RECOLLECTIONS

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In remembering my work on the Artin Conjecture on primitive roots I thought I would do best by providing a mini-autobiography for the period 1953–1967 that encompassed it. By doing this I felt I could give you some insight into how mathematics was at that time in Cambridge which I thought might be of interest.

Having been ill-prepared for Part II of the Maths Tripos on account of my army service, I took this at the beginning of my second year. Also on account of my army service I was eligible for a F.E.T. Grant for a period of up to six years. So I took Part III in my fourth year. By this time I had acquired a great interest in matrices and I judge now had by that time some material for publication. I therefore offered as one of my subjects in Part III a book by Wedderburn on matrices. As a result among my questions in the Part III exam was one on elementary divisors. But what I was asked to prove was incorrect. So I responded in two ways (i) by providing a counter example and then (ii) by proving what I thought the examiner meant me to establish. Nevertheless I did not succeed in securing a 'Distinction' as opposed to a pass. In consequence the lecturer I thought might supervise my research refused to take me on.

There was then a lot of discussion between myself and Corpus Christi College. Also between myself and my Mother and Father, who thought that I shouldn't pursue my mathematical ambitions further. I owe a great debt of gratitude to Birgitta, my fiancée, who encouraged me to persevere in my ambitions.

What followed on was rather traumatic. I tried the celebrated Prof. Hall, the group theorist, who was very polite but said he could not find any suitable topic for me to study. I then approached J. A. Todd because his sort of projective geometry involved a lot of matrix theory. He was not very encouraging saying that before the war only the very best went on to mathematical research.

I confided my difficulties to Dr Michael Drazin (the elder brother of Philip who was later a Professor in Bristol). He like me went to the tea and morning dances at the Dorothy Café (Hawkins) and was a Prize Fellow of Trinity, later a Professor at Duke University in the south of the U.S.A. He said, 'Why don't you try Mr Ingham?'; he said his pupils usually did very well. So I took his advice and made an appointment to see Ingham at King's. He was also very polite but said he would have to see my answers in Part III before taking matters further. A bit later he asked to see me again and asked me to look at a question related to some work of Mirsky. At the end of the long vacation I had something to shew him as a result of which he agreed to supervise my research. Then, after further negotiation between the War Office and my College, it was agreed my work would be supported for a further two years—this was actually most generous of the War Office.

The problem set by Ingham was to find an asymptotic formula for the sum

$$\sum_{n\le x} d(n)d(n+a)d(n+b), \quad a \ne b \ne 0$$

extending his formula for

$$\sum_{n\le x} d(n)d(n+a).$$

One of the objects of this would be then that one should replace $d(n)$ by $r(n)$, the number of representations of n as the sum of two squares. One would thus establish the infinitude of triples $n, n+a, n+b$ that were sums of two squares.

Having told Ingham that I had hardly studied any number theory before he merely recommended that I read a specific four chapters of Hardy and Wright—you can imagine how unlikely such advice would be today.

I found the problem set impossibly difficult. Indeed it remains unsolved so far as I know up to this day. Even Iwaniec with all his perspicuity has I think failed to solve it. However, I have solved the problem of triplets of sums of two squares that it was Ingham's intention for us to solve.

However, I came across the problem of asymptotic formulae for

$$\sum_{n\le x} d(n)d_3(n+a), \quad \sum_{n\le x} d_3(n)d(n+a)$$

that were discussed by Titchmarsh in the Quarterly Journal. The methods I had developed in connection with Ingham's problem, I used to solve the first problem with some complexity. This with some work on the latter problem gave me a Prize Fellowship at Corpus.

Some time onwards while still a pupil of Ingham, Ingham had put an opened copy of Hasse's Zahlentheorie on the table. This was on the page where Artin's primitive root conjectures were stated. Ingham waved his hand towards this and said I might like to try my hand at proving it. Always willing to have a go at problems, I did as he suggested. I came to the conclusion that the problem was not as difficult as the classical prime twin and Goldbach conjectures. But no sooner had I come to this conclusion than Prof. Heilbronn, who spent his vacations away from Bristol in Cambridge, informed me he had cracked the problem. So in disquiet I gave up thinking about the problem. In my seven years as a Lecturer in Bristol nothing was ever said about the matter again so I supposed that the problem had not been solved after all; I was, however, preoccupied with other problems.

Before the next episode of the story I should say a bit more about Mr Ingham. Both he and his wife were a bit eccentric and his wife most charming. He was quite a phenomenon, being a mathematical prodigy but the son of the chief groundsman of Yorkshire County Cricket Club. This explains why in summer months he was usually seen at Fenner's watching the county cricket matches and correcting manuscripts simultaneously; he had acquired the knack of knowing when the cricket action took place. Another peculiarity he had was maintaining the habit of wearing his square when walking from King's College to the Old Schools and back again: of course, in my day, gowns were worn by the lecturers.

The final part of the story finds me just in my chair in the University of Durham. It was a Friday morning and I was soon to catch the express to Bristol, near where I and my wife still lived. I was talking to Vernon Armitage who happened to mention Artin's conjecture and to whom I replied that I was acquainted with it. I then rushed to the station thinking it was obviously still unsolved and that I would jolly well resume my work on it. So ensconced in the dining car with a cigarette (maybe) and a miniature whisky (certainly), I renewed my thoughts on it. By the time I had arrived at Bristol, I was sure I had a conditional proof subject to an extended Riemann Hypothesis for certain Dedekind zeta functions (over Kummerian fields). I saw that this hypothesis could be weakened a bit but not substantially.

Let a be not a perfect square and not -1. Then the number of primes p for which a is a primitive root, mod p, is

$$N(a) \sim \frac{A(a)}{\log x}x, \quad (A(a) \neq 0).$$

Artin's values of $A(a)$ were almost certainly wrong—the value in accord with numerical evidence was that stated by me, reached with the aid of Fröhlich.