Open letter from R. W. Thomason

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Dear Colleague,

I am sending out about a hundred copies of this open letter to squelch uninformed speculation and certain baseless rumors as to the motives of my move to Paris. The positive motives of this move should not be so difficult to understand. Paris is at the same time the most mathematically active center in the world, if rivalled by Princeton, Boston, and perhaps Moscow in this regard, and also a great center of general culture, if somewhat surpassed in this by New York. I find it a most enjoyable place to live, an opinion which seems widely shared. I have found that the facilities and support here are much better than at my previous university, Johns Hopkins, while my salary is only very slightly less. In fact, my disposable income after job-related extra housing and travel expenses has increased. On the other hand, while I have always had good relations with my former colleagues in the math department at Hopkins, I found the conseqences of the very small size (16) and isolation of this department to be very discouraging. What's more, it's located in a cultural wasteland with a high crime rate, having all the danger of New York but none of the excitement. The decison to part from Hopkins is clear enough. But some people have wondered why I, whose career has not been totally without distinction, would accept the disadvantage of working in a second language rather than seek a more desirable position in another American university. The answer to this is partially that I find the balance in Paris more attractive than either of the two different extremes of New York and Boston. But another consideration is my problems with a small but once powerful sector of the American mathematical community, the past ravages of which continue to cause me difficulties. Not all aspects of this story are well-known. <u>While many people have heard that I was without a regular job for three years, most tell me they have no idea why Quillen would have wanted to leave MIT, and few know that it was I who first told Haynes Miller how to prove the p-group case of the Sullivan conjecture.</u> I will give some details of my story below. I here add the obvious remark that the opinions herein expressed are my own, and engage in no way any institution with which I am or have been affiliated.

As is generally known, the problems began with my relation to Spencer Bloch when I arrived at the University of Chicago in the late summer of 1979. As background to this the reader should know that previously I had spent two years at MIT as a Moore Instructor, and had just found in the spring of 1979 my first proof that algebraic K-theory with the Bott element inverted satisfies etale cohomological descent.

My postion at Chicago was a temporary three-year non-tenure-track job for people two years out from graduate school, that of a "L. E. Dickson Assistant Professor". This appointment had been pushed through largely by Peter May because of my work on his specialty at that time, infinite loop space theory. Two years previously, when I was still a graduate student at Princeton, we wrote a joint paper proving the uniqueness of infinite loop space machines, to which I contributed more of the ideas than would probably be assumed by the general public at that time. I thought then that the colaboration would be a good idea anyway since May could place the joint paper in a more prestigious journal than I could alone, and that I'd have an advocate if I applied to Chicago. When I did apply there, he came through. Bloch had been the most opposed to my appointment. In departmental meetings he had criticized severely a talk I had given in January 1979 at Chicago announcing progress toward etale cohomological descent (a talk which he had not been able to attend). Bloch said he wanted to get Jan Stienstra instead (although Steinstra wasn't competing for a position at exactly the same level). In the end, the topologists won out and I was appointed over Bloch's objections. In fact, he got Stienstra as a Dickson Instructor anyway. When I arrived at Chicago, I knew that Bloch had been opposed to me, but I figured that the decision having been made, this whole story was over and we'd get along OK.

However, after being there a couple of months, I had begun to notice that I was the butt of Bloch's railleries even a little more frequently than other people. It was tame stuff: he joked about my then long hair, or the fact that I ate hardly anything at dinners after seminars (in fact, because I am a sufficiently brittle diabetic, my meal times and food intake need to be rigidly controlled). At the time, this didn't seem particularly menacing since Bloch persistently ridicules everyone not in a position to further his career. A bit more disturbing was that the topologists at Chicago showed themselves more and more skeptical of my work in K-theory. This went beyond worries about the bugs in the proof of descent (there were two or three, each inducing uncertainty for a week or two), or about the collapse of my calculation of the Bott-periodic algebraic K-theory of algebraically closed fields (which didn't come back until spring 1981). They seemed to have tacit doubts about the very idea of a connection between algebraic K-theory and etale cohomology, and whether this would be a fruitful project to pursue. As May told me during one of the weeks of uncertainty, "You could drop this; you still have two years to work on something good now." There were a number of little hints like this, but nothing too explicit. Often people said "Well, you'll have to convince the experts". As to the expert they had especially in mind, when I gave a course on the descent theorem Bloch came a few times, then abandoned it, telling others that "I can't find any meat in it". Of course that is his right.

But one day, in the course of a discussion with me which became a bit warm, he blurted out "It's not a sign of competence to think there is a connection between etale cohomology and algebraic K-theory". This statement was startling. It would be astounding, but conceivable, that Bloch could then think that Dwyer, Friedlander, Lichtenbaum, Soulé, and Quillen were incompetent. But it was inconveivable that Bloch regarded himself as incompetent, and there was a contemporary Bloch preprint making many speculations (but no results) about possible connections between K-theory and etale cohomology. It was clear that Bloch did not think his statement to be true. Worse, although he made this wild statement to my face in a heated exchange, I realized he was most probably making tamer false statements in a more calm and convincing tone to the other professors at Chicago. Bitter about my appointment over his objections, and with an immense appetite for influence, he had given signs of resenting me enough to slander me and my work. This would explain the general and persistent doubts at Chicago about the value of my work on K-theory and the repeated warnings that I needed to "convince the experts" that it was worth pursuing. It's true that Bloch could not have gotten away with, nor would he have attempted, saying to an expert in the field something that would be obviously false to such an expert. However, the other professors at Chicago, and most professors on hiring committees everywhere, are not experts on K-theory, and they will not be too suspicious about whatever Bloch tells them. (May, for example was a great believer in hierarchy, and had criticized Bill Browder for assigning me as referee for Waldhausen's paper "Algebraic K-theory of generalized free products" because I was just a graduate student at that time. The fact that I was one of the only four people in the world, including the author, that had at that time real expertise in the subject didn't seem to count much for him.) I did have a proof of my result, but a proof that was long and technically difficult and that I hadn't been able to get anyone else to examine. In any case, I have never seen the members of an appointments committee read the preprints of candidates for a job and check the proofs. They always rely on expert opinion. At that time the recognized authorities in algebraic K-theory were Quillen, who doesn't like to waste his time on politics, and Bloch, who loves to play these games; thus Bloch is more likely to work hard to exert influence on public opinion, and in any case the natural conservatism of academia means I would lose a split opinion. Worse, if I stayed at Chicago for my full term, or even a couple of years, Bloch becomes <u>the</u> expert on my work: there will be the chorus "But Bloch says he isn't any good and Thomason was there working with Bloch for years so he should know." I could have tried complaining about Bloch, but this didn't seem very likely to work. Try to imagine what your reaction would be if a young unproven assistant professor, three years out of graduate school, comes to tell you that your internationally famous colleague, a full professor, is lying about him. By the time Bloch had blurted out his revealing remark, it was late January and past the deadlines to apply for another job for fall 1980. I considered abandoning the descent theorem and working only in strict homotopy theory, but rejected this option for having a normal academic life since I consider the descent theorem too good to give up. I thought about suing Bloch, but this didn't seem too likely to work. If I stayed at Chicago another year, Bloch's authority on my case would be too well established and he'd be able to keep me from getting another position.

The only exit I could see from this trap was just to quit Chicago, be unemployed for a year, and then try to get a job the following year. Before taking such a radical and dangerous step, I thought it was prudent to wait and think about it for awhile. It was clear there was no point in asking anyone else for advice, for they would instinctively reject such an unprecedented ploy without bothering to examine the facts of the situation, which were also relatively incredible, and then would be personally angry at me for disregarding their advice if I quit anyway. Eating a lot of spaghetti, I starting hoarding money to tide me over a year with little or no income. I calculated that by the end of the academic year I would have enought to get by for a year, and so could continue my work. Naturally, the whole spring of 1980 my subjective state was anxiety, even terror. However, there were some glimmers of hope. May told me there was a rumor that MIT might make me an offer that spring: if this worked out I could leave Chicago without any fuss. By April, this particular rumor was discredited. The choice was down to either the very dangerous move of abruptly quitting Chicago, or letting myself be passed off as incompetent and having my work quietly smothered, or getting out of mathematics entirely. I realized that the vast majority of those mathematicians who didn't know me personally would react with scepticism and mild hostility if I quit Chicago. But during my years at Princeton and MIT as graduate student and Moore Instructor I had built up a network of friends that I expected would at least hear me out and judge my work on it merits. Naturally, they wouldn't want to go into open combat with their colleague Bloch because I say he is lying. They wouldn't want to hear about such a thing. I didn't expect they'd attack on my behalf, but I did expect that some would help me to find another position later while most would maintain a risk-free attitude of benevolent neutrality. As for the mass of mathematicians who were not personal friends, I thought that the shock would die out in several years and that upon reflection any reasonable person would conclude that no one would renonce all security and at least a year's worth of income by quitting unless they were really sure something was very very wrong. I expected that this step would have to be taken more seriously than mere griping. It's true that it wasn't very convenient for the math department at the University of Chicago, but it would be even less comfortable for me. However, I felt it was going to be necessary.

I drove from Chicago to spend the summer of 1980 in Cambridge, were I had personal friends and where I intended to spend the projected fateful year of unemployment in 1980-81. As planned, I waited until I had spent a week away from Chicago in a more tranquil environment before making the final irreversible decison to quit . At the end of the week, I was still sure I had to do it. It was the beginning of June. I wrote a short letter of resignation, effective at the start of the 1980-81 academic year. I put it in the mail and immediately called May to tell him I was gone.

Despite my expectation that it was basically hopeless, I made some attempts to find a job for the coming year. To my surprise, there turned out to be a few vague possibilities. Then a few days after I told Mike Artin I needed to find a new job, I was astonished when Quillen showed up and pulled me out of the library at MIT to tell me that as " there's always a little slush in the budget, from visitors that don't show up and things like that", he had arranged that I would have an irregular nine-month appointment at MIT for 1980-81 as a "lecturer" teaching recitation sections of calculus and differential equations. As to the future, Quillen told me that of course he couldn't speak for the department or the hiring committee, but that he would press very hard to have me appointed as a regular assistant professor starting the following year. He let it be clearly understood that he expected this to succeed. To this he added the encouraging remark that " You've advanced the theory beyond what I was able to do." This seemed to be the best possible situation, far better than what I had hoped for. I had known I could count on Quillen: not only had he been always friendly with me, but it was also strongly in his interest to promote my career. I and Waldhausen were the two mathematicians in the with the deepest understanding of Quillen's pioneering work on higher algebraic K-theory, the topological methods of which are difficult to understand for those limited to a more classically algebraic framework. (In particular this is true of Bloch, the close examination of whose work on K-theory shows it had not then, and still has but very rarely, gone beyond the level of K_2 or the "symbolic part" or higher Milnor K-groups, which can still be handled by algebraic techniques.) Quillen couldn't really afford to sacrifice me, one of the main developers of the higher algebraic K-theory, without a severe loss of influence of his ideas. And Quillen was MIT's only Fields medalist, so you would think they would want to keep him. Not knowing much about slush, I had figured he wouldn't be able to help me for 80-81, but that he'd bail me out the following year. Thus I now thought that I was safe for three or four years, after which time the scandal caused by my leaving Chicago would have dissipated, and I should be able to get a permanent position. I was sufficiently encouraged by then that I began to look for an apartment so that I could stop sleeping in various dark corners of MIT, occasionally awkakened by my astonished former students.

Taciturn by nature in any case, I tended not to talk much about why I had quit Chicago, limiting my explanations to a vague "I didn't get along with Bloch, who was very hostile" since no one would be to happy to hear, nor disposed to believe, the full story. I had given Bill Browder such a vague description of the affair when I phoned him in June to ask if he would know of any one-year jobs available anywhere. As always, he was very nice. Like most everyone else that I approached, he started instantly to object (in a friendly way, without the screaming or menacing that I would encounter later from others) that quitting Chicago was a bad move and would cause me many problems. Only Browder had the insight to stop his flow of advice-based-on-ten-seconds-of-thought with the remark "But you must of thought of this yourself," acknowledging ever so little my six months of anxious reflection. Confident of my relations with them, I thought that I should offer some more detailed explanations to my friends from Princeton when I could see them face-to-face. As it was summer and many people were travelling, I waited until September to pay a visit to New Jersey. I had no sooner entered Fine Hall and ascended to the main third floor when I encountered Wu-Chung Hsiang, who without pausing to greet me began screaming: "You're crazy! You did a stupid thing! Your career is finished!" I had always regarded the expression "foaming at the mouth" as a meaningless cliché, and watched with fascination as little bubbling masses of salviva collected at the corners of his lips. Hsiang continued to shout, not leaving me the time to say anything. After several minutes he paused long enough that I could say, "Well, perhaps you should hear my side of the story." He told me that he didn't need to, so I wrote off Hsiang. I was a bit shaken by this reception; I had known Hsiang for five years. When I ran into them, most other professors I knew there seemed relatively indifferent, which I regarded as a good sign. But the most important for me at Princeton was my former thesis advisor, John Moore. He asked me out to dinner, always a bit awkward for me because of my diet. He didn't seem too interested in the Chicago business, and didn't ask me any questions about it when I made a brief allusion to it. Still somewhat upset by Hsiang's reaction and considering Moore's general love of quiet, I didn't insist on the topic. In retrospect, this was a mistake: I wish now Ihad. As it was, Moore did not openly reproach me. In general, things didn't seem too bad for me at that time.

However, over the next few months, I noticed little signs of spreading hostility and a hardening attitude that a renagade such as me should be annihilated. One day, while I was quietly working in my office, Daniel Kan, whom I had regarded as a friend up to this moment, flung open my door and, with face tense and voice straining with rage, growled out, "Well, maybe you can hope to get a job in a junior college somewhere···" but that I was finished as far as research universities go. At least Kan had the courage to tell me this to my face. I had the distinct impression that a considerable number of my other former friends were similarly determined that I should be driven from mathematics (or at least from those academic institutions that identify mathematics to themselves). One shouldn't especially blame Kan just because he was more frank. Less frank was the "someone" about whom I was warned that "Someone in Princeton is really mad at you" by Fred Cohen, with a similar vague statement made independently by Joe Neisendorfer, two mathematicians who remained friendly. Because they knew not to take Hsiang's rantings too seriously, and because they were most closely linked to Moore, I made the inference that they were talking about my former thesis advisor. Moore had close links to Frank Peterson, who had a lot of political influence at MIT.

I was not offered a chance to be associated to someone else's NSF grant, and so applied on my own as sole PI. Late in that spring of 81 I was quite pleasantly surprised to get a two year grant. Given one recognized authority's very negative opinion about my work and the pack of banshees at Princeton and MITscreaming that I should be driven from mathematics, this serves as a notable testimony to the general fairness and efficacity of the NSF's evaluation normalization proceedures. However, I would have trouble living on two-ninths of zero, so I needed to find a job somewhere.

In the winter of 80-81, I had applied to a spectrum of research universities on the coasts, and expected that I probably would be able to stay at MIT thanks to Quillen. However, by May 1981, I had received no offers. Quillen was very surprised he had not been able to convince MIT to give me an assistant professorship, and I would think pretty pissed off. In any case he was on his way out, preparing to go visit Bonn for 1981-82 and Oxford for 1982-83, where he would move a few years later. A rather embarrassed MIT math department offered to renew my lectureship for another nine months rather than throw me on the street.

Still naive enough to believe that my work might possibly influence my career in such conditions, I went on to calculate the Bott-periodic algebraic K-theory of strict hensel local rings in late spring of 1981, completing the proof that inverting the Bott element turned algebraic K-theory into etale topological K-theory, and began to explore the consequences of this for Riemann-Roch problems. I wasn't in much of a mood to enjoy this triumph however, filled with well-founded dread for the future and with the feelings of one betrayed by long-time close associates.

The year 1981-82 passed without many changes in the situation, except that I reduced all personal expenses to a minimum to built up a war chest to continue my work after I became unemployed. I applied to a spectrum of good research universities plus nearly every university in the Boston, New York, Philadelphia, Los Angeles, or San Francisco areas. Several interviewers showed some initial interest in me, but nothing came of all this. While a few people were violently opposed to my getting a job in mathematics, I did also have supporters, some with influence. Among others, Quillen, Friedlander, Soulé, and Ronnie Lee wrote very strong letters for me: I arranged to get copies to check. But as in the previous year, I got no regular offers. Grothendieck has interpreted this as being due to the fact that my work is too Grothendieckian. It is doubtless true that my "abstract" style and the influence of Grothendieck were negative factors on the job market then. Other people with a similar style also had some job difficulties at the time. But none went two sucessive years with strong letters and zero offers, an event which cannot be fully explained by the unpopularity of an "abstract style". It appears that a handful of people were quite successful in blackballing me, most departments not liking controversial "trouble-makers". Moore's favorite image of a math department as a "club of gentlemen" is not totally inapt for many departments. (But I think the ethics of the situations is different since it is not the club that pays dues to its gentlemen.) In any case, it seemed that the people screaming that I should never get a job in mathematics were not making just idle threats. Soulé told me the whole situation was disgusting, that "people aren't looking at your work", and suggested vaguely that I come to Europe. In the end, I got two one-year offers from the Institute for Advanced Study in Princeton and the newly founded MSRI in Berkeley. I chose to go to Princeton, hardly a place where everyone would be friendly to me, because it would be cheaper to move back to Boston the following year if I got no offers, as looked very likely.

Artin was nice enough to help me arrange a phantom extension of my connection to MIT so that my newly renewed NSF grant could be made through MIT. Apparently, NSF grantees can usually find regular positions and don't have such problems of where to channel the grant.

At Princeton, I avoided the University as much as possible, given the attitude of Hsiang and especially Moore. On the other hand, at the Institute itself, everyone was very nice. Borel, and Langlands, who I learned had himself survived several years of very uncertain employment when he resigned his job as Associate Professor at Princeton, were especially supportive. The reader may understand that nevertheless I was in a very anxious mood most of the time, and often couldn't sleep. Still, during this period I discovered and wrote up my second simpler homological induction proof of etale cohomological descent, proved Grothendieck's absolute cohomological purity conjecture rationally, and did my joint work with Gillet on the K-theory of strict local hensel rings. I also made an important suggestion to Haynes Miller, another former Moore student.

Miller was doing his since celebrated work on the Sullivan conjecture, then working on its weak form that any map from any suspension of the classifying space BG of a finite group G to a connected finite CW complex is null-homotopic. This was shortly after Gunnar Carlsson's completion of the proof of the analogous Segal conjecture on the stable cohomotopy of BG. The Segal conjecture had been the most active center of research in algebraic topology in the late 70's and early 80's. The case $G = \mathbb{Z}/2$ had been proved by W. H. Lin, and then the case of $\mathbb{Z}/p$ for p an odd prime by J. H. C. Gunawardena. The case of products of $\mathbb{Z}/2$'s was then done by Carlsson, and the case of products of $\mathbb{Z}/p$'s for p odd by J. Frank Adams, Gunawardena, and Miller; while McClure and May showed that the finite group case would follow from the p-groups case. Carlsson finished off the problem with a difficult proof that the case of products of $\mathbb{Z}/p$'s implied the p-group case, and hence the theorem in general by McClure-May. This acheivement was generally regarded as the key factor in Princeton's offer to him of a post of professor.

In the spring of 1983, while we were both at the Institue, Miller gave a seminar talk on his work on the Sullivan conjecture. He had succeeded in proving the $\mathbb{Z}/p$ case by a complicated and clever calculation using unstable Adams spectral sequences, roughly parallel to arguments in the corresponding case of the Segal conjecture. At the end of his talk, he mentioned that he was trying to understand the case of a product of two $\mathbb{Z}/p$'s, which he thought he should be able to do, and would then proceed to arbitrary finite products, obviously following the analogy with the Segal conjecture. Immediately after Miller's talk, I went up to show him how to make several steps he expected to be very difficult in his program, explaining to him how the p-group case of the Sullivan conjecture followed from the $\mathbb{Z}/p$ case he had done.

The argument I gave him was as follows. Instead of proving for the space of based maps that $\mathrm{Map}.(BG, X)$ is contractible for X a finite CW complex, we seek to prove the equivalent statement for the unbased maps that $\mathrm{Map}(BG, X)$ is homtopy equivalent to X. We rewrite this statement as saying that $\mathrm{Map}(EG, X)^G$ is homotopy equivalent to X for any finite CW complex with trivial G action. Thinking of this mapping space as the cohomology of G with coefficients in X, $\mathbb{H}(G, X)$, we look at the analog of the Lyndon-Hochschild-Serre spectral sequence to deduce the result for G once it is known for a normal subgroup N and the quotient G/N. That is, we note that the homotopy type of $\mathrm{Map}(EG, X)^G$ doesn't depend on the choice of free acyclic G-space EG. Thus we have homotopy equivalences:

$$\mathrm{Map}(EG, X)^G \sim \mathrm{Map}(E(G/N) \times EG, X)^G \sim \mathrm{Map}(EG/N, \mathrm{Map}(EG, X)^N)^{G/N},$$

or more suggestively,

$$\mathbb{H}(G, X) \sim \mathbb{H}(G/N, \mathbb{H}(N, X)).$$

Thus, using also the fact that $\mathrm{Map}(EG/N, \;)^{G/N}$ preserves homotopy equivalences of G/N-spaces, we can deduce the conjecture for G once we know it for N and G/N, as we have then a chain of equivalences:

$$\mathbb{H}(G, X) \sim \mathbb{H}(G/N, \mathbb{H}(N, X)) \sim \mathbb{H}(G/N, X) \sim X.$$

For G a p-group, there is always an $N = \mathbb{Z}/p$ in the center, hence normal. Miller's result applies to this N, and one deduces the general case of a p-group by the obvious induction on the number of elements. I added the remark that one could adapt the usual transfer argument as in May-McClure to deduce the result for a finite group G from the result for p-groups in the case where X was some good kind of H-space, to permit the taking of sums of maps into X. I didn't see how to do the case of non-p-groups G for general X.

Miller seemed a bit stunned by all this, and naturally disappointed that he hadn't done it. Perhaps he was also a bit relieved, since he had expected these steps to take very hard work, judging them by the analogy with the Segal conjecture. He asked me to repeat my argument, which I did. I told him it was related to things I knew very well from my work on etale cohomological descent for algebraic K-theory, the first proof of which proceeded by proving descent for a $\mathbb{Z}/p$ Galois extension and then deducing it for a general Galois extension by an argument similar to what I had just showed him. I said that his work on the critical $G=\mathbb{Z}/p$ case was deeper. Then, the few people left in the room went off to lunch. A few days later, Miller asked me at tea to repeat a few details of the proof, which I did.

Fairly quickly, Haynes came up with an argument inspired by Quillen's analysis of the homotopy type of the poset of p-subgroups of a finite group to replace the transfer argument, and starting giving talks on the Sullivan conjecture for general finite groups. In his lecture in the Princeton University Thursday topology seminar, he did mention that the step to go from $\mathbb{Z}/p$ to p-groups was due to me. Naturally, I didn't go to his detailed talks in the more intimate Monday homotopy theory seminar directed by Moore. However, after this, Haynes' attitude toward me began to change slightly. Although he was always sympathetic about my third consecutive year on the job market with no evident prospects, he began to tease me about my waiting around for a phone call. It seemed to me that I made him slightly nervous, but I admit this interpretation was just my subjective impression.

When Miller published his announcement in the July 1983 Bulletin of the AMS, space limitations did not permit detailed acknowledgments, and anyway he preferred to use another argument (his Thm. 3.2) to pass from $\mathbb{Z}/p$ to p-groups, different from mine, and which permitted him at the same time to pass to general finite groups. However, when the final version of his paper, dedicated to Moore, was published in the Annals of Math 120(1984), this argument had fallen apart. The passage from $\mathbb{Z}/p$ to locally finite groups G is made in §9 of this paper, the ideas of which Miller says in three places are due to Mike Hopkins. (See pp. 41, 42, 79). I agree that this should be true for some of the ideas, especially those relating to the passage from p-groups to locally finite groups. The argument given is more general than and differs in detail from, but is visibly related to and developed out of that which I had suggested to Haynes. As to giving credit to me, I am mentioned once, in the long list of thanks on page 42 where one finds the clause thanking "···; Bob Thomason, for a comment about the material of Section 9, and for insisting that I keep Quillen's book [36] on my desk;···". You could say that this is strictly speaking correct, and even overgenerous since I had little to do with making Haynes look at Quillen's "Homotopical Algebra". However, I thought that as thanks this was a bit thin, even verging on theft. I did give the first proof of one of the cases of the theorem, and if one judges by the perceived analolgy of the Sullivan and Segal conjectures, this p-group case would be a relatively big deal. In his long list of acknowledgments, Haynes does attribute many specific results to other people, and it would not have been stylistically inconsistent to have done the same for me.

My subjective impression was that Miller seemed to feel ashamed about his rather meager acknowledgment of my contribution, as if he regretted having to do it under some kind of constraint. One can objectively remark that Moore's blessing was very important for Miller's career at that point, for arranging a mathematical media blitz for his new result, and without doubt for his eventual position of professor at MIT. Given the climate at that time, Miller may have sensed that Moore wouldn't be as pleased by any result that owed too much to me, and that it would be better not to be too detailed where I was concerned.

For me this was a new level of pariahdom. That I couldn't find a regular position was bad enough. But now people I talked to could be pressured and made to feel awkward. I became a bit more careful about discussing mathematics, especially algebraic topology, not wanting to drag unsuspecting colleagues into an unpleasant situation.

In that spring of 1983 however, my third consecutive year on the job market, I finally received one regular offer, to go to Johns Hopkins University as an Assistant Professor with promise of rapid promotion. I will always be grateful to the members of the math department there who took me in and allowed me to continue my work when no one else would touch me. The mathematicians there were very nice to me, but the department is very small and badly supported by the university. There was a noticable difference in intellectual level between me and the departmental average. In general, I was never very happy there, through no fault of theirs.

When it then became clear that it would be impossible to completely get rid of me, the more overt attacks on me ceased. People, doubtless thinking I might be useful in hiring their graduate students, began to try to make up with me, saying things like "Well, you've made a mistake, but you've paid the price". Considering the reason I had to bail out of Chicago quickly, the reader with some concern for professional obligations of honesty will understand why I felt outraged by this kind of attempted rapprochement.

The reader's reaction to this whole long story is likely to be that it's all over years ago and I should forget about it. It seems that I shouldn't be griping about my career: I was a Sloan fellow and an invited speaker at the ICM. However, it remains that up to the time I left the US I was paid much less than most of my colleagues of the same age, and wound up living 200 miles from work so as to have a stimulating environment on the days I could go home. The fact that I was under a cloud and banished in the period when my best work on etale cohomological descent for K-theory came out has had a continuing effect on acceptance of this work. When people ask me how many papers I've published in Princeton's Annals of Math, I have to laugh. Finally, let me add one more little recent anecdote. In the winter of 1989-90, while I was in France and thinking about moving there, a bit after the time I received the invitation to speak at the ICM1990, I also sent out a couple of applications to spend my half-pay sabbatical year of 90-91 at big departments in the US. I got no offers from them. The most intersting case was MIT, where I had asked Hopkins, Macpherson, and Miller to push me. Haynes took the lead, saying he thought it would be "terrific" that I visit. I did tell him that I might decide to stay in France and not to push himself out on a limb if he encountered violent opposition. The next thing I heard about it was when I met Macpherson in May, and he asked me when I was coming. He was surprised that I hadn't received an invitation. I was less so.

I hope the reader can understand that I was really fed up with all this. Though France has its problems too, I don't encounter anything similar there, where my situation is much more pleasant and I expect, productive.

Yours,

Robert Thomason