Reminiscences on the Life and Mathematics of J. Frank Adams

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Frank Adams was both my closest personal friend and my closest mathematical friend. I will say a little about his mathematical work here, but a fuller appreciation is being prepared for publication elsewhere. I will try to convey something of his style and of his feelings about mathematics, again letting him speak in his own words.

Adams was knowledgeable about many other fields, but topology was his love. While all of his work was at a very high level, two groups of early papers stand out particularly:

On the structure and applications of the Steenrod algebra (June 1957)
On the non-existence of elements of Hopf invariant one (April 1958)

Vector fields on spheres (October 1961)
On the groups $J(X)$—I (May, 1963)
II (September 1963)
III (November 1963)
IV (July 1965)

The dates given are the dates of submission; actually, according to $J(X)$—IV, much of the material in the $J(X)$ papers dates from the years 1960–1961.

The first two papers above were concerned with the Hopf-invariant-one problem. One way of motivating the problem is to ask the possible dimensions n of a real division algebra D. Given D, we obtain a map f from the unit sphere $S^{2n-1} \subset D \times D$ to the one-point compactification S^n of D by sending (x,y) to $x^{-1}y$ if $x \neq 0$ and to the point at infinity if $x = 0$. If we form the two-cell complex $X = S^n \cup_f e^{2n}$, we find that its cohomology is Z in dimensions n and $2n$ and that the cup square of the generator in dimension n is a generator in dimension $2n$. We say that f has Hopf invariant one. The homotopical problem asks what dimensions n support a map of spheres $f\colon S^{2n-1} \to S^n$ of Hopf invariant one. In view of the real, complex, quaternion, and Cayley numbers, $n = 1, 2, 4$, and 8 are possible. Adams proved that these are the only possibilities.

The first paper I mentioned can be viewed as a failed attempt to prove this result. All it obtained on the problem was that, if $n > 4$, one couldn't have Hopf-invariant-one maps for both n and $2n$. However, since the paper introduced what is now called the Adams spectral sequence, it can't be written off as a total loss. In fact, the Adams spectral sequence is the most important theoretical tool in stable homotopy theory, and its introduction marked the real starting point of this fundamental branch of algebraic topology.

The Adams spectral sequence converges from

$$E_2 = \mathrm{Ext}_A(H^*(X; Z_p), Z_p)$$

to the p-primary component of the stable homotopy groups of the space X, where A denotes the Steenrod algebra of stable operations in mod p cohomology. The connection with the Hopf-invariant-one problem is that the mod 2 cup square is a Steenrod operation, and this allows a translation of the problem into a stable one. Certain differentials in the Adams spectral sequence give decompositions of Steenrod operations into composites of secondary operations. In a two-cell complex X, there are no intermediate dimensions in the mod 2 cohomology, hence such a decomposition of the relevant Steenrod operation implies that the cup square of the integral generator in dimension n is zero mod 2.

In the second paper above, the Hopf-invariant-one problem was solved by means of an explicit decomposition of all of the relevant mod 2 Steenrod operations in terms of secondary operations.

My own 1964 doctoral thesis was motivated by the Adams spectral sequence, specifically by the following passage from Adams's 1960 Berkeley lecture notes:

> The groups E_2 are recursively computable up to any given dimension; what is left to one's intelligence is finding the differentials in the spectral sequence, and the group extensions at the end of it.
>
> This account would be perfectly satisfying to a mathematical logician: an algorithm is given for computing E_2; none is given for computing d_r. The practical mathematician, however, is forced to admit that the intelligence of mathematicians is an asset at least as reliable as their willingness to do large amounts of tedious mechanical work. The history of the subject shows, in fact, that whenever a chance has arisen to show that a differential d_r is non-zero, the experts have fallen on it with shouts of joy—"Here is an interesting phenomenon! Here is a chance to do some

Proof by K-Theory of the Non-Existence of Elements

We need some cohomology operations in K-theory.

First I will show that the exterior powers λ^i are defined as elements of $K(X)$. They are certainly defined on vector-bundles—over each point $x \in X$ you take the i^{th} exterior power of the fibre you find there; and we have

$$\lambda^k(\xi \oplus \eta) \cong \bigoplus_{i+j=k} \lambda^i(\xi)\otimes\lambda^j(\eta).$$

We use this formula in the usual way. We form the ring of formal power-series $K(X)[[t]]$, and inside it we have the group G of formal power-series

$$1 + x_1 t + x_2 t^2 + \cdots + x_i t^i + \cdots .$$

We define a function Λ from vector-bundles over X to G by

$$\Lambda(\xi) = 1 + \lambda^1(\xi)t + \lambda^2(\xi)t^2 + \cdots + \lambda^i(\xi)t^i + \cdots$$

and note that

$$\Lambda(\xi \otimes \eta) = \Lambda(\xi)\Lambda(\eta).$$

So Λ gives us a homomorphism $K(X) \to G$. The coefficients in $\Lambda(x)$ give us the exterior powers $\lambda^i(x)$.

The formal properties of the exterior powers are not very convenient, so we process them. Recall the symmetric function theorem, that

$$Z[x_1, x_2, \cdots, x_m]^{\Sigma_m} \cong Z[\sigma_1, \sigma_2, \cdots, \sigma_m]$$

where σ_i is the i^{th} elementary symmetric function of $x_1, x_2, \cdots, x_m$, that is,

$$(t + x_1)(t + x_2)\cdots(t + x_m) = t^m + \sigma_1 t^{m-1} + \cdots + \sigma_i t^{m-i} + \cdots + \sigma_m .$$

In particular, the power-sum

$$\pi_k = x_1^k + x_2^k + \cdots + x_m^k$$

is symmetric, and can be written as a unique polynomial

$$\pi_k = Q_k(\sigma_1, \sigma_2, \cdots, \sigma_k)$$

where Q_k does not depend on m provided $m \geqslant k$. We now take

$$\psi^k(x) = Q_k(\lambda^1(x), \lambda^2(x), \cdots, \lambda^k(x)).$$

For example,

$$\begin{aligned}\pi_2 &= x_1^2 + x_2^2 + \cdots + x_m^2\\ &= (x_1 + x_2 + \cdots + x_m)^2 - 2(x_1x_2 + x_1x_3 + \cdots)\\ &= \sigma_1^2 - 2\sigma_2,\end{aligned}$$

so

$$\begin{aligned}\psi^2(x) &= (\lambda^1(x))^2 - 2\lambda^2(x)\\ &= x^2 - 2\lambda^2(x) \qquad \text{(any } x \in K(X)).\end{aligned}$$

The properties of the operations ψ^k include:

nice, clean research!"—and they have solved the problem in short order.

On the other hand, the calculation of *Ext* groups is necessary not only for this spectral sequence, but also for the study of cohomology operations of the n^{th} kind: each such group can be calculated by a large amount of tedious mechanical work: but the process finds few people willing to take it on.

That was what I took on in my thesis. But my calculations in fact forced some calculations of differentials, and those calculations did not all agree with the ones tabulated by Adams in his cited lecture notes. I wrote him on February 23, 1964, pointing out his mistakes. I hasten to add that mistakes of any sort were most unusual in Frank's work. That marked the beginning of our friendship and the start of a correspondence which averaged one or two letters a month in each direction over the last twenty-five years, interrupted only by his frequent visits to Chicago and my visits to Cambridge.

Frank was the most competitive man I have ever met. Let me give one example. In the spring of 1971 my younger son was 2½ years old, the age of language acquisition and thus of most accurate memory. One day Frank and he were playing the card game Concentration on our living room floor. My wife said something to Frank, and he snapped back "Be quiet, I'm concentrating!"

In fact, by then he had mellowed. He was far more intense in earlier years. In his Spring 1960 Berkeley notes, he described some work in progress on the vector-fields-on-spheres problem, which asks for the maximum number of linearly independent vector fields on S^n for each n. Hirosi Toda, in Japan, was also working on the problem and had some partial results. With this spur, Adams had polished off the problem completely by October 1960. Moreover, his methods were totally different from those he had been working on in the spring. Then, he was thinking in terms of

of Hopf Invariant One

J. F. Adams

(i) $\psi^k f^* = f^* \psi^k$

(ii) $\psi^k(x + y) = \psi^k(x) + \psi^k(y)$

(iii) $\psi^k(xy) = \psi^k(x) \cdot \psi^k(y)$

(iv) $\psi^k(\psi^\ell(x)) = \psi^{k\ell}(x) = \psi^\ell(\psi^k(x))$

(v) If $x \in \tilde{K}(S^2)$ then $\psi^k(x) = kx$; hence by products, if $x \in \tilde{K}(S^{2n})$ then $\psi^k(x) = k^n x$.

See JFA, Vector fields on spheres, *Annals of Math.* 75 (1962), 603–632.

Now we consider a cell-complex $X = S^{2n} \cup_f e^{4n}$. For any such X (with $n > 0$) we have $H^{2n}(X;Z) = Z$, $H^{4n}(X;Z) = Z$; we can take generators x, y; we then have $x^2 = \lambda x$ for some $\lambda \in Z$; λ is the *Hopf invariant* of f. For example, if $f = \eta$: $S^3 \to S^2$ we get $X = S^2 \cup_\eta e^4 = CP^2$ and $\lambda = 1$; similarly for $n = 2, 4$. If $2n$ were replaced by an odd integer we should have $x \cdot x = -x \cdot x$ and $\lambda = 0$. We can always construct an example with $\lambda = 2$ (take $S^{2n} \times S^{2n} = (S^{2n} \vee S^{2n}) \cup e^{4n}$ and identify the 2 copies of S^{2n} in $S^{2n} \vee S^{2n}$). It used to be a problem whether we could find more cases with $\lambda = 1$, i.e., more maps of Hopf invariant one. We will see that this is impossible.

From the Atiyah-Hirzebruch spectral sequence we have

$$\tilde{K}(X) \cong Z \otimes Z$$

on two generators ξ, η of filtrations $2n, 4n$; we shall have $\xi^2 = \lambda\eta$ where λ is the Hopf invariant we seek. We can assume

$$\psi^2\xi = 2^n\xi + \mu\eta, \ \psi^2\eta = 2^{2n}\eta$$
$$\psi^3\xi = 3^n\xi + \nu\eta, \ \psi^3\eta = 3^{2n}\eta$$

for some scalars $\mu, \nu \in Z$. Working out the equation

$$\psi^2\psi^3\xi = \psi^3\psi^2\xi,$$

we find

$$3^n(3^n - 1)\mu = 2^n(2^n - 1)\nu.$$

Consider for example the case $n = 8$, which is the first really interesting one. On the right we have a factor 2^8. On the left we have

$$\begin{aligned} 3^8 - 1 &= (3^4 - 1)(3^4 + 1) \\ &= 80 \cdot 82 \\ &= 2^5 \cdot \text{odd}. \end{aligned}$$

Therefore μ is divisible by 2^3 at least; in particular, μ is even. So

$$\psi^2\xi = 2^8\xi + \mu\eta \text{ is even (equal to } 2\zeta\text{, say).}$$

We have $\psi^2\xi = \xi^2 - 2\lambda^2(\xi)$, so ξ^2 is even (equal to $2(\zeta + \lambda^2(\xi))$). Therefore λ is even. This completes the proof.

Elementary number theory shows that the same argument works for all $n \neq 1, 2, 4$.

The original source is JFA & MFA, *Quart. Jour. Math.* 17 (1966), 31–38.

ordinary cohomology, higher order cohomology operations, and differentials in the Adams spectral sequence. As he wrote in the published account, "The author's work on this topic may be left in decent obscurity, like the bottom nine-tenths of an iceberg." In fact, his solution of the problem was obtained by the introduction and exploitation of what are now called the Adams operations in topological K-theory $K(X)$.

Recall that $K(X)$ is the Grothendieck ring determined by the semi-ring of isomorphism classes of vector bundles over X. The vector-fields problem is closely related to the study of the groups $J(X)$. These are quotients of the groups $K(X)$ obtained by classifying vector bundles in terms of fiber homotopy equivalence rather than bundle equivalence. The first of the $J(X)$ papers contained a remarkable conjecture—now called the Adams conjecture—and proved it in special cases. It gave an upper bound for $J(X)$ in terms of the Adams operations. That is, it asserted that certain elements of $K(X)$ specified in terms of Adams operations were always in the kernel of the natural homomorphism $K(X) \to J(X)$. The remaining $J(X)$ papers made clear that the Adams conjecture was of fundamental importance in algebraic topology.

The Adams conjecture was later proven by Sullivan and Quillen, and their proofs led to a cornucopia of new mathematics. Sullivan's proof led him to the now ubiquitously used theory of localization and completion of topological spaces. Quillen's proof led him inexorably to the now standard definition of the higher algebraic K-groups of rings.

Rather than say more about Adams's mathematics, I will let him give an example of his style of exposition. In going over his papers in England, I found his lecture notes on the definitive proof, using the Adams operations in K-theory, of the non-existence of elements of Hopf invariant one. This proof is due to Adams and Atiyah. The lecture notes assume a little

knowledge of the relationship between ordinary cohomology and K-theory, as given by the Atiyah-Hirzebruch spectral sequence, but the lecture was clearly intended to be accessible to graduate students.

It may be objected that the algebra at the end of the proof was left to the reader. That reflects Adams's considered position on the relation between topology and algebra in his work. As he once wrote me:

> I am usually interested in writing papers in which one reduces topological problems to algebra. From that point of view, one tends to accept algebra as a subject under our control; one writes algebra only as required.

In fact, Adams always aimed at geodesic solutions to problems, developing only such theory and doing only such calculations as were essential to the main line of argument. I myself am more Bourbakian, and his attitude is amusingly conveyed by the following quote from a letter he wrote me in 1984, when we were collaborating on a paper:

> It is not like you, Peter, to miss the correct level of generality. Riddle: does JFA ever miss the correct level of generality? Answer: if his wife and daughters stayed away he would miss them, but as for the correct level of generality, he hardly seems to feel the lack of it.

Frank had very forceful opinions on the writing of mathematics, and he took it upon himself to try to keep the literature honest. This came out particularly in Section 6 of a crusty paper in the proceedings of the 1982 Aarhus conference on algebraic topology. It included the following quotes. I must admit that the attitude expressed is one that I share. In fact, some of the examples of sloppy mathematics that led to the diatribe were supplied by me in correspondence, in an area that I knew well and that Frank was learning.

> If you catch anyone writing a sentence like that, make a note that you do not trust his critical faculties.
>
> Linguistically, notation with very strong associations, which are totally different from its declared logical meaning, is misleading notation. I suggest we should use misleading notation only when we wish to mislead, for example, on April 1st. Since mathematicians do not normally intend to deceive, misleading notation is especially dangerous to authors capable of self-deception.
>
> . . . I am moved to preach a sermon on this subject. So, if such of my friends as have favorite pieces of minor sloppiness will please put them down and walk quietly away from them, I will begin.
>
> I earnestly desire that people should not copy out of previous papers without pausing to ask whether the passages to be copied make sense. And when we write a sentence which implies that one checks A and B, then we shall take scrap paper and check A and B—from the definitions. And for those of us who have the care of graduate students, I recommend that we give them critical faculties first and their PhD's afterwards. Here ends my sermon.

He wrote even more effectively about such matters in private correspondence as can be seen in the preceding Memorial Address. His letters were always a delight, although his handwriting required careful deciphering. Imagine the pleasure of receiving the following piece of doggerel in the mail. It concerns another aspect of Frank's role in policing the topological literature.

The Publication System: A Jaundiced View

This is the paper X wrote.

This is the editor, all distraught,
Who tore his hair at the horrible thought
Of printing the paper X wrote.

This is the friend whose help was sought
by E, the editor, all distraught,
Who tore his hair and groaned at the thought
Of the horrible paper X wrote.

This is the proof, all shiny and new,
Of 2.1 and 2.2
Conceived by F, whose help was sought
By E, the editor, all distraught,
Who tore his hair and groaned at the thought
Of the terrible paper X wrote.

This is the Referee's Report
Which says SUCH THINGS ARE BETTER SHORT
And gives the proof, all shiny and new,
Of 2.1 and 2.2
Proposed by F, whose help was sought
By E, the editor, all distraught,
Who tore his hair and groaned at the thought
Of the odious paper X wrote.

This Covering Note pretends to be
Detached about the referee.
("He doesn't tell you how to fix
The proof of Theorem 2.6.")

> *It quotes the Referee's Report*
> *Which says Such Things are Better Short*
> *And gives the proof, all shiny and new*
> *Of 2.1 and 2.2*
> *Proposed by F, whose help was sought*
> *By E, the editor, all distraught,*
> *Who tore his hair and groaned at the thought*
> *Of the pitiful paper X wrote.*

"But we are sure it can be mended;
If wholly changed it could be splendid."
Typing on a new machine,
F answers for his magazine.

Signed, $E_1 = F_2 = X_3$.

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