Daniel Gorenstein received his Ph.D. in algebraic geometry as a student of Oscar Zariski at Harvard University in 1950. By the late 1950s, his research interests had shifted to finite group theory, and over the next thirty years he made extensive contributions to the classification of the finite simple groups. In 1987 he was elected to the National Academy of Sciences and to the American Academy of Arts and Sciences. He is currently a professor at Rutgers University.

The Classification of the Finite Simple Groups A Personal Journey: The Early Years

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Dedicated to Yitz Herstein:
Dear friend, whose zest for life will be long remembered.

The first time I ever heard the idea of classifying finite simple groups was in the mid 1950s in the parking lot of the Air Force Cambridge Research Center at Hanscom Field in Bedford, Massachusetts. After work one day, Seymour Hayden, a member of a group of mathematicians with whom I was a consultant on the design and analysis of cryptographic systems, was describing the thesis problem that Richard Brauer had given him: Show that a simple group having the group $GL_2(q)$ as centralizer of one of its involutions (elements of order 2), q a suitable odd prime power, must necessarily be isomorphic to $PSL_3(q)$, the 3-dimensional projective linear group over the Galois field of q elements. As an algebraist, the problem had a natural appeal to me, but it was not until a good many years later, after I myself had begun working on simple groups, that I came to realize the pioneering depth and originality of Brauer's perspective, which was no less than to mount an inductive attack on the classification of the finite simple groups by means of the structure of the centralizers of their involutions.

At the time I was still ostensibly an algebraic geometer, having obtained my doctorate with Oscar Zariski at Harvard in 1950. Zariski's deep insight into the algebraic nature of the singularities of plane curves had led to his recognition of the significance of a freeness property of their adjoint curves,

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and I had implemented his ideas in my thesis. This freeness condition was later to provide the basis for the much more general notion of a *Gorenstein* ring, but I always felt that *Zariski* ring would have been a more appropriate designation.

In the summer of 1957 Horst Feistel's group of cryptanalysts at AFCRC sponsored a research project on classified and unclassified cryptanalytic problems at Bowdoin College with A. A. Albert as director. Albert, who had a longstanding interest in cryptanalysis and was also a consultant to Feistel's group, invited a distinguished group of university algebraists to participate, including I. N. "Yitz" Herstein, Irving Kaplansky, Irwin Kleinfeld, Richard Schafer, and George Seligman. In preparation for the project, Feistel's group prepared a long list of classified and unclassified problems. Although many of these were of a field-theoretic or combinatorial character, under Sy Hayden's influence they included a number of purely group-theoretic questions related to a type of cryptographic system then under investigation.

Consider a vector space V over $GF(2)$ of large dimension n (say $n \geq 64$), and suppose one is given two machine-constructible one-one transformations X, Y of V onto itself. One can design an enciphering system by forming a power product

$$F = X^{i_1} Y^{j_1} X^{i_2} Y^{j_2} \dots X^{i_r} Y^{j_r}.$$

For each vector v in V, the image $F(v)$ of v under the transformation F is viewed as the "encipherment" of the "message" v, with the exponents $i_1, j_1, i_2, j_2, \dots, i_r, j_r$ considered to be the unknown "key" to the code. To "crack" the code, one is required to determine the values of these exponents on the basis of a knowledge of X, Y, and the image of F on a large percentage (even on all) the vectors of V.

Clearly the structure of the group G generated by X and Y should have some bearing on cryptanalytic properties of the given code. Hayden suggested some degeneracy problems: How secure is the code if it should happen that every element of G has the form $X^i Y^j$ or, more generally, $X^i Y^j X^k$? What is the structure of the group G in these cases? Setting $A = \langle X \rangle$ and $B = \langle Y \rangle$, G is then an example of an *AB-group* or an *ABA-group* — i.e., a group G containing proper subgroups A, B such that every element g in G has the form $g = ab$ or aba', respectively, where $a, a' \in A$ and $b \in B$. In the cryptanalytic context, A and B were, of course, cyclic.

AB-groups had been studied by many authors, but at that time no one had ever considered the structure of *ABA*-groups. That summer Herstein and Kaplansky examined the case in which A and B have the same prime order p and showed that such an *ABA*-group is necessarily solvable. I decided to investigate a case at the other end of the spectrum: A, B arbitrary, but every element $g \in G - A$ has a *unique* representation of the form aba', $a, a' \in A, b \in B^{\#}$. This turned out to be a very serendipitous choice, for I was immediately plunged into the center of finite group theory. Indeed, this uniqueness assumption directly implies that A is its own normalizer in G and A meets each of its distinct G-conjugates in the identity. It then follows from a well-known theorem of Frobenius that A possesses a normal complement T in G with the elements of $A^{\#}$ fixing only the identity of element of T in their action on T by conjugation.

Thus here I was confronted with a special case of the Frobenius conjecture from the turn of the century: A finite group admitting a fixed-point-free automorphism of prime period is necessarily nilpotent. John Thompson's celebrated doctoral thesis in which he verified the Frobenius conjecture in full generality was still a few years away. However, in the situation under investigation the uniqueness assumption gives the following precise expression for the order of G:

$$|G| = |A|(1 + |A|(|B| - 1)).$$

But by LaGrange's theorem, the subgroup B must have order dividing $|G|$. I was able to exploit this divisibility criterion to give an essentially arithmetic proof that T was, in fact, an elementary abelian p-group for some prime p. This special case of the Frobenius conjecture was to become my first paper on finite groups.

Directly after the Bowdoin project, I had a second piece of good fortune. I had made arrangements to spend the academic year 1957–1958 at Cornell University with Shreeram Abhyankar in the hope of getting my stagnant career in algebraic geometry off the ground. Unsuccessful efforts to extend my thesis to algebraic surfaces in 3-space had taught me only that singularities of algebraic surfaces were far more complicated than those of curves; and I had decided that I needed algebraic criteria for a curve in 3-space (or more generally in n-space) to satisfy the "adjoint condition $\nu = 2\delta$". This was the problem I hoped to settle that year. But over the summer Abhyankar had a serious automobile accident and did not arrive at Cornell until late in the fall semester, by which time I had become thoroughly immersed in the structure of ABA-groups.

By coincidence, Herstein had just joined the Cornell faculty, and Yitz and I had become close friends over the preceding summer. Beginning that September we decided to investigate ABA-groups in which A and B were each cyclic, but of relatively prime order, and conjectured that such a group was necessarily solvable. The kind of arithmetic arguments that had sufficed in the prior uniqueness case were no longer applicable; proof of solvability required a deeper knowledge of finite group theory. Herstein was forced to give me a cram course that fall at the blackboard. These discussions often became quite heated, for they were always confined to topics we thought would be relevant to the problem at hand. I recall one day Harold Widom coming in from the next office and suggesting we keep the conversation a little less informal since he had a student in his office.

By the time Yitz and I had established the conjecture, I began to view myself as a finite group theorist rather than an algebraic geometer. It was difficult to quarrel with success: Algebraic geometry seemed to require a broader mathematical background than I was ever able to assimilate, but group theory was a more self-contained subject in which I could move around more freely. Abhyankar was not happy with this turn of events. If I insisted on studying groups, I could at least choose an interesting problem such as the structure of the group of Cremona transformations of the plane! But by then I wasn't listening.

My friendship with Walter Feit also dates from that year at Cornell. He had just returned to the faculty after a stint in the service. His seemingly unbounded enthusiasm for finite group theory attracted me to him, especially as Herstein considered group theory only a brief diversion, soon returning to his deeper commitment to ring theory. Not that Feit and I were able to discuss mathematics so easily, for he was a product of Brauer's character-theoretic tradition, while the little group theory I then knew was limited to the basic material of Zassenhaus's book. Then, too, Walter could be outspoken in his criticism of results he felt to be of insufficient significance.

The following year John Thompson came onto the scene with his remarkable proof of the Frobenius conjecture, foreshadowing the powerful technique of local group-theoretic analysis that he was soon to develop. The result even made the New York Times. The accompanying fanfare was fully justified, for Thompson's argument was quite spectacular: No one had ever subjected the subgroup structure of a group to such complex, almost wild dissection.

The intensity with which Thompson approached mathematics was awe-inspiring. Beyond his sheer brilliance, it was this depth of concentration that helps explain how he was able to produce the intricate Frobenius argument *ab initio.* An episode that captures this quality remains with me. My friendship with Thompson began when he was giving a series of lectures at Cornell on his thesis and we were both staying at Herstein's apartment. Yitz and I had been thinking about the smallest nonprime "Frobenius problem" — finite groups admitting a fixed-point-free automorphism α of period 4. It was easy to construct nonnilpotent examples, and we conjectured that such a group G had to be solvable. This was prior to the solvability of groups of odd order (since each orbit of α on $G^{\#}$ has length 4, G is necessarily of odd order, but a direct proof would have been of interest in any case).

The problem had an obvious appeal to Thompson and he became engrossed in it one evening. I soon went off to bed. When I awoke at 5:30 and went into the living room, to my astonishment there was John still sitting, totally absorbed, the floor scattered with papers. He hadn't quite settled the conjecture, but it was evident that he had seen more deeply into the order 4 problem in one night than had Yitz and I over an extended period.

I should at least add that Herstein and I did succeed in proving the solvability of G one summer a few years later at a Stanford University sequel to the Bowdoin cryptanalysis project. The proof came to us in only a few minutes, almost as a fluke: By some miraculous algebraic juggling, for each pair of primes p, q dividing $|G|$, we were able to force the unique α-invariant Sylow p- and q-subgroups of G to be permutable with each other. The solvability of G then followed directly from Philip Hall's well-known Sylow characterization of solvable groups. One of Thompson's results did survive, however, for inclusion in our resulting paper. Graham Higman had shown that the nilpotency class of a p-group admitting a Frobenius automorphism of prime period r is *bounded* as a function of r. But Thompson had produced p-groups of arbitrarily high class admitting such an automorphism of period 4.

It was also that summer at Stanford that Dan Hughes pointed out to me that every doubly transitive permutation group is an ABA-group, with A a one-point stabilizer and B of order 2. It was not until some time later that I learned that every group G of Lie type is an ABA-group with A a Borel subgroup of G and B a subgroup of G such that $A \cap B$ is normal in B with $B/A \cap B = W$, the Weyl group of G (the doubly transitive case corresponding to groups of Lie rank 1). Undoubtedly it was this implicit connection between ABA-groups and important families of finite groups that explains why their structure had afforded me such a rich introduction into finite group theory.

By 1960, I was ready to move on from ABA-groups. Albert was organizing a group theory year at the University of Chicago and had invited Brauer, Graham Higman, Noboru Ito, Michio Suzuki, G. E. "Tim" Wall, along with a number of younger group-theorists: Norman Blackburn, Feit, and John Walter. Thompson was an instructor at Chicago that year and Jonathan Alperin was there as a graduate student. I made arrangements to spend my sabbatical from Clark University at Chicago, yet another fortunate decision. For it was during that year that Feit and Thompson broke open the odd order theorem and I was able to watch large portions of it unravel as Thompson sketched his arguments on the blackboard. Little did I realize that I would soon be applying them in my own research!

I was living next door to Suzuki in the "compound," a rather old University of Chicago apartment building that housed, among others, almost all the group theory year visitors. Suzuki was then in the midst of his fundamental centralizer of involution characterizations of groups of Lie type of characteristic 2. In his recent classification of simple groups in which the centralizer of every involution is assumed to be a 2-group he had been led to the discovery of the family of doubly transitive simple groups $Sz(2^n)$ that bears his name (within a few months it was realized by several mathematicians that Suzuki's groups were, in fact, related to the family $B_2(2^n)$ of groups of Lie type). Suzuki was not then fluent enough in English to engage in the kind of back-and-forth discussions from which I learned best. Perhaps this also explains his hesitancy to discuss his certainly outstanding research while it was in progress. I knew this was my loss, but although we became good friends, I was never able to get a feeling for Suzuki's approach to mathematics through conversation, only from his published papers.

On the other hand, I did manage to learn a bit of character theory that year from Feit, who was then further developing his ideas about isometries and coherence of character rings. I was so struck by the power and originality of a preliminary version he had given me to read that I urged him to publish it at once. Walter wisely rejected my advice, waiting until it had reached full flower as Chapter V of the odd order paper.

When asked what techniques he used, Thompson is reputed to have replied: "Sylow's theorem." This was only partially facetious, for on the surface all Thompson seemed to be doing was taking centralizers of elements and normalizers of subgroups of prime power order in contexts in which these "local" subgroups, as Alperin was later to refer to them, were all solvable. Thompson's originality lay in the questions he was raising and in his realization of the significance they had for the subgroup structure of the group G he was investigating. In a sense his questions turned G on its head: fixing a Sylow p-subgroup P of G, Thompson focused on the collection of P-invariant subgroups of G of order prime to p. He termed these *P-signalizers* and denoted them by $и(P)$ (the symbol designating N upside down). What can one say about the structure of the subgroup of G generated by all P-signalizers? Assuming P contains a normal abelian subgroup of rank ≥ 3, he was able to prove in the odd order context (and later in his study of simple groups in which all proper subgroups are solvable) that the elements of $и(P)$, in fact, generate a p'-group X. Thus, under these conditions the set of all P-signalizers possesses a unique maximal element, namely X. In the odd order theorem, by varying the prime p, Thompson went on to study the corresponding maximal p-signalizers, and thereby obtained an initial description of the structure of the maximal subgroups of G. In the later minimal simple group problem, it was 2-signalizers alone and the structure of a maximal subgroup containing a Sylow 2-group that would come to dominate the analysis.

Feit suggested that a good way for me to get into simple group theory would be by extending a recent result of Suzuki, who had determined all simple groups which contain an element of order 4 commuting only with its own powers — I should try to find all simple groups whose *automorphism* groups contain such an element of order 4. The given condition quickly implies that such a group G must have dihedral Sylow 2-groups, and Feit told me to speak to John Walter who had just begun thinking about the general dihedral Sylow 2-group problem. The two of us soon decided to work together on the problem, and thus began a long and fruitful, if often stormy, collaboration.

The known simple groups with such Sylow 2-groups consist of the family $L_2(q) = PSL_2(q)$, q odd, $q \geq 5$, plus the alternating group A_7. Using Brauer's character-theoretic methods, Brauer, Suzuki, and Wall had characterized these linear groups by the approximate structure of the centralizers of their involutions and Suzuki had obtained a similar characterization of A_7 as part of his self-centralizing order 4 theorem. In $L_2(q)$ itself, these centralizers are dihedral groups, while in A_7 they are groups of order 24 with dihedral Sylow 2-groups. Moreover, in $L_2(q)$ and A_7 these centralizers have index 1 or 3 in maximal subgroups; and it was in terms of such structures and embedding that Brauer, Suzuki, and Wall had obtained their characterization theorems.

On the other hand, in an arbitrary simple group G with dihedral Sylow 2-groups, all one can assert at the outset is that the centralizer C of an involution has the form

$$C = O(C)S,$$

where $O(C)$ denotes the unique largest normal subgroup of C of odd order, called the *core* of C by Brauer, and $S \in \mathrm{Syl}_2(G)$. In particular, C has a normal 2-complement. On the other hand, there is no a priori reason why $O(C)$ is not as complicated a group of odd order as one can dream up, far from being cyclic as in the known groups. Likewise C may be far from a maximal subgroup of G.

Thus Brauer's type of hypothesis left completely open the problem of how to force centralizers of involutions to possess a structure that approximated those in the groups one was trying to characterize. Neither John Walter nor I realized then that we were encountering a central problem in the classification of the simple groups and that our initial gropings represented the beginnings of what was eventually to become an extensive theory aimed at the elimination of such "core obstruction" in the centralizers of involutions. During that year in Chicago we made only partial inroads into the general dihedral problem, limiting ourselves to the special case in which $O(C)$ was assumed to be abelian.

Returning to Clark University in Worcester, Massachusetts, but living in a suburb of Boston, I began to attend Brauer's group theory seminar at Harvard. Thompson had just become an Assistant Professor at Harvard, but unfortunately for me he was to remain only for a single year, returning to Chicago as an Associate Professor the following year. At the time such extremely rapid academic advancement was much rarer than it has since become.

During that year at Harvard, Thompson began his monumental classification of the minimal simple groups. He soon realized that he didn't need to know that *every* subgroup of the given group was solvable, but only its local subgroups, and he dubbed such groups *N-groups*. However, the odd order theorem was still fresh in his mind. One afternoon I ran into him in Harvard Square and noticed that he had a copy of Spanier's book on algebraic topology under his arm. "What in the world are you doing with Spanier?" I asked. "Michael Atiyah has given a topological formulation of the solvability of groups of odd order and I want to see if it provides an alternate way of attacking the problem," was his reply.

Through Brauer's seminar and the many colloquium parties around Boston, Brauer and I gradually became good friends. Perhaps it was my Boston schooling, but I found it very difficult to address him other than as "Professor Brauer." He became upset with the deference I showed him and insisted that I call him "Richard." Now that I have passed the age that Brauer was at that time, I can better appreciate his desire to be treated simply as an equal among mathematicians of any age.

At that time, I found our mathematical conversations slightly uncomfortable. We always seemed to be talking past each other. Brauer had come to finite group theory via algebraic number theory and the theory of algebras. Many of the questions he posed about groups had an arithmetic basis and his way of thinking about groups was through their representations and especially their character tables. He never studied the local group-theoretic developments that were springing up around him. I was always surprised when he would refer to some comment of mine as "my methods." From my perspective local analysis *was* finite group theory; character theory, despite its undisputed power, seemed to me to be imposed on the subject from the outside. I had no way of foreseeing that just a few years later, he and I together with Alperin would forge a beautiful synthesis of both techniques in classifying groups with semidihedral or wreathed Sylow 2-groups.

It took John Walter and me four years of sustained effort to prove that $L_2(q)$, q odd, and A_7 are, in fact, the only simple groups with dihedral Sylow 2-subgroups. To guide us, we imagined that the local structure of the group G we were studying was similar to that of a natural "prototype": namely, the semidirect product G^* of a group X^* of odd order by $L_2(q)$ or A_7. In particular, if C^* denoted the centralizer of the involution in G^* corresponding to C, then $X_0^* = X^* \cap C^*$ would be a normal subgroup of C^* and so under our assumption on the subgroup structure of G, C would contain a normal subgroup X_0 of the same general shape as X_0^*. Of course, the simplicity of G should somehow force X^* and hence also X_0 to be the identity.

On the other hand, from the available data about the structure of centralizers of involutions in G, there was no visible subgroup X in G corresponding to X^* — only its intersections with various centralizers of involutions. To prove that $X_0 = 1$, we realized we would first have to *construct* a subgroup X in G that corresponded to X^*. Because G was simple, while X^* was normal in G^*, we should then presumably be able to use the embedding of X in G to force the desired conclusion $X = 1$.

We tried to model construction of this "pseudo-normal" core obstruction subgroup X and to derive properties of its embedding in G after Thompson's N-group analysis.

As mentioned earlier, Thompson had shown in the N-group situation that for $S \in \mathrm{Syl}_2(G)$, $\mathscr{U}(S)$ possesses a unique maximal element Y (assuming A contains a normal abelian subgroup of rank ≥ 3). Thus this subgroup Y represented the corresponding obstruction subgroup in the N-group case. As a consequence, it was to be expected that the normalizer $M = N_G(Y)$ of Y in G would turn out to be a "large" subgroup of G; and indeed Thompson argued that M contained the normalizer in G of every nonidentity subgroup of S — in particular, the centralizer in G of every involution of S. Assuming $Y \neq 1$ (whence M is proper in the simple group G), M was thus an example of what soon came to be called a *strongly embedded* subgroup.

Groups containing a strongly embedded subgroup had been in the air from the early 1960s, beginning with Suzuki's work on doubly transitive permutation groups in which a one-point stabilizer contains a regular normal subgroup of even order (Suzuki's family $Sz(2^n)$ consists of doubly transitive groups with this property, as do the groups $L_2(2^n)$ and $U_3(2^n) = PSU_3(2^n)$, the 3-dimensional projective unitary group over $GF(2^n)$). Instrumental to Suzuki's ultimate classification of such doubly transitive groups was the concept of a coherent set of characters that had been introduced by Feit in his own work on permutation groups of this type and which was soon to play such an important role in the odd order theorem.

It was only a few years later that Helmut Bender gave a complete classification of groups G containing a strongly embedded subgroup M. The given conditions on M are equivalent to the assertion that in the permutation representation of G on the set of G-conjugates of M every involution of G fixes a unique point. Bender argued first that any permutation group with this property is necessarily doubly transitive and went on to show that its one-point stabilizer M possesses a regular normal subgroup of even order. Thus, in effect, his proof consisted of a reduction to Suzuki's prior classification theorem.

Bender's general result was not available for the initial N-group and dihedral Sylow 2-group analyses, which required somewhat ad hoc arguments to treat their strongly embedded subcases. However, Bender's theorem enables one to give uniform operational meaning to the terms "pseudo-normal" subgroup and "elimination of core obstruction" in the centralizers of involutions. Indeed, in the presence of such core obstruction, the basic strategy is to construct a nontrivial subgroup X of G of odd order whose normalizer M is strongly embedded. Bender's theorem then yields the possibilities for G and M: namely, $G \cong L_2(2^m)$, $U_3(2^n)$, or $Sz(2^n)$ with M a Sylow 2-normalizer of G. However, in these groups a Sylow 2-normalizer possesses no nontrivial normal subgroups of odd order. Since X is such a normal subgroup of M, this is a contradiction, and we conclude that no such core obstruction can exist.

In the dihedral Sylow 2-group problem John Walter and I still had the primary task of producing the pseudo-normal obstruction subgroup X with a strongly embedded normalizer. We were unable to emulate Thompson's N-group analysis directly, for it was no longer necessarily true in our prototype G^* that X^* is the unique maximal element of $\varkappa(S^*), S^* \in \mathrm{Syl}_2(G^*)$. In fact, it is entirely possible that the S^*-signalizers generate G^* itself. In effect, this forced us to consider nonsolvable local subgroups of G in the process of constructing the desired subgroup X. As one step in that construction, we verified that the groups $L_2(q)$ and A_7 were "p-stable" for all odd primes p, a notion we introduced to describe the faithful action of a group H on a $GF(p)$-module V in which no nontrivial p-element of H has a minimal polynomial of the form $(1-x)^2$ in its action on V. The notion of p-stability was soon to become an important general concept of local group theory.

As a consequence of the elimination of core obstruction, we were able to establish a crude approximation of the structure and embedding of our centralizer C with that of the centralizer of an involution in $L_2(q)$ or A_7. However, to reach the exact hypotheses of the Brauer-Suzuki-Wall characterizations of the family $L_2(q)$ or Suzuki's corresponding characterization of A_7 required considerable additional analysis, based primarily on Brauer's theory of blocks of characters. But in the end John Walter and I succeeded in proving the first characterization of simple groups in terms of the structure of their Sylow 2-groups.

By far the largest portion of both my own and John Walter's subsequent work on simple groups has been devoted to developing techniques for eliminating core obstruction and applying them in successively more general classification theorems; it is for this reason that I have discussed the dihedral Sylow 2-group problem at such length.

Our collaboration was extremely valuable to both of us, but it had not been an easy one. We frequently argued over different potential approaches to a particular aspect of the problem, holding tenaciously to our own preferred perspective and yielding only when the inherent character of the problem forced us in one or another direction. Furthermore, it often took me a long time to grasp some point John was trying to make.

The most extreme example of this communication gap occurred some time later during the 1968–1969 group theory year at the Institute for Advanced Study. John was upset by a paper of mine, of which I had just received the galleys, and claimed that he had previously pointed out one of its central ideas. I didn't doubt him at all, but was quite dismayed, for I had no idea that the earlier conversation to which he was referring had any connection with the contents of the paper. I immediately added a footnote that John

Walter had independently obtained the same result. Despite our difficulties, we remained close friends, and collaborated on several occasions after the dihedral Sylow 2-group theorem. This was perhaps inevitable since John and I were at that time the only two finite group theorists besides Brauer concerned with the general structure of centralizers of involutions in simple groups.

With the completion of the dihedral problem, John wanted to study groups with abelian Sylow 2-groups. This was a good test case, for now the centralizer of an involution could a priori have arbitrarily many nonsolvable composition factors, whereas in the dihedral and N-group problems, the centralizer of every involution was necessarily solvable. On the other hand, I was more interested at that time in trying to get a general handle on the problem of core obstruction. But our perspectives were similar, both desiring to develop techniques for forcing the centralizers of an involution in an arbitrary simple group to have a structure approximating that of one of the known simple groups, and relying on others to establish Brauer-type characterizations in terms of the exact structure of these centralizers. As a result, our work was far removed from the new sporadic simple groups being discovered in the 1960s. These exciting developments were adding an entirely new dimension to the study of simple groups, considerably heightening the interest the recent classification theorems had already generated. Everyone working in the field was affected by the sporadic groups syndrome, and a whole generation of young mathematicians was soon attracted to the study of simple groups.

The discovery of new simple groups followed its own natural rhythm. It was Rimhak Ree who first realized the implications of the Lie-theoretic interpretation of Suzuki's family, which he then used as a model for constructing two further families of simple groups, related to the exceptional groups $G_2(3^n)$ and $F_4(2^n)$, respectively. Ree's groups constituted the last of the sixteen families of finite simple groups of Lie type.

Centralizers of involutions in Ree's groups of characteristic 3 had an especially simple structure: $Z_2 \times L_2(q)$, q odd (as well as elementary abelian Sylow 2-groups of order 8). In addition, like Suzuki's family, these groups were doubly transitive with a one-point stabilizer containing a regular normal subgroup. It was therefore natural to attempt a Brauer-type characterization of this family in terms of centralizers of involutions of this form. The work was begun by Nathaniel Ward, a student of Brauer's, and taken up independently by Janko and Thompson. Together, they established the doubly transitive nature of such a group of "Ree type" provided $q \geq 5$.

Classification of all doubly transitive groups in which a one-point stabilizer contains a regular normal subgroup turned out to be one of the single most difficult chapters of the entire classification proof, with the Ree type groups by far the hardest subcase. [Ultimately they were shown to consist of the groups

$L_2(q)$ and $U_3(q)$ together with the Suzuki groups (of characteristic 2) and the Ree groups of characteristic 3: precisely the groups of Lie type of Lie rank 1.] The Ree group case was not completed until well into the 1970s by Enrico Bombieri, who took up the problem where Thompson had left it after a series of three papers, spaced over almost a decade's time. Thompson's analysis, consisting largely of very delicate generator-relation calculations, had shown that the isomorphism type of G was uniquely determined by the value of a single parameter σ associated with G. Using classical elimination theory, Bombieri was able to prove that σ had the same value as in the corresponding Ree group. [In the Ree groups themselves, $q = 3^n$, n odd, $n > 1$ and σ is an automorphism of $GF(q)$ satisfying the condition $x^{\sigma^2} = x^3$ for all $x \in GF(q)$.]

Janko was drawn to the exceptional case $q = 5$, which could not be directly ruled out as a possible value of q in a group of Ree type. At the outset, Janko had no particular reason to think that a simple group G existed with such a centralizer of an involution. However, his attitude changed when his character-theoretic computations did not yield a contradiction, but rather that such a G would be forced to have order $175,560 = 2^3 \cdot 3 \cdot 5 \cdot 7 \cdot 11 \cdot 19$. When he next was able to show that G would also have to possess a 7-dimensional rational representation over $GF(11)$, he became convinced that a simple group with the given properties must exist. Ultimately, his construction of J_1, the first sporadic group in a century, was obtained as a group generated by two specific 7×7 matrices with coefficients in $GF(11)$.

This was a remarkable achievement, for Janko had been self-trained in Yugoslavia; and although he had spent some time in Frankfurt with Reinhold Baer and had already written several papers in group theory, he was now working in almost complete mathematical isolation at Monash University in Australia. His discovery electrified the group theory world to a far greater extent than had the Suzuki-Ree groups which, because of their Lie-theoretic basis, were regarded as part of our "normal" universe. But Mathieu's five groups had remained its sole exceptional constellation for 100 years.

Encouraged by his success, Janko daringly concluded that if a slight "perturbation" of the centralizer of an involution in a Ree group could lead to a new group, perhaps the same might be true for other known centralizers. But which known simple group to select and how to perturb the centralizer of one of its involutions was not an easy decision, for the probability of success was extremely small (although, of course, unknown at the time, there remained only a total of 20 as yet undetected possibilities in the entire firmament of finite simple groups). Moreover, the process of deriving a contradiction from an incorrect choice of centralizer could well require almost as much effort as construction of a new simple group from a correct one.

But despite the enormous range of available possibilities, miraculously Janko's first choice was a correct one! The centralizer of an involution in the Mathieu group M_{12} is the semidirect product of a nonabelian group of order 32 by the symmetric group Σ_3, so Janko pinned his hopes on the semidirect product of a nonabelian group of order 32 by the alternating group A_5. And wonder upon wonder, at the end of his analysis, there emerged not one, but two new sporadic groups J_2 and J_3 of respective orders $2^7 \cdot 3^3 \cdot 5^2 \cdot 7$ and $2^7 \cdot 3^5 \cdot 5 \cdot 17 \cdot 19$. Not one for excessive modesty, Janko titled his paper "Some new simple groups of finite order, I," although nowhere in the paper could his groups be found, only the "experimental evidence" for their existence. Janko was operating on the metamathematical principle (as later events would fully justify) that determination of an explicit order, a compatible local subgroup structure, and character table for a "potential" simple group is a sufficient predictor of the existence of an actual group.

Despite Janko's failure to establish the existence of either group, his work represented an outstanding achievement; but it remained for Marshall Hall and David Wales to construct J_2 and for Graham Higman and John McKay to construct J_3. Janko's data implied that *if* such a group J_2 existed, it would necessarily possess a primitive permutation representation of degree 100 with $U_3(3)$ as one-point stabilizer, having transitive constituents of degrees 1, 33, and 66, respectively. It was in this form that Hall and Wales carried out their construction, by producing a graph with 100 vertices on which the desired group J_2 acted as a group of automorphisms.

A general theory of such rank 3 primitive permutation groups had earlier been developed by Helmut Wielandt and Donald Higman (note that all doubly transitive groups are in this sense rank 2 primitive permutation groups); however, no one had ever thought to examine the several known exceptional combinatorial configurations related to such groups. But now with the demonstrated existence of J_2, a flurry of activity occurred around these exceptional configurations, leading quickly to the construction of three further sporadic groups of this type — by D. Higman and Charles Sims, Jack McLaughlin, and Suzuki, respectively. The Higman-Sims group was, in fact, constructed within 24 hours of Hall's lecture at Oxford on the construction of J_2, likewise as a rank 3 primitive permutation group of degree 100, but with the Mathieu group M_{22} as one-point stabilizer and subdegrees 22 and 77. Not too long thereafter Arunas Rudvalis produced the evidence for yet a fourth such rank 3 permutation group.

I first met Janko during the Institute's 1968–1969 group theory year. He lived up to his colorful reputation: forceful, with a distinctive manner of expressing his enthusiasms. He was fully confident that his centralizer perturbation technique was bound to uncover many more new simple groups. In fact, not long before, one of his students, Dieter Held, had produced the evidence for a sporadic group with centralizer of an involution isomorphic to that of the largest Mathieu group M_{24}. Now Janko had decided to perturb the centralizer of an involution in McLaughlin's sporadic group, which was isomorphic to the double cover $2A_8$ of the alternating group A_8. Janko was analyzing the cases $2A_9$ and $2A_{10}$ with great determination. He showed me two notebooks full of carefully hand-written arguments, the statements of individual lemmas often covering nearly a page — one of his well-known trademarks. The arguments involved an elaborate combination of character theory and local analysis, but unfortunately their upshot was that no simple groups existed with centralizer of involution of either of these shapes.

Janko could hardly be faulted for dropping the project — after all, it was new simple groups he was seeking not contradictions. But he was by no means discouraged and soon shifted his attention to another, even more ambitious type of centralizer of involution problem that would later yield a fourth sporadic group to bear his name. However, this time fortune was against him, for if Janko had only persevered one further degree to $2A_{11}$, yet another sporadic group would have rewarded his efforts!

The groups $2A_{10}$ and $2A_{11}$ have isomorphic Sylow 2-groups of order 2^8, and this 2-group had previously surfaced as an exceptional potential candidate for a Sylow 2-group of a simple group in Anne MacWilliams's thesis under Thompson, in which she studied 2-groups that possess no normal abelian subgroups of rank ≥ 3. With this foreknowledge, Thompson suggested to his student Richard Lyons that it might be worth considering the $2A_{11}$ centralizer of an involution problem, and so the Lyons sporadic group came to pass. With it, the discovery of sporadic groups had come full circle: centralizers of involutions motivating the study of rank 3 primitive permutation groups, and one of the resulting groups in turn suggesting a series of centralizer of involution problems that yielded a further sporadic group.

But the sporadic groups were not to be so neatly pigeon-holed, for two further constellations of groups were then emerging from other, quite surprising sources: John Conway's three sporadic groups in the late 1960s from his brilliant examination of the automorphism group of the 24-dimensional Leech lattice and shortly thereafter Bernd Fischer's three sporadic groups from his attempt to characterize the symmetric groups from properties of their conjugacy class of transpositions.

These discoveries served to intensify the search for sporadic groups, which group theorists felt must be lurking behind every unusual configuration. So, along with centralizers of involutions, automorphism groups of other interesting graphs and integral lattices were systematically investigated as were variations of Fischer's transpositions. But the few sporadic groups that, in fact, remained to be uncovered were not to be so easily coaxed out of hiding; and in the end, the graph- and lattice-theoretic approaches would yield no further groups. Moreover, Fischer's investigations of this period gave no hint of the ultimate sporadic group yet to come — the wonderful monster of Fischer and Robert Griess — with its magical connections to elliptic functions, automorphic forms, and mathematical physics.

George Glauberman's Ph.D. thesis under Bruck at the University of Wisconsin had concerned the structure of *loops* of odd order; yet within a few years of his arrival at the University of Chicago in 1965, he had proved two fundamental results of far-reaching import for the local analysis of finite simple groups. His Z^**-theorem*, generalizing a theorem of Brauer and Suzuki on groups with quaternion Sylow 2-groups, showed that a Sylow 2-group S of a simple group G contains no *isolated* involutions — i.e., for any involution x of S, S always contains a G-conjugate y distinct from x. In fact, as is easily seen, y can always be chosen to centralize x. Thus $C_G(x)$ and $C_G(y)$ are distinct, isomorphic centralizers of involutions with $y \in C_G(x)$ and $x \in C_G(y)$. Interrelationships between two such centralizers have strong consequences for both the analysis of 2-fusion in G and for the structure of the centralizers of its involutions.

Glauberman's equally significant ZJ*-theorem*, building on some earlier results of Thompson, became an indispensible tool for studying the structure of p-local subgroups, p an odd prime. Under the assumption that H is "p-stable" and $C_H(O_p(H)) \leq O_p(H)$, the ZJ-theorem asserts that the characteristic subgroup $Z(J(P))$ of a Sylow p-subgroup P of H is, in fact, *normal* in H. Here $O_p(H)$ denotes the unique largest normal p-subgroup of H and $J(P)$ denotes the *Thompson* subgroup of P, which is generated by the abelian subgroups of P of maximal order.

Whereas the proof of the Z^*-theorem had been based on Brauer's general theory of blocks of characters, that of the ZJ-theorem involved delicate commutator calculations related to subgroups of P. Subsequently, Glauberman was to refine this commutator calculus into an art of consummate depth and technical virtuosity. One could look at a few pages of one of his papers, without knowing its author and immediately conclude that "Only George could have done this."

Although Glauberman often displayed a wry sense of humor, his demeanor was very serious and rather formal, and this quality was reflected in his distinctive lecturing style. He would write a sentence on the blackboard, read it verbatim, and continue in this fashion for the entire lecture. What never ceased to amaze me was that with such an expository style and often beginning a series of lectures with the most basic material, not far from the very definition of a group, George could build steadily until no later than the third lecture he would be presenting deep, highly original mathematics.

Although Alperin wrote his Ph.D. thesis with Graham Higman on p-groups during the 1960–1961 Chicago group theory year, he was, in fact, then a second year graduate student at Princeton. He had chosen to go to Princeton because of its loose doctoral requirements — one year of residence and a good thesis sufficed. At Chicago he was really a professional mathematician, spending more time with the faculty than with other graduate students. When the Christmas holidays rolled around and I was returning to Newton, Massachusetts to visit my wife and children, who had remained behind while I was in Chicago, we discovered to our mutual surprise that Jon had the same destination, for his parents lived in Newton only a short distance from my home. This was the start of a long and close relationship, for whenever he would later come home on a visit, we would get together to discuss mathematics.

By the mid 1960s we had written two joint papers: One on the Schur multipliers of the Suzuki groups and of the Ree characteristic 3 groups, the other refining Alperin's fundamental fusion theorem, which asserts that for any prime p global p-fusion in a group is p-locally determined in a very precise fashion. Since the dihedral Sylow 2-group problem was at last behind me, we decided it was time to consider another general classification theorem. Since the defining relations of semidihedral 2-groups are very similar to those of dihedral 2-groups, we concluded that a good choice of problem might be to study groups with semidihedral Sylow 2-groups.

Here the target groups were the families $L_3(q)$, $q \equiv -1(\bmod 4)$ and $U_3(q)$, $q \equiv 1(\bmod 4)$, and the smallest Mathieu group M_{11}. [The groups $L_3(q)$, $q \equiv 1(\bmod 4)$, and $U_3(q)$, $q \equiv -1(\bmod 4)$, have "wreathed" Sylow 2-groups (i.e., isomorphic to $Z_{2^n} \int Z_2, n \geq 2$).] Having the dihedral Sylow 2-group analysis to guide us, we first determined the general structure of the centralizer C of an involution of a minimal counterexample G and then attempted to eliminate core obstruction in C, so that its structure would begin to approximate that of the centralizer of an involution in one of the target groups we wished to characterize. However, we were soon faced with a difficulty that had not been present in the earlier problem: the existence of non p-stable p-local subgroups for some odd prime p. This possibility could occur now because the group $SL_2(p^r)$ might be involved in such a p-local subgroup and in the action of $SL_2(p^r)$ on its natural module, p-elements *do* have quadratic minimal polynomials. Try as we would, we were unable to construct the desired strongly embedded subgroup in the presence of core obstruction and non p-stability. Instead, our local analytic arguments succeeded only in proving the existence of what we referred to as a *weakly embedded* subgroup M of G. By definition, M contained a Sylow 2-group S of G and for every involution x of G, we could not quite assert that $C_G(x) \leq M$, but only that M "covered" $C_G(x)$ modulo its core — that is,

$$C_G(x) = C_M(x)O(C_G(x)).$$

It also followed from the minimality of G that $M/O(M)$ contained a normal subgroup of odd index isomorphic to $L_3(q)$, $U_3(q)$, or M_{11}. Unfortunately, there was no Bender-type classification of groups with such a weakly embedded subgroup M for us to quote to obtain a contradiction.

Finally, we decided to ask Brauer whether his character-theoretic methods might provide a way of eliminating this troublesome configuration. It had been Brauer's results about the family $L_3(q)$, $q \equiv -1(\bmod 4)$, in the 1950s that had represented the first centralizer of involution characterization theorem and which had motivated Hayden's thesis problem. To establish that earlier result, it had been necessary for Brauer to first analyze the structure of the principal 2-block of an arbitrary group with semidihedral Sylow 2-groups. He was now able to bring this general knowledge to bear in considering our problem. To our delight, Brauer soon produced a beautiful, complete solution. Indeed, by comparing the characters in the principal 2-blocks of G and M, he was able to prove that there existed a nontrivial character χ in the principal 2-block of G whose restriction to M *remained irreducible*. It then followed on general principles that $O(M)$ is contained in the kernel of χ. However, as G was by assumption simple, this forced $O(M) = 1$, contrary to the fact that Alperin and I had constructed M in the first place as a p-local subgroup of G for some odd prime p. [The case $M/O(M) \cong M_{11}$ was exceptional for Brauer, but he provided us with a separate character-theoretic argument to cover the case $C_G(x)/O(C_G(x)) \cong GL_2(3)$ for some involution x in full generality.]

Alperin was a meticulous expositor, both in his lectures and in his writings, and he painstakingly prepared a draft of the long and difficult local-theoretic portions of the semidihedral analysis, while Brauer wrote up his character-theoretic results. In the meantime, I spent several months extending our local arguments to the wreathed case. But Alperin felt he had had enough of general classification theorems and definitely preferred to leave the wreathed problem to someone else. However, with my commitment to the total classification of the simple groups, I knew there was a strong likelihood that "someone else" would turn out to be me, and I did not look forward to the prospect of preparing another 300-page manuscript conceptually similar in outline to the semidihedral one.

Finally, we turned to Brauer to arbitrate the dispute, letting him decide whether or not to include the wreathed case in our writeup. When Brauer produced a thick notebook from his files containing the corresponding analysis of the principal 2-block of groups with wreathed Sylow 2-groups, that conclusively settled the matter! This was, of course, a considerable relief to me, but it had a further benefit as well, for once Alperin had adjusted to the fact that we were covering both the semidihedral and wreathed cases, he produced a short, elegant fusion-theoretic argument that in a simple group of 2-rank ≤ 2, Sylow 2-groups are necessarily either dihedral, semidihedral, wreathed, or homocyclic abelian or else have order 2^6 and are isomorphic to those of $U_3(4)$. Thus, with Lyons's subsequent characterization of $U_3(4)$ by the structure of its Sylow 2-group and Brauer's elimination of the homocyclic abelian possibility, our final result together with the earlier dihedral

Sylow 2-group theorem, yielded a complete classification of all simple groups of 2-rank ≤ 2. It had already been evident from the N-group case analysis that there would be a fundamental dichotomy between the methods needed to study simple groups of 2-rank ≤ 2 and those of 2-rank ≥ 3. As a consequence, our classification theorem had greater significance than a result of its type would otherwise have warranted.

A final aspect of the semidihedral, wreathed problem was not settled until that 1968–1969 group theory year at the Institute. When Alperin, Brauer, and I arrived for the year, prepared to complete our proof, there existed only a partial centralizer of involution characterization of the groups $U_3(q)$, analogous to Brauer's earlier $L_3(q)$ theorem. However, Brauer's character-theoretic methods implied only that a group G with centralizer of an involution approximating that in $U_3(q)$ was once again doubly transitive with one-point stabilizer possessing a regular normal subgroup and two-point stabilizer cyclic of order $(q^2 - 1)/d$, where $d = \text{g.c.d.}(q + 1, 3)$, but did not handle the classification of such doubly transitive permutation groups.

Suzuki had successfully treated the case $q = 2^n$ for both values $d = 1$ and 3, but for odd q he had only been able to push through the resulting generator-relation analysis when $d = 1$. Thus Alperin, Brauer, and I were faced with the prospect of stating the conclusion of our main theorem in the form "then $G \cong L_3(q)$, q odd, M_{11}, or G is doubly transitive of 'unitary type,'" a frustratingly anti-climactic statement with which to end our long and difficult argument.

Early in 1969, at the suggestion of Walter Feit, Ed Nelson, then chairman of the Princeton math department, called me to ask whether I would consider taking on a graduate student, whom as a matter of strict policy the Princeton department would not support beyond four years. He told me that Mike O'Nan was a strong student who had worked unsuccessfully on a difficult problem in analysis and had subsequently become interested in finite group theory. Since I had just accepted a position at Rutgers for the coming fall, this seemed to be a fine idea, and I agreed to meet with O'Nan.

At the time, I had not yet heard Tony Tromba's description of his fellow graduate student O'Nan as the brightest person he had ever met; all I could see was a cheerful freckle-faced young fellow who was describing the reading he had done in group theory. I suggested some further material for him to study and told him I would arrange for his enrollment as a graduate student at Rutgers. At the beginning of March he returned, saying he felt ready for a thesis problem. This seemed to me to be rushing it a bit, but nevertheless I went ahead and put forth several suggestions. Among them was the problem of groups of unitary type, which I brought up more to relieve my own frustration at the unsatisfactory status of the semidihedral, wreathed theorem. Given Suzuki's experience with the problem, it was clearly unwise to suggest it to a graduate student whom I hardly knew.

About a month later, O'Nan phoned me up, "I think I've made some progress on that problem you gave me, and besides I may have found a new simple group." When I realized he was referring to the unitary problem, I became rather skeptical, but agreed we should meet. By the time he arrived at my office a few days later he had shown that his "new" simple group was none other than $U_3(5)$ in a not so easily recognized guise. But he handed me a short manuscript that purported to give a local proof that the automorphic parameter σ of $GF(q)$ associated with a group of unitary type had to be the identity, a conclusion Suzuki had been able to achieve in the $d = 1$ case only by global arguments. When Suzuki, who was also spending the year at the Institute, confirmed the validity of O'Nan's argument, I knew something special was at work here.

In fact, by June, O'Nan had completely solved the unitary type problem by a brilliant combination of generator-relation and geometric analysis, which included the only application I have ever seen of contour integration to a purely algebraic problem. Obviously there was no point in O'Nan's enrolling as a graduate student at Rutgers! Professor Iwasawa read the thesis and O'Nan received his doctorate at Princeton, arriving at Rutgers in the fall as a postdoctoral fellow.

I had another unusual graduate student experience that Institute year. In 1964 I had left Clark for Northeastern University in Boston and Thomas Hearne, one of my graduate students at Northeastern, was in the process of establishing a centralizer of involution characterization of the Tits simple group (the Ree group associated with $F_4(2^n)$ is simple only for $n > 1$; but for $n = 1$, it contains a simple subgroup of index 2, whicn was first studied by Tits). One day Hearne called me from Boston to tell me he thought the Tits group possessed only solvable local subgroups, which would mean it was an N-group. However, this group was not on Thompson's initial list of N-groups, so again I was immediately skeptical and began to pepper Hearne with questions. But as he had a satisfactory answer for each of my questions, I suggested he call Thompson in Chicago.

In the meantime, I went back to my copy of the first draft of Thompson's N-group proof which he had prepared back in 1964. He had hoped to complete it by the time of the International Congress in Moscow where he was to receive the Fields Medal. But because of the inordinate length of the proof — over 600 typed pages — he had been unable to finish the final chapter, which in the preprint consisted solely of a sequence of statements without proofs. This chapter treated the case of *thin* groups of 2-rank ≥ 3 not containing a strongly embedded subgroup (by definition, a group is thin if Sylow p-subgroups of every 2-local subgroup are *cyclic* for all odd primes p). It was clear from its internal structure that the Tits group satisfied these conditions, so here obviously was the place for me to look. There, on the very last page of the chapter, appeared the assertion that every maximal 2-local subgroup of a thin N-group G has order $2^a \cdot 3$. However, the Tits group contains a maximal 2-local of order $2^{10} \cdot 5$. Thus, even if the Tits group turned out to be an N-group (as it did), I realized this would affect only the very end of Thompson's argument. Because of the length of the proof, Thompson was to publish the N-group paper in six installments, from 1968 to 1974; and there in Part VI the Tits group emerges out of the intricate local analytic arguments as the unique thin solution.

My investigations of the general problem of elimination of core obstruction in the centralizers of involutions proceeded more or less concurrently with my work on the semidihedral, wreathed problem. In the N-group paper, Thompson had reduced the analysis of S-signalizers for $S \in \mathrm{Syl}_2(G)$ to the corresponding question about A-signalizers, where A is a maximal normal abelian subgroup of S. He had argued that the set $\varkappa(A)$ of all A-invariant subgroups of G generate a subgroup X_A of G of odd order. But clearly the elements of $\varkappa(A)$ are permuted among themselves by $N_G(A)$ and, in particular, by S as A is normal in S by assumption. Since every S-signalizer is obviously an A-signalizer, it then followed that X_A is, in fact, the unique maximal element of $\varkappa(S)$.

The advantage of working with an abelian subgroup A is that $A \leq C_G(a)$ for every $a \in A^\#$, whereas the corresponding inclusion does not hold with S in place of A if S is nonabelian. Hence A "remains in the act" throughout the analysis, which allows one to compare the embeddings of A in the various centralizers of elements of $A^\#$. In fact, Thompson was able to reduce the desired A-signalizer conclusion to the assertion that $O(C_G(a))$ is the unique maximal A-invariant subgroup of odd order in $C_G(a)$ for each $a \in A^\#$.

It was therefore clear to me that in considering a general simple group G, I should be focusing on abelian 2-subgroups A of G and A-invariant subgroups of $C_a = C_G(a)$ as a ranges over $A^\#$. Again I preferred to visualize the local subgroup structure of G in terms of a prototype G^*, which I now took to be the semidirect product of a group X^* of odd order by a group K^*, where $K^* = K_1^* \times K_2^* \times \cdots \times K_r^*$ is the direct product of known simple groups K_i^*, $1 \leq i \leq r$, and I let A^* be an abelian 2-subgroup of K^* corresponding to A. My aim was to find some *canonical* procedure for identifying the subgroup X^* of G^* from data limited to the groups $C_a^* = C_G^*(a^*)$ for $a^* \in (A^*)^\#$. I would then transfer this procedure to G and A in the hope of being able to construct a pseudo-normal subgroup X whose normalizer would turn out to be strongly embedded in G (whenever X was nontrivial).

As observed earlier, the set of A^*-signalizers in K^* might well generate K^*, in which case those in G^* would generate G^*. However, another class of examples puts the difficulty in even sharper focus. Indeed, if K^* contains a unique maximal A^*-signalizer W^* (as is entirely possible), then the subgroup

$Y^* = W^*X^*$ is the unique maximal A^*-signalizer in G^*. In such a situation, how is one to distinguish the subgroup X^* within Y^*? Especially as Y^* may possess many normal subgroups isomorphic to X^*.

John Walter was facing the very same questions with his prototype in the abelian Sylow 2-group problem: for him, $A^* \in \mathrm{Syl}_2(K^*)$ and each K_i^* was either isomorphic to $L_2(q_i)$ for suitable q_i (namely, $q_i = 2^{n_i}$ or $q_i \equiv 3, 5 \pmod 8$), to a Ree group of characteristic 3, or to Janko's group J_1. He tried to model his identification of X^* and the corresponding pseudo-normal obstruction subgroup X in his group G after our dihedral Sylow 2-group approach. However, because of the far greater complexity that the internal structure of G could now possess, the process was horrendously difficult. Observing his progress, I became increasingly convinced that without an alternate strategy for identifying obstruction subgroups, the entire classification program would soon become overwhelmed by technical difficulties.

In the search for a new approach, I made the trivial observation that for each $a^* \in (A^*)^\#$ the subgroups $C_{X^*}(a^*)$ are A^*-invariant of odd order and for any $b^* \in (A^*)^\#$ satisfy the relation

$$(^*) \qquad C_{X^*}(a^*) \cap C_{b^*} = C_{X^*}(b^*) \cap C_{a^*}.$$

Moreover, the subgroups $C_{X^*}(a^*)$ generate X^*.

Of course, in G itself, the desired subgroup X is not visible, so neither are the subgroups $C_X(a)$ for $a \in A^\#$. But condition $(^*)$ suggested that perhaps I should be looking for A-invariant subgroups $\theta(a)$ of C_a for $a \in A^\#$ satisfying the analogous relation

$$\theta(a) \cap C_b = \theta(b) \cap C_a$$

for all $b \in A^\#$; and I raised the crucial question: *Under such circumstances would the subgroups $\theta(a)$ generate a subgroup X_A of G of odd order*? If true, then X_A would be a potential candidate for the desired obstruction subgroup X.

Because of the "functorial" way I was approaching the problem, I decided to call a function θ satisfying the given compatibility condition an *A-signalizer functor* on G. Imposing several additional technical conditions on such A-signalizer functors θ, in time I succeeded in proving that the "closure" X_A of θ is, in fact, a group of odd order. Once again I used Thompson's N-group analysis as a guide, piecing together Sylow p- and q-subgroups of the $\theta(a)$'s as p, q ranged over all odd primes.

It never occurred to me that Saunders Mac Lane might view my use of the term "functor" as improper. My relationship with Mac Lane dated back to the early 1940s. I had written my undergraduate thesis with him (on finite abelian groups) and I was in the middle of my doctoral thesis on "Eilenberg-Mac Lane" spaces after World War II when Mac Lane left Harvard for the

University of Chicago. Now 20 years later, I had been invited to give a colloquium talk at Chicago on my signalizer functor theorem, but not until I had convinced Mac Lane that the term "functor" would never appear without the qualifying adjective "signalizer" did he agree to attend the lecture. Alperin found this story somewhat ironic, pointing out that not long before, he himself, Mac Lane's colleague, had introduced the term "conjugacy functor" to capture abstractly a particular type of conjugacy property within finite groups.

A few years later, David Goldschmidt gave a short, elegant proof of a far superior signalizer functor theorem, removing all my unpleasant technical hypotheses, and valid for abelian r-groups A of rank ≥ 4 and arbitrary primes r, under the assumption that each $\theta(a)$ is an A-invariant *solvable* r-subgroup of C_a. [When $r = 2$, the Feit-Thompson theorem implies, of course, that all A-signalizer functors satisfy this solvability condition.] Goldschmidt's lovely idea was to proceed by induction on the number of prime divisors of $|\theta(a)|$ as a ranges over $A^{\#}$, and from the given θ he constructed auxiliary A-signalizer functors on G with fewer such prime divisors. The theorem and proof were typical of Goldschmidt's many contributions to simple group theory. He had the capacity to stand back, looking past all the classification theorems being steadily produced, and see into the very heart of a particular problem.

At the time I concocted the notion of a signalizer functor, I had no idea it was destined to become the basis for a general technique for eliminating core obstruction in the centralizers of elements of arbitrary prime order in simple groups. Indeed, I was still faced with the serious problem of producing effective signalizer functors — i.e., signalizer functors whose closures would turn out to be the desired pseudo-normal obstruction subgroup X of G.

There was, however, one situation that included N-groups as a subcase, in which the definition of an appropriate signalizer functor was self-evident. John Walter and I introduced the term *balanced* group for the class of groups G such that for every pair of commuting involutions x, y of G,

$$O(C_x) \cap C_y = O(C_y) \cap C_x.$$

Obviously if A is any abelian 2-subgroup of a balanced group G and one sets for $a \in A^{\#}$

$$\theta(a) = O(C_a),$$

then θ defines an A-signalizer functor on G. Thus in this case, if A has rank ≥ 3, the signalizer functor theorem yields the conclusion that the subgroup

$$X_A = \langle O(C_a) | a \in A^{\#} \rangle \text{ is of odd order.}$$

[The signalizer functor theorem had by then been extended by Goldschmidt to the case in which A is an abelian 2-group of rank 3.] Under the assumption of balance, John Walter and I attempted to prove that the normalizer M of

X_A is strongly embedded in G (provided $X_A \neq 1$), thereby contradicting Bender's theorem and thus forcing the desired conclusion

$$X_A = O(C_a) = 1 \text{ for every } a \in A^{\#}.$$

We managed to verify strong embedding in a special case (which covered N-groups as a subcase), but despite considerable effort were unable to establish this conclusion for the general balanced group, succeeding only in proving the weaker result that M contains the normalizer in G of every 2-subgroup of M of rank ≥ 2. For brevity, call such a subgroup M *half*-strongly embedded in G.

In my mind, the "late years" begins with Michael Aschbacher's remarkable result in the early 1970s in only his second paper in finite group theory. By a dazzling display of technical brilliance and originality he showed in complete generality that a simple group (of 2-rank ≥ 3) containing a half-strongly embedded subgroup M either contains a strongly embedded subgroup (namely, M) or else is isomorphic to Janko's group J_1. In particular, this settled the balanced group problem with which John Walter and I had been struggling. From the moment of Aschbacher's half-strongly embedded paper, he was to assume a dominant role in the pursuit of the classification of the finite simple groups, producing one astonishing theorem after another with breath-taking rapidity. Only Thompson's appearance in the late 1950s can be compared with Aschbacher's dramatic entrance into the field.

However, back then in the 1960s, I was still groping for a way of producing effective signalizer functors, continuing to study my prototype $G^* = K^*X^*$ and abelian 2-subgroup A^* of K^*. It was the cores $O(C_{K^*}(a^*))$ for $a \in (A^*)^{\#}$ that were, of course, the cause of the difficulty. Balance corresponded to the case in which these cores are all trivial. This was indeed true if each of the simple factors $K_i^*, 1 \leq i \leq r$, of K^* were of Lie type of characteristic 2, an alternating group of even degree, or a sporadic group, but was known to fail, in general, if one of the K_i^* was of Lie type of odd characteristic or an alternating group of odd degree. On the other hand, one could at least assert, no matter what the isomorphism type of K_i^*, that $O(C_{K_i^*}(a^*))$ was a *cyclic* group.

Believing it unreasonable for such a "small" group to cause too much difficulty, I began to toy with the cores of the centralizers of involutions in a Klein four subgroup U^* of A^* in the hope of finessing the problem. I soon made the critical observation that

$$\bigcap_{u^* \in (U^*)^{\#}} O(C_{K^*}(u^*)) = 1,$$

provided none of the K_i^* were isomorphic to A_{n_i} for some $n_i \equiv 3 \pmod 4$. [If K^* is replaced by $\mathrm{Aut}(K^*)$ in the prototype G^*, then the groups $L_2(q_i)$ must be added to the list of exceptions for K_i^* for suitable odd q_i.] If one now sets $\Delta_{G^*}(U^*) = \bigcap_{u^* \in (U^*)^\#} O(C_{u^*})$, it follows from the preceding equality for K^* that

$$\Delta_{G^*}(U^*) \leq X^*$$

(again apart from the specified exceptions for K_i^*). Furthermore, as A^* was assumed to have rank ≥ 3, X^* is generated by the corresponding $\Delta_G(U^*)$:

$$X^* = \langle \Delta_{G^*}(U^*) | Z_2 \times Z_2 \cong U^* \leq A^* \rangle.$$

Thus I had achieved my objective of finding a "functorial" description of the subgroup X^* entirely in terms of A^* (and its embedding in G^*).

To exploit this observation, I decided to mimic the idea of balance and introduced the notion of a *2-balanced* group, defined by the conditions

$$\Delta_G(U) \cap C_G(V) = \Delta_G(V) \cap C_G(U)$$

for every pair of commuting four subgroups U, V of G.

At this point, there seemed to be only a single potential candidate for the desired A-signalizer functor θ: namely, for $a \in A^\#$,

$$\theta(a) = \langle C_a \cap \Delta_G(U) | Z_2 \times Z_2 \cong U \leq A \rangle.$$

Under the assumption that G is 2-balanced, I was indeed able to prove that θ was an A-signalizer functor on G whenever A had rank ≥ 4. By the signalizer functor theorem, the closure X_A of θ had odd order. However, it is immediate from the definition of θ that

$$X_A = \langle \Delta_G(U) | Z_2 \times Z_2 \cong U \leq A \rangle.$$

Thus I had succeeded in finding a functorial description of the sought-after pseudo-normal obstruction subgroup of the general 2-balanced group G.

The importance of 2-balance rests on the fact that it is very close to being a universal property of finite groups. Indeed, just as it holds in the prototype G^* if none of the K_i^* are alternating groups of odd degree or 2-dimensional linear groups of odd characteristic, so it can be shown to hold in an arbitrary group G provided certain critical composition factors of the centralizers of involutions of G are not of one of these isomorphism types. John Walter and I therefore began to study the theory of 2-balanced groups and to further develop what came to be called the "signalizer functor method."

In the formative years of signalizer functor theory, I was, of course, impatient to learn how effective it would be as a method for eliminating core obstruction in some specific Sylow 2-group characterization theorems. During the Institute's 1968–1969 group theory year, it soon became apparent to me that Koichiro Harada, recently arrived from Japan, was already a master at analyzing 2-fusion and 2-local structure in groups with "small" Sylow 2-subgroups and would therefore make an ideal partner with whom to test the theory. We settled on Janko's two groups J_2 and J_3, which have isomorphic Sylow 2-groups of order 2^7.

Beginning with an arbitrary simple group G with such a Sylow 2-subgroup S, Harada's task was to force $C_z/O(C_z)$ to be a split extension of a nonabelian group of order 32 by A_5, where z is the involution in the center of S, and I was to use signalizer functor theory to force $O(C) = 1$ — or as we euphemistically put it — to "kill the core". Once this was achieved, Janko's original centralizer of involution result would be applicable and it would follow that $G \cong J_2$ or J_3, the desired conclusion of our theorem.

To carry out my assigned task in this problem, it was first necessary to prove that G was, in fact, a balanced group. However, a priori, it was entirely possible that the centralizer of some involution possessed a non "locally balanced" composition factor isomorphic to $L_2(q)$, q odd, which thereby acted as an obstruction to balance. Because Harada and I were novices at the game and also because our analysis was being carried out prior to Goldschmidt's general signalizer functor theorem, achievement of our objective required a far more elaborate effort than we had anticipated. In particular, we had to first establish a similar characterization of $L_3(4)$ by its Sylow 2-group (of order 2^6 and isomorphic to a subgroup of our Janko 2-group of order 2^7).

During this eventful Institute year, I had accepted a position at Rutgers University, starting the next fall. If I hadn't already been living in Princeton, with Rutgers only a few miles away, I doubt very much if I would have been emotionally able to move permanently from the Boston area. By coincidence, Harada was to be spending a second year at the Institute. With this geographical proximity, we were able to continue our collaboration, pursuing characterizations of other known simple groups of 2-rank 3 and 4 by their Sylow 2-groups. Gradually we each became considerably more facile at our respective parts of the enterprise. Thus, in rapid succession, Harada and I obtained characterizations of the families $G_2(q), {}^3D_4(q), PSp_4(q)$, q odd, of groups of Lie type, the alternating groups A_n, $n = 8, 9, 10, 11$, and the sporadic groups M_{12}, M_{22}, and M_{23} as well as the McLaughlin and Lyons groups Mc and Ly by their Sylow 2-groups.

Beyond demonstrating the power of the signalizer functor method, this sequence of results put Harada and me in a position to approach a general problem that had arisen in connection with the construction of half-strongly embedded subgroups in balanced groups after identification of the pseudo-normal subgroup X had been achieved. This second phase of the signalizer functor method, dealing with the embedding of $M = N_G(X)$, only worked smoothly when a Sylow 2-group S of G is *connected* — i.e., for any two four-subgroups U, V of S, there exists a chain of four subgroups $U = U_1, U_2, \dots, U_m = V$ of S with U_i centralizing U_{i+1}, $1 \leq i \leq m - 1$.

Very much later, after the classification of the simple groups had been completed, Harada, Lyons, and I were to find an easy way of treating the problem of groups with nonconnected Sylow 2-groups, but at that time it was considered to be difficult as well as of critical importance. The problem was directly linked to MacWilliams's thesis results on 2-groups containing no normal abelian subgroups of rank ≥ 3, for it is almost immediate from the definition of connectivity that every 2-group containing such a normal abelian subgroup is connected.

On the other hand, there seemed to be no direct approach to the problem since the notion is not inductive — i.e., nonconnected 2-groups of rank ≥ 3 always contain connected subgroups and connected homomorphic images of rank ≥ 3, so that consideration of a minimal counterexample G to any proposed theorem would give no information about the structure of proper subgroups of G. However, included among MacWilliams's results was the following general theorem: If a 2-group S contains no normal abelian subgroups of rank ≥ 3 (in particular, therefore, if S is not connected), then every homomorphic image of every subgroup of S has rank ≤ 4. For brevity, we say that such a 2-group S has *sectional* rank ≤ 4.

MacWilliams's result showed that the set of nonconnected 2-groups is included within the *inductive* family of 2-groups of sectional rank ≤ 4. I naively suggested to Harada that perhaps we ought to try to classify all simple groups of sectional 2-rank ≤ 4, thereby determining the simple groups with a nonconnected Sylow 2-group as a corollary and thus disposing of the problem once and for all. However, the list of known simple groups of sectional 2-rank ≤ 4 is an extensive one, while only Janko's two groups J_2 and J_3 have nonconnected Sylow 2-groups. Obviously then, even if our approach were to be successful, it would not provide an efficient way of handling the nonconnectedness problem. Unfortunately, a simpler alternative did not seem to be available.

On a superficial level the strategy for proving the sectional 2-rank ≤ 4 theorem was clear. If one begins with a minimal counterexample G, then the nonsolvable composition factors of any proper subgroup of G likewise have Sylow 2-groups of sectional rank ≤ 4, but are of lower order than G, so are among the groups listed in the conclusion of the proposed theorem. On the basis of this loose internal information, the goal of the analysis would be to force a Sylow 2-subgroup S of G to be isomorphic to that of one of the target groups, in which case we would be able to invoke either one of our prior Sylow 2-group characterization theorems or a similar characterization by David Mason of the two remaining families $L_4(q)$ and $U_4(q)$, q odd, to show that no counterexample existed.

This may sound simple enough, but I had given little thought to how this forcing process was to be achieved. It quickly became evident that this problem constituted a very formidable challenge and, moreover, successful execution would be based almost entirely on fusion-theoretic and 2-local arguments with only a minimal role for signalizer functor theory. Although I made a modest contribution to the required 2-group analysis, the burden of the final 450-page sectional 2-rank ≤ 4 classification proof fell almost entirely on

Harada. For a time, this placed a strain on our relationship since he quite justifiably felt that he was doing most of the work. However, coming from a culture in which elders are accorded considerable respect, it was very difficult for him to express his complaints openly. In the end, our friendship weathered this trying period and Harada received the recognition he so richly deserved.

I doubt whether there was any group-theorist other than Harada capable of seeing the subtle case division the proof demanded and of carrying through the extremely delicate analysis necessary to pin down the structure of S in each case. The difficulties he encountered can be measured against a second perspective, for the proof violated an early maxim of Thompson: Whenever possible, avoid questions whose solutions require analysis of the structure of p-groups!

With the solution of the nonconnectedness problem, I felt that simple group theory was entering a new era, in which it would now be possible to consider much broader classification theorems. I had become so confident of the signalizer functor method's effectiveness that I even began trying to visualize the general features of a complete classification proof. Elimination of core obstruction in the centralizers of involutions would form the obvious first stage. In those cases that would ultimately lead to either groups of Lie type of odd characteristic, large degree alternating groups, or those sporadic groups that shared these "odd type" features, the second stage would presumably consist in forcing centralizers of involutions to approximate those in one of the target groups. [John Walter and I successfully tested these two steps of the classification program under very stringent hypotheses related to groups of Lie type of odd characteristic.] Now the stage would be set for verification of the "Brauer principle": Each target group would be uniquely characterized by the structure of the centralizers of its involutions.

If this program could be implemented in complete generality, it would mean that a minimal counterexample G to the full classification theorem would be a group of what I had previously termed *characteristic* 2-type — i.e., for any 2-local subgroup H of G,

$$C_H(O_2(H)) \leq O_2(H).$$

Among the known simple groups (of sectional 2-rank ≥ 5), only the groups of Lie type of characteristic 2 and a few sporadic groups have this property. Therefore to complete the classification of the finite simple groups, it would remain to prove that every such simple group of characteristic 2 type was isomorphic to one of the latter target groups.

Almost the entire analysis of N-groups had been concerned with groups of characteristic 2 type, and I was struck by the parallel between the major case division required for the classification of groups of odd type and that of Thompson's N-group theorem. Just as the low 2-rank case had required special treatment, so too did N-groups in which every 2-local subgroup has p-rank ≤ 2 for all odd primes p; and just as "generic" groups of odd type were being analyzed by studying centralizers of involutions, so, too, Thompson had analyzed general N-groups by studying centralizers of elements of odd prime order.

Surely this parallel could not be accidental, but must be intrinsic to the very nature of the classification problem. Therefore with Thompson's N-group analysis as model, I let my imagination run freely, picturing a procedure for classifying arbitrary simple groups of characteristic 2-type. First would come the low 2-local p-rank cases. Once these cases had been disposed of, I visualized the signalizer functor method again taking over, but now to eliminate "p'-core" obstruction in the centralizers of elements of odd prime order p (at that time, I was giving special emphasis to the case $p = 3$). Pursuing the odd type parallel, I anticipated the next step would consist of forcing the structure of centralizers of elements of order p to approximate those in one of the specified target groups, and finally one would try to establish Brauer-type characterizations of these groups by the structure of their centralizers of odd prime order. I even went so far as to test the plausibility of this last phase of the proof by having my first doctoral student at Rutgers, Robert Miller, derive a characterization of the groups $L_4(2^n)$, n even, by the structure of the centralizer of an element of order 3 ($\cong GL_3(2^n)$).

This was all obviously sheer speculation, supported more by my own enthusiasm than by any mathematical argument. Throughout I was using the solutions to particular classification problems to suggest the projected shape of the proof in the general case. Even within the odd type situation with which most of my work had been concentrated, I completely failed to foresee how difficult elimination of core obstruction in the centralizers of involutions would, in general, turn out to be, nor did I anticipate the more than 50 separate papers that would ultimately be required to verify the Brauer principle in all cases. I was on even shakier ground with groups of characteristic 2 type, for I was dependent on the limited local subgroup perspective of N-groups as sole guide for treating the general case and had no inkling of the several fundamental new techniques their analysis would ultimately require.

And what effect might all the as yet undiscovered simple groups have on my global strategy? So far as we then knew, there might still exist a great many undetected sporadic groups — perhaps even entire families of new simple groups. It took blind faith on my part to believe that none of these groups would have an internal structure that deviated sharply in its general features from that of any existing simple group, otherwise the techniques we had developed might be totally inadequate for analyzing the subgroup structure of a minimal counterexample to the full classification theorem.

Furthermore, as if this were not sufficient to give me pause, my classification strategy was being formulated without proper weight being given to two important developments of the late 1960s which at the time I had not adequately absorbed. First, Bender had succeeded in considerably simplifying portions of Thompson's local analysis in the odd order proof and was using his approach to initiate a powerful new technique for studying centralizers of involutions, which for a period vied with the signalizer functor method in effectiveness. Indeed, in time Bender was to produce dramatic simplifications of both the dihedral and abelian Sylow 2-group classification theorems. Unfortunately, his method seemed to work smoothly only in the presence of p-stable local subgroups, thus limiting its ultimate range of applicability.

Secondly, Fischer's investigations of groups generated by a conjugacy class of *transpositions* — i.e., a conjugacy class of involutions the product of any two members of which has order 1, 2, or 3 (conditions satisfied by the transpositions in symmetric groups) — had led not only to the discovery of the three sporadic groups that bear his name, but also to a powerful technique, partially geometric, partially group-theoretic, for analyzing problems of this general type.

Fischer, too, was at the Institute that group theory year, but though we frequently conversed, I was then unable to appreciate the significance of his achievements. My picture of Fischer is with an ever-present cigarette in his hand or mouth, drawing a sequence of dots on the blackboard by which with his deep geometric intuition he meant to convey the internal structure of a particular group. But lacking such geometric intuition, I was never able to fully grasp the connection.

In any event, unrestrained optimism protected me from my strategy's obvious shortcomings. For me, this program represented the synthesis of everything I had managed to learn about simple groups during the preceding decade and I strongly believed it would inevitably provide the basis for the future course of the classification proof, even if it were to take to the year 2000 to implement. When Alperin learned of my proposed plan, he suggested I present my ideas at his upcoming group theory conference at the University of Chicago in July 1972. The four lectures I gave there, in which I outlined a 16-step program for classifying the finite simple groups, have come to be what I consider the end of the "early years[1]."

Although the lectures were to influence the research directions of several of the younger group-theorists, they produced few converts at the time they were delivered, for my outline failed to provide a detailed prescription for carrying out any of the individual steps. The audience reacted with justifiable

[1]These lectures have been published in the appendix of my survey article "The classification of finite simple groups," *Bull. Amer. Math. Soc.* **1** (1979), 43–199.

skepticism, viewing the program as little more than unsubstantiated speculation. However, no one in that room, myself included, was prepared for the welter of results about simple groups that would come tumbling out over the next five years — new sporadic groups, new techniques, broad classification theorems — the work of many authors, but with Michael Aschbacher leading the field. By 1977, it had become clear to most finite group-theorists that at least in outline form the program I had envisaged in 1972 provided the underlying features of the full classification theorem that was taking shape before our eyes and which was to be completed within the space of just five more years. But all these later developments are a story for another time.