand, there seems to be no strong or evident metaphysical or epistemological basis for sets. This apparent perplexity led to metaphysical recastings of sets and more subtle appropriations like the sole reliance on some prior logical or iterative conception of set. After Gödel, set theory began to shed this ontological burden and the main concerns became what sets *do* and how set theory is to advance as an autonomous field of mathematics. With Cohen there was an infusion of mathematical thinking and of method and a proliferation of models, much as in other modern, sophisticated fields of mathematics. Taking V as the

[29] There is a persistent Israeli revisionism in the other direction, following Saharon Shelah.

ground model goes against the sense of V as the universe of all sets and "Tarski's undefinability of truth", but actually V has become a *schematic* letter for a ground model. This further drew out that in set theory as well as in mathematics generally, it is a matter of method, not ontology.

Forcing has been interestingly adapted in a category theory context which is a casting of set theory in intuitionistic logic. The basic discovery, jointly due to William Lawvere and Myles Tierney, was that forcing can be interpreted as the construction of a certain topos of sheaves. The internal logic of the topos of presheaves over a partially ordered set is essentially Cohen's forcing, while passing to the subtopos of sheaves of the double-negation Grothendieck topology gives weak forcing. In the first, 1970 paper on elementary topoi Lawvere [37] gave a brief indication of how Cohen's independence of CH would look from a topos point of view, and then Tierney [62] provided the details. Later in 1980 Peter Freyd [28] gave a direct topos-theoretic proof of the independence of AC. Although his models could have been obtained by standard set-theoretic methods, they look simpler from the topos point of view than from the set-theoretic point of view. The details of correlation were worked by Solovay in unpublished notes, and by Andreas Blass and Andre Scedrov [5].[30]

In subsequent years Cohen would publish only five more papers. The first [16] was on decision procedures; the second [18] was a throwback on forcing; written many years later, the third [19] was an extended reminiscence of the discovery of forcing to which we have already referred; the fourth [20] was a paper on Skolem in which he articulated his views on the limits of proof; and the last [21] was a reminiscence of his career and interactions with Gödel, covering in summary terms ground covered elsewhere, for a conference on the centenary of Gödel's birth. We deal successively with [16, 18, 20].

In work that came full circle back to his budding interest in decision procedures in his graduate days at Chicago, Cohen [16] developed a concrete decision procedure for the p-adic fields. In well-known work of the mid-1960s, James Ax and Simon Kochen [2] had used ultraproducts to get a complete, recursive set of axioms for p-adic number theory and therewith a decision procedure. Cohen proceeded more directly as in the Tarski decision procedure for real-closed fields to carry out an elimination of quantifiers. With his constructive approach, Cohen was able to get a primitive recursive decision procedure as well to easily isolate the properties of fields with valuations which are being used.

In [18] Cohen answered a question that he had himself posed in [14] by getting a model of ZF with an automorphism σ whose square σ^2 is the identity. Such a σ would have to fix the ordinals, and so would be the identity if AC were to hold—since models of AC are determined by their sets of ordinals. Hence, this was a new way of getting $\neg$AC. Also, no

[30]My thanks to Andreas Blass for all the particulars of this paragraph.

model of ZF with an automorphism can be well-founded. As a variation on method, Cohen was able to start with a ill-founded countable model of ZF and force the desired result in an extension without altering the "ordinals". To get a σ as desired, he introduced certain generic sets along rank levels and constructed σ generically through the use of a particular complete sequence. As mentioned earlier, this confirmed as a methodological point that forcing can be carried out on ill-founded models.

In [20], a paper given at a conference on mathematical proof at the British Royal Society, Cohen discussed Thoralf Skolem, the limits of proof and formalization, and "the ultimate pessimism deriving from Skolem's views". In the process, Cohen brought in his own experiences and approaches and summed up his own thinking. In Skolem Cohen evidently saw a kindred spirit, one whose mathematical work and conclusions in an earlier time he viewed his own as complementing and extending. The work was of course the Lowenheim-Skolem theorem leading to the Skolem Paradox, from which Skolem drew that conclusion that first-order axiomatization in terms of sets cannot be a satisfactory foundation for mathematics. This "pessimism" Cohen will extend, to assert (p. 2408) that "it is unreasonable to expect that any reasoning of the type we call rigorous mathematics can hope to resolve all but the tiniest fraction of possible mathematical questions."

Cohen proceeded to give his own gloss on developments from Frege through Hilbert to Gödel. Coming to Skolem's infusion of witnessing constants, Cohen wrote (p. 2411):

> The fundamental discovery of Lowenheim-Skolem, which is undoubtedly the greatest discovery in pure logic, is that the invention (or introduction) of 'constants' as in predicate calculus, is equivalent to the construction of a 'model' for which the statements hold.

This of course has an underlying resonance with forcing. Cohen pointed out how "Skolem's work received amazingly little attention", and went to accord Skolem a high place in a salient passage that brings out Cohen's own standpoint (p. 2411):

> Skolem wrote in a beautiful, intuitive style, totally precise, yet more in the spirit of the rest of mathematics, unlike the fantastically pedantic style of Russell and Whitehead. Thus, Hilbert even posed as a problem the very result that Skolem had proved, and even Gödel, in his thesis where he proved what is known as the Completeness Theorem, does not seem to have appreciated what Skolem had done, although in a footnote he does acknowledge that 'an analogous procedure was used by Skolem'. A possible explanation lies in the fact that Skolem emphasized models, and was amazingly prescient in some of his remarks concerning independence proofs in set theory. A discussion of the priority question can be found in the notes to Gödel's Collected Works (Gödel 1986). Gödel was undoubtedly

> sincere in his belief that his proof was in some sense new, and in view of his monumental contributions I in no way wish to find fault with his account. What is interesting is how the more philosophical orientation of logicians of the time, even the great Hilbert, distorted their view of the field and its results.

Earlier, Cohen had quoted the well-known passage from Skolem [50], p. 229 in which Skolem considered adjoining a set of natural numbers to a model of set theory in connection with the continuum problem.

On constructivity, Cohen notably gave as what he believed to be "the first example of a truly non-constructive proof in number theory" the Thue-Siegel-Roth result that he had been working on in his first graduate days at Chicago.

Discussing consistency questions, Cohen sketched the idea of Gentzen's consistency proof for Peano Arithmetic "in my own version which I intend to publish some day." (p. 2414) Cohen had a few years earlier given talks, and even circulated a manuscript, about this version at Stanford and Berkeley.

Getting to "the ultimate frontier", set theory, Cohen more or less reworks his earlier [17] remarks on the foundations of set theory, mentioning (p. 2416) that "Through the years I have sided more firmly with the formalist position." Despite this, Cohen arrived at the ultimate pessimism via a basic ambivalence (p. 2417):

> ... Even if the formalist position is adopted, in actual thinking about mathematics one can have no intuition unless one assumes that models exist and that the structures are real.
>
> So, let me say that I will ascribe to Skolem a view, not explicitly stated by him, that there is a reality to mathematics, but axioms cannot describe it. Indeed one goes further and says that there is no reason to think that any axiom system can adequately describe it.

Cohen then returned to the bedrock of number theory and gave as an example the twin primes conjecture as beyond the reach of proof. "Is it not very likely that, simply as a random set of numbers, the primes do satisfy the hypothesis, but there is no logical law that implies this?"[31] How about higher axioms of infinity resolving more and more arithmetical statements? "There is no intuition as to why the consideration of the higher infinite should brings us closer to solving questions about primes." Cohen speculated whether statistical evidence will someday count as proof, and ended:

> In this pessimistic spirit, I may conclude by asking if we are witnessing the end of the era of pure proof, begun so gloriously by the

[31] See [20], p. 2418. On the other hand, Chen Jingrun [6] in 1966 proved, toward Goldbach's conjecture, that every sufficiently large even number is the sum of two primes or the sum of a prime and a *semiprime*, i.e., a product of two primes. In the process he proved, toward the twin primes conjecture, that there are infinitely many primes p such that $p + 2$ is either a prime or a semiprime.

Greeks. I hope that mathematics lives for a very long time, and that we do not reach that dead end for many generations to come.

§6. **Envoi.** Within a decade after Cohen's discovery forcing became a systematic part of the burgeoning field of modern set theory. That generic extensions have the same ordinals and satisfy ZF and the basic connection between the ground model and extension through the forcing relation are now simply taken for granted as part of the underlying theory. Rather, the focus is on the connections between the combinatorial properties of the partial order of conditions and structural properties of the extension. Through a natural progression of mathematical development there has evolved a vast range of different notions of forcing as themselves paradigms of construction, a vast technology for iterated forcing, and a vast web of interactions among forcing properties and central propositions of set theory.

Forcing has thus come to play a crucial role in the transformation of set theory into a modern, sophisticated field of mathematics, one tremendously successful in the investigations of the continuum, transfinite combinatorics, and strong propositions and their consistency strength. In all these directions forcing became integral to the investigation and became part of their very sense, to the extent that issues about the method became central and postulations in its terms, "forcing axioms", became pivotal.

Cohen would not be party to any of these further developments. It could be said that he was in the end a problem solver rather than a system builder. Whatever is the case, Cohen seemed to evince little interest in the many new models of set theory and the elaboration of forcing as method and returned to the bedrock of number theory with a specifically formalist attitude toward mathematics.

With forcing so expanded into the interstices of set theory and the method so extensively amended from the beginning, what is the "it" of Cohen's forcing and his individual achievement? Cohen discovered a concrete and widely applicable means of operationally extending a standard model of set theory to another without altering the ordinals. The central technical innovation was the definable forcing relation, through which satisfaction for the extension could be approached in the ground model. Cohen's achievement was thus to be able to secure properties of new sets without having all of their members in hand and more broadly, to separate and then interweave truth and existence.

How singular a phenomenon is Cohen's forcing? On the precursory side, there were the Beth and Kripke semantics for modal and intuitionistic logic[32] in which satisfaction is not fixed in just a one model. But here, there is no thought of securing the *existence* of a new model. More pointedly, there were constructions in recursion theory, like the Kleene-Post argument for getting

[32]See Dummett [22].

incomparable Turing degrees and particularly Spector's two-quantifier argument[33] for getting minimal Turing degrees. But here, there is no thought of securing *truth* for a full model.

Of other roads to independence not taken, one might have thought at the time of appealing to an omitting-types theorem for first-order logic. But how does one ensure the second-order property of well-foundedness, without which other problems ensue? With later work in infinitary logic one can secure well-founded extensions. But how does one secure the Power Set Axiom?

The force of forcing is that, while appearing dramatically on the mathematical stage, it is a basically simple, though remarkable powerful, method. In reminiscences Cohen wrote[34]

> ... it's somewhat curious that in a certain sense the continuum hypothesis and the axiom of choice are not really difficult problems—they don't involve technical complexity; nevertheless, at the time they were considered difficult. One might say in a humorous way that the attitude toward my proof was as follows. When it was first presented, some people thought it was wrong. Then it was thought to be extremely complicated. Then it was thought to be easy. But of course it *is* easy in the sense that there is a clear philosophical idea. There were technical points, you know, which bothered me, but basically it was not really an enormously involved combinatorial problem; it was a philosophical idea.

Without bearing the historical weight of an earlier turn to logic in set theory and proceeding from "ordinary mathematics" to algebraitize truth and existence together, Cohen was able to cut through to a construction that actuates a *new way of thinking*. In Kantian terms, Cohen provided an *organon*, an instrument for the generation of new knowledge.

REFERENCES

[1] Donald J. Albers, Gerald L. Alexanderson, and Constance Reid (editors), ***More mathematical people***, Harcourt Brace Jovanovich, Boston, 1990.

[2] James Ax and Simon Kochen, *Diophantine problems over local fields II. A complete set of axioms for p-adic number theory*, ***American Journal of Mathematics***, (1965), pp. 631–648.

[3] Stefan Banach, *Über die Barie'sche Kategorie gewisser Funktionenmengen*, ***Studia Mathematica***, vol. 3 (1931), p. 174.

[4] John L. Bell, ***Boolean-valued models and independence proofs in set theory***, Oxford Logic Guides, Clarendon Press, Oxford, 1977, second ed., 1985; third ed., 2005.

[5] Andreas R. Blass and Andre Scedrov, *Freyd's models for the independence of the axiom of choice*, ***Memoirs of the American Mathematical Society***, vol. 79 (1989), no. 404, p. 134 pp.

[33] See [58]; the argument is cast as a forcing argument in Lerman [38].

[34] See [1], p. 58.

[6] Jingrun Chen, *On the presentation of a large even number as the sum of a prime and the product of at most two primes*, ***Kexue Tongbao*** (***Chinese Science Bulletin***), vol. 17 (1966), pp. 385–388, in Chinese.

[7] Paul J. Cohen, *On Green's theorem*, ***Proceedings of the American Mathematical Society***, vol. 10 (1959), pp. 109–112.

[8] ———, *On a conjecture of Littlewood and idempotent measures*, ***American Journal of Mathematics***, vol. 82 (1960), pp. 191–212.

[9] ———, *A note on constructive methods in Banach algebras*, ***Proceedings of the American Mathematical Society***, vol. 12 (1961), pp. 159–163.

[10] ———, *Idempotent measures and homomorphisms of group algebras*, ***Proceedings of the International Congress of Mathematicians*** (***Stockholm,*** 1962), Institut Mittag-Leffler, Djursholm, 1963, pp. 331–336.

[11] ———, *The independence of the continuum hypothesis I*, ***Proceedings of the National Academy of Sciences of the United States of America***, vol. 50 (1963), pp. 1143–1148.

[12] ———, *A minimal model for set theory*, ***Bulletin of the American Mathematical Society***, vol. 69 (1963), pp. 537–540.

[13] ———, *The independence of the continuum hypothesis II*, ***Proceedings of the National Academy of Sciences of the United States of America***, vol. 51 (1964), pp. 105–110.

[14] ———, *Independence results in set theory*, ***The theory of models, Proceedings of the 1963 international symposium at Berkeley*** (John W. Addison Jr., Leon Henkin, and Alfred Tarski, editors), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1965, pp. 39–54.

[15] ———, ***Set theory and the continuum hypothesis***, W. A. Benjamin, New York, 1966.

[16] ———, *Decision procedures for real and p-adic fields*, ***Communications on Pure and Applied Mathematics***, (1969), pp. 131–151.

[17] ———, *Comments on the foundations of set theory*, ***Axiomatic set theory*** (Dana S. Scott, editor), Proceedings of Symposia in Pure Mathematics, vol. 13, American Mathematical Society, Providence, 1971, pp. 9–15.

[18] ———, *Automorphisms of set theory*, ***Proceedings of the Tarski symposium***, Proceedings of Symposia in Pure Mathematics, vol. 25, American Mathematical Society, Providence, 1974, pp. 325–330.

[19] ———, *The discovery of forcing*, ***Rocky Mountain Journal of Mathematics***, vol. 32 (2002), no. 4, pp. 1071–1100.

[20] ———, *Skolem and pessimism about proof in mathematics*, ***Philosophical Transactions of The Royal Society***, vol. 363 (2005), pp. 2407–2418.

[21] ———, *My interaction with Kurt Gödel*: *The man and his work*, ***Horizons of truth***, Cambridge University Press, Cambridge, 2009, in honor of the 100th annniversay year of Kurt Gödel (2006).

[22] Michael Dummett, ***Elements of intuitionism***, Oxford Logic Guides, vol. 2, Clarendon Press, Oxford, 1977.

[23] William B. Easton, *Proper class of generic sets*, ***Notices of the American Mathematical Society***, vol. 11 (1964), p. 205, abstract.

[24] ———, *Powers of regular cardinals*, ***Annals of Mathematical Logic***, vol. 1 (1970), pp. 139–178, abridged version of Ph.D. thesis, Princeton University 1964.

[25] Solomon Feferman, *Some applications of the notions of forcing and generic sets*, ***Fundamenta Mathematicae***, vol. 56 (1965), pp. 325–345.

[26] ———, *Some applications of the notions of forcing and generic sets* (*summary*), ***The theory of models, Proceedings of the 1963 international symposium at Berkeley*** (John W. Addison Jr., Leon Henkin, and Alfred Tarski, editors), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1965, pp. 89–95.

[27] Solomon Feferman and Azriel Levy, *Independence results in set theory by Cohen's method II*, ***Notices of the American Mathematical Society***, vol. 10 (1963), p. 593, abstract.

[28] Peter J. Freyd, *The axiom of choice*, ***Journal of Pure and Applied Algebra***, vol. 19 (1980), pp. 103–125.

[29] Kurt F. Gödel, *Consistency proof for the generalized continuum hypothesis*, ***Proceedings of the National Academy of Sciences of the United States of America***, vol. 25 (1939), pp. 220–224.

[30] ———, ***The consistency of the axiom of choice and of the generalized continuum hypothesis with the axioms of set theory***, Annals of Mathematics Studies, no. 3, Princeton University Press, Princeton, 1940.

[31] ———, *Collected works, volume IV*: *Correspondence A–G*, (Solomon Feferman and John W. Dawson Jr., editors), Clarendon Press, Oxford, 2003.

[32] James D. Halpern, *The independence of the axiom of choice from the Boolean prime ideal theorem*, ***Fundamenta Mathematicae***, vol. 55 (1964), pp. 57–66.

[33] James D. Halpern and Hans Läuchli, *A partition theorem*, ***Transactions of the American Mathematical Society***, vol. 124 (1966), pp. 360–367.

[34] James D. Halpern and Azriel Levy, *The ordering theorem does not imply the axiom of choice*, ***Notices of the American Mathematical Society***, vol. 11 (1964), p. 56, abstract.

[35] ———, *The Boolean prime ideal theorem does not imply the axiom of choice*, ***Axiomatic set theory*** (Dana S. Scott, editor), Proceedings of Symposia in Pure Mathematics, vol. 13, American Mathematical Society, Providence, 1971, pp. 83–134.

[36] Alexander S. Kechris and Alain Louveau, ***Descriptive set theory and the structure of sets of uniqueness***, London Mathematical Society Lecture Note Series, vol. 128, Cambridge University Press, Cambridge, 1987.

[37] F. William Lawvere, *Quantifiers and sheaves*, ***Actes du Congrès International des Mathématiciens*** (***Nice*** 1970), vol. 1, Gauthier-Villars, Paris, 1971, pp. 329–334.

[38] Manuel Lerman, ***Degrees of unsolvability***, Perspectives in Mathematical Logic, Springer-Verlag, Berlin, 1983.

[39] Azriel Levy, *Independence results in set theory by Cohen's method I*, ***Notices of the American Mathematical Society***, vol. 10 (1963), pp. 592–593, abstract.

[40] ———, *Independence results in set theory by Cohen's method III*, ***Notices of the American Mathematical Society***, vol. 10 (1963), p. 593, abstract.

[41] ———, *Independence results in set theory by Cohen's method IV*, ***Notices of the American Mathematical Society***, vol. 10 (1963), p. 593, abstract.

[42] ———, *Definability in axiomatic set theory I*, ***Logic, methodology and philosophy of science. Proceedings of the*** 1964 ***international congress at Jerusalem*** (Yehoshua Bar-Hillel, editor), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1965, pp. 127–151.

[43] ———, *Definability in set theory II*, ***Mathematical logic and foundations of set theory. Proceedings of an international colloquium*** (Yehoshua Bar-Hillel, editor), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1970, pp. 129–145.

[44] Gregory H. Moore, *The origins of forcing*, ***Logic Colloquium '86*** (Frank R. Drake and John K. Truss, editors), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1988, pp. 143–173.

[45] Sylvia Nasar, ***A beautiful mind***, Simon and Schuster, New York, 1998.

[46] John C. Oxtoby, ***Measure and category, a survey of the analogies between topological and measure spaces***, Graduate Texts in Mathematics, vol. 2, Springer-Verlag, New York, 1971.

[47] Dana S. Scott, *Measurable cardinals and constructible sets*, ***Bulletin de l'Académie Polonaise des Sciences, Série des Sciences Mathématiques, Astronomiques et Physiques***, vol. 9 (1961), pp. 521–524.

[48] John C. Shepherdson, *Inner models of set theory—part III*, ***The Journal of Symbolic Logic***, vol. 18 (1953), pp. 145–167.

[49] Joseph R. Shoenfield, *Unramified forcing*, ***Axiomatic set theory*** (Dana S. Scott, editor), Proceedings of Symposia in Pure Mathematics, vol. 13, American Mathematical Society, Providence, 1971, pp. 357–381.

[50] Thoralf Skolem, *Einige Bemerkungen zur axiomatischen Begründung der Mengenlehre*, ***Matematikerkongressen i Helsingfors den 4–7 juli 1922, Den femte skandinaviska matematikerkongressen, Redogörelse***, Akademiska-Bokhandeln, Helsinki, 1923, pp. 217–232.

[51] Stephen Smale, *Differentiable dynamical systems*, ***Bulletin of the American Mathematical Society***, vol. 73 (1967), pp. 747–817.

[52] Robert M. Solovay, $2^{\aleph_0}$ *can be anything it ought to be*, ***The theory of models, Proceedings of the 1963 international symposium at Berkeley*** (John W. Addison Jr., Leon Henkin, and Alfred Tarski, editors), Studies in Logic and the Foundations of Mathematics, North-Holland, Amsterdam, 1965, abstract, p. 435.

[53] ———, *The measure problem*, ***Notices of the American Mathematical Society***, vol. 12 (1965), p. 217, abstract.

[54] ———, *Real-valued measurable cardinals*, ***Notices of the American Mathematical Society***, vol. 13 (1966), p. 721.

[55] ———, *A model of set theory in which every set of reals is Lebesgue measurable*, ***Annals of Mathematics***, vol. 92 (1970), pp. 1–56.

[56] ———, *Real-valued measurable cardinals*, ***Axiomatic set theory*** (Dana S. Scott, editor), Proceedings of Symposia in Pure Mathematics, vol. 13, American Mathematical Society, Providence, 1971, pp. 397–428.

[57] Robert M. Solovay and Stanley Tennenbaum, *Iterated Cohen extensions and Souslin's problem*, ***Annals of Mathematics***, vol. 94 (1971), pp. 201–245.

[58] Clifford Spector, *On degrees of recursive unsolvability*, ***Annals of Mathematics***, vol. 64 (1956), pp. 581–592.

[59] ———, *Measure-theoretic construction of incomparable hyperdegrees*, ***The Journal of Symbolic Logic***, vol. 23 (1958), pp. 280–288.

[60] John R. Steel, *Forcing with tagged trees*, ***Annals of Mathematical Logic***, vol. 15 (1978), pp. 55–74.

[61] Stanley Tennenbaum, *Souslin's problem*, ***Proceedings of the National Academy of Sciences of the United States of America***, vol. 59 (1968), pp. 60–63.

[62] Myles Tierney, *Sheaf theory and the continuum hypothesis*, ***Toposes, algebraic geometry and logic***, Lecture Notes in Mathematics, vol. 274, Springer, Berlin, 1972, pp. 13–42.

[63] Petr Vopěnka, *The general theory of* ∇*-models*, ***Commentationes Mathematicae Universitatis Carolinae***, vol. 8 (1967), pp. 145–170.

[64] Benjamin H. Yandell, ***The honors class. Hilbert's problems and their solutions***, A K Peters, Natick, 2002.

DEPARTMENT OF MATHEMATICS
BOSTON UNIVERSITY
111 CUMMINGTON STREET
BOSTON, MASSACHUSETTS 02215, USA
E-mail: aki@math.bu.edu