PETER SWINNERTON-DYER (1927-2018)

Peter Swinnerton-Dyer, whose work has greatly influenced the study of diophantine geometry in the 20th century, died at his home near Cambridge on December 26, 2018, at the age of 91. He attended Eton College, where the photograph below was taken. There he became interested in diophantine equations from reading Heath's translation of "Diophantus of Alexandria", and wrote his first paper [21] on the equation $x^4 + y^4 = z^4 + t^4$ while still at Eton.

Immediately after Eton, he went to Trinity College, Cambridge, and then, apart from a few years as a civil servant in London, he spent the rest of his life in Cambridge. In 1973 he became Master of St. Catharine's College, Cambridge, and was Vice-Chancellor of the University of Cambridge from 1979-1981. In 1983, he took on the onerous task of distributing government funding to Universities, first as chair of the University Grants Committee, and subsequently as Chief Executive of the Funding Council, during the years of the Thatcher government. He then happily returned to full time mathematical work in Cambridge for the rest of his life. In the short article which follows, we have tried to remember both the mathematics and the personal qualities of this remarkable man, who has influenced profoundly all of our own mathematical work and ideas, and from whose generous friendship we have all benefited so much. The reader will find other accounts of both his mathematics and his charismatic personality in a volume [16] dedicated to him on his 75th birthday. [1]

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1. Memories of Peter Swinnerton-Dyer, by Bryan Birch

Peter's first paper [21], written when he was a schoolboy at Eton, was about the arithmetic of diagonal quartic surfaces, as were several subsequent papers, for instance [24, 18, 30]; despite his being better known for the Birch–Swinnerton-Dyer Conjecture, it is fair to say that the arithmetic of algebraic surfaces was his lifetime mathematical interest. At first, he was a voice crying in the wilderness, but in the late 1960's Manin became interested in the subject, and for his last thirty years or so Peter was the senior member of a vigorous school on the subject, as Colliot-Thélène and Skorobogatov will describe later in this article.

Here, I will describe some of Peter's earlier work. In 1945 he went up to Trinity College Cambridge; after taking his BA he carried out research under the supervision of J. E. Littlewood. In 1950 he was awarded a Prize Fellowship at Trinity on the strength of his thesis on van der Pol's equation. Littlewood had worked on this equation during the war in collaboration with Mary Cartwright; Peter's thesis was not published at the time, but much later he published a group of papers in collaboration with Cartwright. During his Prize Fellowship Peter worked on various problems of number theory, which included a massive collaboration with E.S. Barnes [4] on the inhomogeneous minima of binary quadratic forms; in particular they determined which real quadratic fields are norm euclidean. The most interesting paper from this period is [9], a joint paper with Ian Cassels in which they tried unsuccessfully to extend the inhomogeneous minimum results to indefinite ternary quadratic forms, and to products of three real linear forms. The mathematics is beautiful, but they were too far ahead of their time, and hit rock! The 'natural' result they would have wished to prove for ternary quadratics was the Oppenheim conjecture, which was proved later by Margulis. As for products of three linear forms, their paper is quoted with approval by Lindenstrauss in his Fields Medal lecture, in the context of the Littlewood conjecture.

At the end of his Prize Fellowship, Peter spent the academic year 1954-5 with a Commonwealth Fund fellowship in Chicago; he went there intending to learn analysis with Zygmund, but was kidnapped by Weil, who converted him to algebraic geometry, particularly over the rationals. Weil's influence on Peter's mathematics was paramount; from that time on, Peter remained an arithmetic geometer, albeit with an unexpected affection for second order differential equations.

I first met Peter in the autumn of 1953, when he examined a second year prize essay I had written on the Theory of Games; he was very nice about it, though it was clear he would have preferred it to be somewhat shorter! While he was away in Chicago, I began research in the Geometry of Numbers under Ian Cassels, and when Peter returned as a teaching fellow I got to know him well; he taught me to love opera (I have happy memories of sitting on the floor listening to his recording of Callas singing Casta Diva), and we wrote a couple of (respectable but unremarkable) papers together. However, at the time, I was most interested by Davenport's analytic number theory. In turn, I went to Princeton with a Commonwealth Fund fellowship for the 1956-7 academic year, and while I was there I both wrote joint papers with Davenport by transatlantic mail, and also learnt a great deal of new mathematics. In particular I learnt of the beautiful reformulation of Siegel's work on quadratic forms in terms of a natural "Tamagawa measure" for linear algebraic groups. I seem to remember that Tamagawa gave a lecture, and Weil's comments made it exciting.

I returned to Cambridge, where Peter was now working in the Computer Laboratory, designing the operating system for TITAN, the machine planned to succeed EDSAC II. We wondered whether there was a similar phenomenon to the work of Siegel and Tamagawa for elliptic curves E over $\mathbb{Q}$. Specifically, was there a correlation between their local behaviour as described by their L-function $L(E, s)$, and their global behaviour, meaning their group $E(\mathbb{Q})$ of rational points? Our application for machine time was approved, with low priority - on certain nights we could use EDSAC from midnight until it broke down, which was typically after a couple of hours. The first task was to compute the rank of $E(\mathbb{Q})$ for a large number of elliptic curves. Cassels had provided the mathematics to do this via 2-descent, but at that time the computing problem was an awkward one because we needed to deal with several curves in parallel, and the amount of fast memory available on EDSAC was so very small. Peter was one of the very few people who could have managed it. The local behaviour presented more fundamental problems, because about the only thing we knew about $L(E, s)$ was that it converged when the real part of s was large enough. As John Coates describes later on, we were reduced to the naive expedient of computing the products $\prod(N_p/p)$. There was indeed a correlation, good enough to convince us but not enough for us to expect to convince anyone else! We needed to learn more mathematics! I think it was Davenport who told us that Hecke had dealt with $L(E, s)$ when E had complex multiplication. For the curves $y^2 = x^3 - Dx$, the critical value $L(E, 1)$ is a finite sum of values of elliptic functions, easily machine computable if D is not too large, and computable by hand using some algebraic number theory when D is really small. Initially, Peter calculated about 60 critical values approximately, replacing the Weierstrass function by $1/u^2$ if I remember correctly. I plotted their logarithms against $\log D$, and found that the points lay on parallel lines of slope -1/4 about $\log D$ apart; thus the values $L(E_D, 1)$ were a constant multiple of $D^{-1/4}$, and a somewhat mysterious power of 2, which we managed to identify in terms of local factors and the Tate–Shafarevich group. Eureka! The critical value of the L-function really meant something!

From that point on, the investigation was a delight. As I have said, at the start we knew practically nothing about the analytic theory of $L(E, s)$, so we had to find everything out. There turned out to be an incredibly beautiful theory, with modular functions being just part of it. We had the joy of working in a fresh, partly new and partly forgotten, area of deep mathematics, which was so beautiful that it was certain to be important. No one else, except some close friends, had any idea of what we were finding, so we had no need to publish prematurely. Cassels described our conjectures in his lecture at the 1962 Stockholm ICM [8], and the papers [6] were published a little later.

The computer laboratory was a wonderful place to work; very informal, a trifle ramshackle (fire precautions were paramount, but the equipment was built without unnecessary frills), and enormously exciting. Everything seemed possible! I have particularly happy memories of those nighttime sessions. Gina Christ was the first computer assistant attached to the engineering laboratory. She shared an office with Peter and was qualified to turn the machine off, a complicated process, as one had to avoid electric surges. Thus she normally kept us company for the nighttime sessions, and these were of course an ideal environment for getting to know one another. We got married in the summer of 1961, and of course Peter was my best man.

In the autumn of 1962 I moved to a job in Manchester, and then in 1966 to Oxford, where I have remained since. I continued to see Peter often, but we no longer lived in the same city and it was long before the internet. Thus we read each other's papers, but collaborated much less.

Leaving our conjecture aside, Peter's most important achievement in the 1960s was the operating system for TITAN. He also wrote a dozen or more papers on a variety of other subjects, including the first counterexample to the local-global principle for cubic surfaces [22] and his paper with Atkin [3] where they published their conjecture concerning modular forms on non-congruence subgroups. I could say much more, but refer the reader to "In Lieu of Birthday Greetings" at the beginning of [16], where there is also a more comprehensive list of Peter's papers.

2. The discovery of the conjecture of Birch and Swinnerton-Dyer, by John Coates

The conjecture discovered jointly by Peter Swinnerton-Dyer and Bryan Birch in the early 1960s both surprised the mathematical world, and also forcefully reminded mathematicians that computations remained as important as ever in uncovering new mysteries in the ancient discipline of number theory. Although there has been some progress on their conjecture, it remains today largely unproven, and is unquestionably one of the central open problems of number theory. It also has a different flavour from most other number-theoretic conjectures in that it involves exact formulae, rather than inequalities or asymptotic questions.

As we will explain in a little more detail below, their conjecture grew out of a series of brilliant numerical experiments on the early EDSAC computers in Cambridge, whose aim was to uncover numerical evidence for the existence of some kind of analogue for elliptic curves of the beautiful exact formulae proven by Dirichlet for the class number of binary quadratic forms, and powerfully extended to all quadratic forms by Siegel. The work of Dirichlet and Siegel had been extended to linear algebraic groups in general in the 1950s by Kneser, Tamagawa, Weil, and others. However, it was Birch and Swinnerton-Dyer alone who made the daring step of trying to find some analogue for elliptic curves. We now briefly recall their path-breaking computations, which were first published in [8] and [6]. An elliptic curve defined over the rational field $\mathbb{Q}$ is a non-singular projective curve of genus 1 defined over $\mathbb{Q}$ endowed with a given rational point $\mathcal{O}$. By the Riemann-Roch theorem, any such elliptic curve E will have a generalized cubic equation of the form

$$y^2 + a_1xy + a_3y = x^3 + a_2x^2 + a_4x + a_6, \tag{2.1}$$

all of whose coefficients are rational integers, with $\mathcal{O}$ being the unique point at infinity on (2.1). Of course, such an equation is not unique, but we simply take any one having the property that the absolute value of its discriminant Δ is as small as possible amongst all such equations for E. Then the set $E(\mathbb{Q})$ of rational points on E has a natural structure of an abelian group, and the celebrated theorem of Mordell asserts that this abelian group is always finitely generated. For each prime number p, define N_p to be the integer such that $N_p - 1$ is the number of solutions of the equation (2.1) viewed as a congruence modulo p. In the autumn of 1958, Birch and Swinnerton-Dyer began computing the finite products

$$f_E(P) = \prod_{p \leq P} N_p/p, \tag{2.2}$$

where p runs over all primes $\leq P$. They observed that the rate of increase of $f_E(P)$ as $P \to \infty$ seemed fairly closely related to the rank of $E(\mathbb{Q})$ as an abelian group, and were led to conjecture that $f_E(P)$ should be asymptotic as $P \to \infty$ to an expression of the form $C_E(\log P)^{g_E}$ for some strictly positive constant C_E; here, and in what follows, g_E will denote the rank of $E(\mathbb{Q})$. However, as they explain in [6], the value of $f_E(P)$ oscillates vigorously as P increases, and there seemed no hope of being able to guess a formula for the constant C_E from their numerical data. To overcome this difficulty, they quickly realized that they should instead work with the complex L-function $L(E, s)$ of E, which is defined by the Euler product

$$L(E, s) = \prod_{p|\Delta}(1 - t_pp^{-s})^{-1} \prod_{(p,\Delta)=1} (1 - t_pp^{-s} + p^{1-2s})^{-1}, \text{ where } t_p = p + 1 - N_p. \tag{2.3}$$

This Euler product converges in the half plane $\mathcal{R}(s) > 3/2$. Leaving aside all questions of convergence, one might expect that $L(E, 1)$ should then be related formally to $f_E(\infty)^{-1}$. This led them to their first revolutionary conjecture, which assumes the analytic continuation of $L(E, s)$ to $s = 1$.

Conjecture 2.1. *(Weak Birch-Swinnerton-Dyer conjecture) $L(E, s)$ has a zero at $s = 1$ of exact order g_E.*

In these early computations described in [6], Birch and Swinnerton-Dyer worked with the family of curves

$$E_D : y^2 = x^3 - Dx \tag{2.4}$$

where D is a non-zero integer, which is not divisible by either 4 or the 4-th power of an odd prime (more precisely, they considered all such D, for which the product of the odd primes dividing them is less than 108). Thus E_D is an elliptic curve with complex multiplication by the Gaussian integers $\mathbb{Z}[i]$, and Birch and Swinnerton-Dyer were using in [6] some 19th century work, due originally to Eisenstein and Kronecker, proving the analytic continuation of $L(E_D, s)$ to the whole complex plane, and giving a closed formula for $L(E_D, 1)$ in terms of values at points of finite order on E_D of Eisenstein series of weight 1. However, it must be stressed that this 19th century work had in no way given the slightest hint of some possible connexion between $L(E_D, 1)$ and the existence of non-trivial rational points on E_D. The first outcome of the EDSAC computations of Birch and Swinnerton-Dyer was to establish the apparent numerical validity of Conjecture 2.1 for the curve E_D for roughly 10^3 values of D, and this alone immediately convinced the world that they had uncovered something remarkable. However, at the same time, they took up the equally mysterious question of finding an exact arithmetic formula for $L(E, 1)$ when this value is non-zero, probably thinking that it should somehow be in the spirit of Dirichlet's celebrated exact formula for the class number of an imaginary quadratic field K in terms of $L(\chi, 1)$, with χ the non-trivial character of $\mathrm{Gal}(K/\mathbb{Q})$. Let $\omega_E = dx/(2y + a_1x + a_3)$ be the canonical holomorphic differential attached to the curve (2.1), and write Ω_E for the least positive real period of ω_E. When $L(E, 1) \neq 0$, it seems they guessed that $L(E, 1)/\Omega_E$ should be a rational number (an assertion which they proved for the curves E_D) which is closely related to the order of the mysterious and conjecturally finite Tate–Shafarevich group $Ш(E)$ of E. We recall that $Ш(E)$ is defined in terms of Galois cohomology by

$$Ш(E) = \mathrm{Ker}(H^1(\mathrm{Gal}(\overline{\mathbb{Q}}/\mathbb{Q}), E(\overline{\mathbb{Q}})) \to \prod_v H^1(\mathrm{Gal}(\overline{\mathbb{Q}_v}/\mathbb{Q}_v), E(\overline{\mathbb{Q}_v})), \tag{2.5}$$

where the bar denotes algebraic closure, v runs over all places of $\mathbb{Q}$, and $\mathbb{Q}_v$ is the completion of $\mathbb{Q}$ at v. They quickly found that the naive hope that, when $L(E, 1) \neq 0$, one might have an arithmetic formula of the form $L(E, 1)/\Omega_E = \#(Ш(E))/\#(E(\mathbb{Q}))^2$ failed for many of the curves E_D for the following reason. The one general result known about $Ш(E)$ at the time of their work, and indeed it is still the only general result known today, was the theorem of Cassels asserting that, if $Ш(E)$ is finite, then its order must be a perfect square. Their computations showed that, while the exponent of each odd prime in the factorization of $L(E_D, 1)/\Omega_{E_D}$ was even for all D in the range considered, this failed to be true for the prime $p = 2$ and certain values of D. Prodded by Cassels, they then realized that their naive conjecture should be replaced by the following modified form arising from considering an analogue of the Tamagawa number of E:-

Conjecture 2.2. *If $L(E, 1) \neq 0$, then*

$$L(E, 1)/\Omega_E = \frac{\#(Ш(E))}{\#(E(\mathbb{Q}))^2} c_\infty(E) \prod_{p|\Delta} c_p(E).$$

Here $c_\infty(E)$ is the number of connected components of $E(\mathbb{R})$, and for p dividing the minimal discriminant Δ of E, $c_p(E)$ is the index $[E(\mathbb{Q}_p) : E_0(\mathbb{Q}_p)]$, where $E_0(\mathbb{Q}_p)$ is the subgroup of points in $E(\mathbb{Q}_p)$ with non-singular reduction modulo p. For the curves E_D, they explicitly determined the factors $c_p(E)$ at the primes dividing $2D$ in [6], showing that they were all powers of 2. Then, seemingly miraculously they found that, in the range of values of D they were considering, with $L(E_D, 1) \neq 0$, Conjecture 2.2 did indeed predict that the order of $Ш(E_D)$ should be one of the values 1, 4, 9, 16, 25, 36, 49, or 81, and so always a square! In [6], they only explicitly discussed Conjecture 2.2 when $L(E, 1) \neq 0$, but it must have been known to them by this time that there was a fairly straightforward generalization of it to all elliptic curves E, involving the g_E-th derivative of $L(E, s)$ at $s = 1$, but with additionally the determinant of the canonical Néron-Tate height pairing on $E(\mathbb{Q})$ appearing on the numerator of the right hand side. The conjunction of Conjecture 2.1, and this general version of Conjecture 2.2, is what is known today as the strong Birch–Swinnerton-Dyer conjecture. Today, this strong Birch–Swinnerton-Dyer conjecture has been tested numerically more extensively than any other conjecture in the history of number theory, with the possible exception of the Riemann Hypothesis. For the most systematic account of these computations, see the website www.lmfdb.org/EllipticCurve/Q, which gives numerical data on the conjecture for the 2,247,187 elliptic curves E with conductor $< 360,000$. The numerical results obtained have always been in perfect accord with the strong Birch–Swinnerton-Dyer conjecture, assuming that the mysterious square of an integer which arises in the calculations is indeed the order of the Tate–Shafarevich group.

The international echoes of their work after it became public in 1965 were enormous, starting with the celebrated Bourbaki lecture in Paris in 1966 by John Tate [37], discussing their conjecture for abelian varieties of arbitrary dimension over all global fields, and going a remarkably long way towards proving the geometric analogue of it. They themselves quickly realized that their conjecture also explained one of the ancient mysteries of number theory as to why every positive integer $N \equiv 5, 6, 7 \bmod 8$ should be the area of a right-angled triangle, all of whose sides have rational length (it is a simple classical exercise to prove that a positive integer N is the area of a right-angled triangle all of whose sides have rational length if and only if the curve E_{N^2} has infinitely many rational points). Indeed, when $D = N^2$, Hecke's functional equation relating $L(E_D, s)$ and $L(E_D, 2-s)$ shows that $L(E_D, s)$ has a zero at $s = 1$ of odd order precisely when $N \equiv 5, 6, 7 \bmod 8$. Unfortunately, it is still unknown how to prove the part of Conjecture 2.1 asserting that $L(E, 1) = 0$ implies that $g_E > 0$, and so the ancient question remains open at present. In yet another direction, Peter carried out the first systematic computations on whether or not all elliptic curves E over $\mathbb{Q}$ are modular in the following sense. On multiplying out the Euler product (2.3), we obtain a Dirichlet series $L(E, s) = \sum_{n=1}^{\infty} c_n n^{-s}$, whose coefficients c_n are rational integers. Let C_E be the conductor of E. We say that E is modular if, on writing $q = e^{2i\pi\tau}$, the function $f_E(\tau) = \sum_{n=1}^{\infty} c_n q^n$ is a classical primitive cusp form of weight 2 for the subgroup $\Gamma_0(C_E)$ of $SL_2(\mathbb{Z})$ consisting of all matrices $\begin{pmatrix} a & b \\ c & d \end{pmatrix}$ with $c \equiv 0 \bmod C_E$. The question of whether E is modular was clearly very important for the Birch–Swinnerton-Dyer conjecture, since the work of Hecke shows, in particular, that $L(E, s)$ is an entire function when E is modular. In the late 1960s, Peter instigated the first systematic computations, described at the end of the volume [5], which listed all the modular elliptic curves $E/\mathbb{Q}$ with $C_E \leq 200$. It is now history that several great pieces of number theory emerged from these computations. Firstly, Birch, realized the importance of the neglected idea of K. Heegner for constructing rational points on modular elliptic curves. Then beautiful theoretic work by Gross and Zagier on the one hand, and Kolyvagin on the other hand, led to the following best known theoretical result in the direction of the conjecture of Birch and Swinnerton-Dyer.

Theorem 2.3. *(Kolyvagin–Gross-Zagier) Assume that $L(E, s)$ has a zero at $s = 1$ of order at most 1. Then the order of this zero is equal to g_E, and $Ш(E)$ is finite.*

Almost nothing is known about the conjecture of Birch and Swinnerton-Dyer when $L(E, s)$ has a zero at $s = 1$ of order strictly greater than one. Unfortunately, we also still seem to be quite a long way from proving the exact formula for the order of $Ш(E)$ predicted by the strong Birch–Swinnerton-Dyer conjecture when $L(E, s)$ has a zero of order at most 1, although the methods of Iwasawa theory have proven the p-part of this formula for many primes p. Note that there is now no assumption in the above theorem that E should be modular, because it is now history that Andrew Wiles [38], spurred along by mathematical ideas emerging from work on the Birch–Swinnerton-Dyer conjecture, found a marvellous proof that all E with square free conductor are indeed modular (this was then generalized in [7] to a proof that all elliptic curves over $\mathbb{Q}$ are modular). Moreover, it was shown by Ribet [17], prior to Wiles's work, that a proof that all elliptic curves over $\mathbb{Q}$ with square free conductor are modular would imply Fermat's celebrated conjecture asserting that, for any integer $n \geq 3$, the equation $x^n + y^n = z^n$ has no solution in integers x, y, z with $xyz \neq 0$. Curiously, Fermat had been led to this conjecture when he noted in the 17th century that his argument of infinite descent on the curve E_D for $D = 1$ implies his conjecture for $n = 4$, hinting at an almost spiritual connexion with the discoveries, made centuries later by Birch and Swinnerton-Dyer on the same family of curves!

Throughout all of the mathematical developments arising from his conjecture with Birch, Peter remained extremely level-headed, and carried on quietly his own mathematical work in both arithmetic and algebraic geometry, often using his great computational skills. He was also willing to take up heavy burdens for what he felt would be the general good of the University community. To graduate students in pure mathematics, Peter was always extremely generous and kind. On most evenings in the 1960s, he provided pre-dinner drinks for all comers in his beautiful rooms in Trinity College, and it was there that many of us first met distinguished international mathematical visitors, who were drawn to Cambridge by the work of Peter, and Ian Cassels. He continued to regularly attend the Departmental Number Theory Seminar until the end of his life.

3. Rational points on higher dimensional varieties, by Jean-Louis Colliot-Thélène

Diophantine Geometry and Differential Equations, A meeting in honour of Prof Sir Peter Swinnerton-Dyer's 70th birthday, Newton Institute, Cambridge (UK), 22nd-23rd September 1997
Standing, left to right: Richard Taylor, Noel Lloyd, Jan Nekovář, Jean-Louis Colliot-Thélène, Miles Reid, Don Zagier. Sitting, left to right: Colin Sparrow, Peter Swinnerton-Dyer, Bryan Birch.

Peter Swinnerton-Dyer's mathematical work is many-sided. He would not follow fashion, but would take up a classical subject and introduce new ideas, which he often shared with his co-authors, leaving it to others to develop them into systematic theories. When working on a proof he would not refrain from applying brute force and would often embark on lengthy computations [23, 32].

The general mathematical public is well aware of the Birch and Swinnerton-Dyer conjecture. Peter made many other contributions to arithmetic geometry. His two most cited papers are his joint paper with Atkin [2], prompted by computations of Dyson, and [25], related to work of Deligne and Serre. Both are concerned with congruences for coefficients of modular forms, suggested by work of Ramanujam. Among his other papers which continue to generate research today, let me mention his work with Ian Cassels [9] on the geometry of numbers, his work with M. Artin [1] on the Tate conjecture for a class of $K3$ surfaces over a finite field, his work with B. Mazur [15] on the arithmetic of Weil curves and p-adic L-functions, his work on lattice points on a convex curve [26], his recent work on the effect of twisting on the 2-Selmer groups [34]. Peter also made a number of contributions, in particular [20] and [33], to Manin's conjecture (1990) on counting points of bounded height. Here is a typical quote from [33]:

"This paper describes the mixture of ideas and computation which has led me to formulate more precise conjectures related to this problem. The process of refining (the initial guess) is iterative. One first formulates a more detailed conjecture. This then suggests computations which will provide evidence about the plausibility or otherwise of that more detailed conjecture; and if the evidence is confirmatory, it may suggest a further refinement of the conjecture. This process is of course only available to those who think that a conjecture should be supported by evidence."

Peter had a lifelong interest in rational points on some higher dimensional projective varieties over number fields: cubic surfaces and hypersurfaces, intersections of two quadrics of dimension at least 2, and also quartic surfaces. From the geometric point of view, the first two types of varieties are rationally connected varieties, whereas quartic surfaces are $K3$ surfaces.

Assuming that such a projective variety X over a field k is smooth, the first question is whether the set $X(k)$ of rational points is dense in X for the Zariski topology. The next question is whether a class of varieties to which X belongs satisfies the Hasse principle: if the set $X(\mathbb{A}_k) = \prod_v X(k_v)$ of adèles of X is not empty, is there a rational point on X? A stronger question is if $X(k)$ is dense in the topological set $X(\mathbb{A}_k)$. In this case one says that weak approximation holds. In 1962 Peter found the first counterexamples to these properties for cubic surfaces.

Starting with results of Minkowski and Hasse, these questions were thoroughly investigated for a very special class of rationally connected varieties, namely (compactifications of) homogeneous spaces of connected linear algebraic groups. Counterexamples to both the Hasse principle and weak approximation were constructed. These questions also come up in the study of curves of genus one, where Tate–Shafarevich groups, which are conjecturally finite, measure the failure of the Hasse principle.

In 1970 Manin suggested a general framework to explain many known failures of the Hasse principle, including the examples produced by Peter in 1962. Calling in the Brauer–Grothendieck group, he noticed that the closure of the set $X(k)$ of rational points is included in the Brauer–Manin set $X(\mathbb{A}_k)^{\mathrm{Br}}$ consisting of adèles orthogonal to the Brauer group of X. When the closure $X(k)^{cl}$ coincides with $X(\mathbb{A}_k)^{\mathrm{Br}}$, we say that the Brauer–Manin property holds.

At the same time, both Swinnerton-Dyer and Manin drew attention to work of F. Châtelet (1958) on some special cubic surfaces, where one can apply a factorization process somewhat analogous to descent on elliptic curves. Peter asked how general this process was, and whether it could be iterated. I had the good fortune of spending the year 1969-1970 in Cambridge, with Peter Swinnerton-Dyer, then Dean of Trinity College, as a mentor. He had just written his survey "Applications of algebraic geometry to number theory" – I still have the manuscript, in his beautiful handwriting. He suggested that I work on Châtelet's surfaces.

This would ultimately lead to at times intense exchanges of letters (in particular during Peter's time at the University Grants Committee and at the University Funding Council) and to a series of joint works (also with others) – the first one in 1984, the last one in 2012.

As a first answer to Peter's questions from 1970, a formal framework for this descent process (based on torsors under tori) was developed by Jean-Jacques Sansuc and me in the 70s. Our approach also clarified the connection with the Brauer–Manin set.

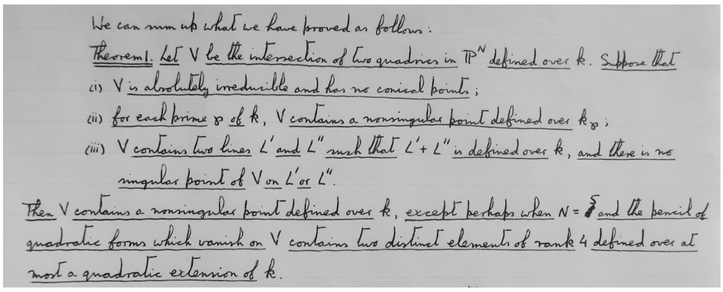
Starting around 1982, work of Peter, myself, Sansuc, Skorobogatov and many younger authors, by now too many to be listed here, has resulted in a series of precise conjectures on rational points on rationally connected varieties over a number field. There are some unconditional theorems and some conditional theorems which tell us what to expect. There is also a further series of unconditional theorems in a different direction, where one asks for existence and density (in a suitable sense) of zero-cycles of degree one. In this direction the initial breakthrough is due to P. Salberger (1988). I shall restrict myself to a description of some results Peter was involved in.

- Weak approximation holds for smooth intersections of two quadrics with a rational point in projective space $\mathbb{P}^n$ for $n \geq 5$ [10].
- The Hasse principle holds for smooth intersections of two quadrics in $\mathbb{P}^n$ for $n \geq 8$ [10].
- The Brauer–Manin property holds for generalized Châtelet surfaces, which are given by an affine equation $y^2 - az^2 = P(x)$, where P is a polynomial of degree 3 or 4. In the special case when $P(x)$ is irreducible, the Hasse principle and weak approximation hold [10].
- The Brauer–Manin property holds for the total space of a pencil of Severi–Brauer varieties over $\mathbb{P}^1$ conditionally on Schinzel's hypothesis. (This is a common generalization of Dirichlet's theorem on primes in an arithmetic progression and of the twin prime conjecture; various versions of this hypothesis were discussed by Bouniakowsky, Dickson, Hardy and Littlewood, Schinzel, Bateman and Horn). The unconditional proof of an analogous statement for zero-cycles instead of rational points [11, 13].
- Density of rational points on certain surfaces with a pencil of curves of genus one, including some diagonal quartic surfaces, conditionally on the combination of Schinzel's hypothesis and the conjectured finiteness of Tate–Shafarevich groups [14].
- The Hasse principle holds for diagonal cubic hypersurfaces in projective space $\mathbb{P}^n$ over the field of rational numbers for $n \geq 4$, conditionally only on the finiteness of the Tate–Shafarevich groups [31, 19].
- Various (unconditional) counterexamples: to the Hasse principle and weak approximation for cubic surfaces [22]; to an early conjecture on a geometric characterization of varieties on which rational points are potentially dense [12]; to an early conjecture on the structure of the closure of the set of rational points in the set of real points in a variety over $\mathbb{Q}$ where the rational points are dense for the Zariski topology [12].

Let us say a few words about the techniques involved.

The long paper [10] builds upon a combination of the descent method and the fibration method. The paper solves the questions on Châtelet surfaces raised in 1970. It came out of a combination of the descent formalism mentioned above and a fibration method initiated by Peter in 1982. To make a long story short, if one starts with a smooth projective surface X over a number field k with affine equation $y^2 - az^2 = P(x)$ with $P(x)$ of degree 4, and with the property $X(\mathbb{A}_k)^{\mathrm{Br}} \neq \emptyset$, the descent process on X produces a variety Y which projects onto X, is a (singular) intersection of two quadrics in a higher dimensional projective space, contains a pair of skew conjugate linear spaces, and satisfies $Y(\mathbb{A}_k) \neq \emptyset$. The rough idea now is to intersect Y with a suitable linear space so that the intersection Z

is an intersection of two quadrics in $\mathbb{P}^4$ with a pair of skew conjugate lines, still satisfying $Z(\mathbb{A}_k) \neq \emptyset$. Such surfaces are known to satisfy the Hasse principle. One concludes $Y(k) \neq \emptyset$ hence $X(k) \neq \emptyset$. The details are delicate.



We can sum up what we have proved as follows:

Theorem 1. Let V be the intersection of two quadrics in $\mathbb{P}^N$ defined over k. Suppose that

(i) V is absolutely irreducible and has no conical points;

(ii) for each prime $\mathfrak{p}$ of k, V contains a nonsingular point defined over $k_{\mathfrak{p}}$;

(iii) V contains two lines L' and L'' such that $L' + L''$ is defined over k, and there is no singular point of V on L' or L''.

Then V contains a nonsingular point defined over k, except perhaps when $N = 5$ and the pencil of quadratic forms which vanish on V contains two distinct elements of rank 4 defined over at most a quadratic extension of k.

Hasse's proof of his principle for zeros of quadratic forms in four variables has four ingredients: Hensel's lemma, Dirichlet's theorem on primes in an arithmetic progression, the law of quadratic reciprocity and the Hasse principle for quadratic forms in three variables. Replacing Dirichlet's theorem by Schinzel's hypothesis gives the Hasse principle for surfaces with equation $y^2 - az^2 = P(x)$, where $P(x)$ is irreducible of arbitrary degree (1979). The argument was extended to pencils of conics, quadrics and Severi–Brauer varieties [11]. In [13], one pushed the idea further and gave precise arithmetic and geometric conditions on a fibration $X \to \mathbb{P}^1$ for the method to work. In these papers one also extracts the essence of Salberger's device (1988), which enables one to transform a conditional proof for rational points as described above into an unconditional proof of a similar statement for zero-cycles of degree one.

These results concern families $X \to \mathbb{P}^1$ whose fibres satisfy the Hasse principle. A spectacular idea of Peter's [28], developed in [14], was a sophisticated version of these arguments applicable to some surfaces with a pencil of curves of genus one, which hitherto were not thought to be natural candidates for the Hasse principle. This will be detailed in Alexei Skorobogatov's contribution.

4. Sir Peter Swinnerton-Dyer, mathematician and friend, by Alexei Skorobogatov

I met Peter Swinnerton-Dyer in February 1989 on my first visit to the West. The Soviet Union had barely opened to the world, and nobody was sure how long this openness would last. To seize the opportunity, I managed to arrange for a private invitation, obtained a visa and bought a train ticket from Moscow to London via East and West Berlin. I came to meet Peter in his grand office near Regent's Park. His status among young Russian mathematicians was that of a celebrity, not in the least due to his nobility which added another twist to his fame. Peter was positively charming with his pleasant and benevolent manners, which – for a Russian – was striking if one bore in mind his elevated position in the British government. I knew that at the time he was the head of the University Funding Council (formerly the University Grants Committee), but I could not imagine the scale of the controversy related to his role in reforming British universities. We discussed mathematics of course. It was clear where Peter's heart was, so I was not too surprised that once his job in the government was over, he resumed his mathematical work at full blast.

The next time I saw him and his wife Harriet was at a soirée chez Jean-Louis Colliot-Thélène in the Parisian suburb of Massy, when Yuri and Xenia Manin were also present. The conversation was flowing a little less easily than the wine. Later, in a deliberate snub to the French and Russian schools of arithmetic geometry, largely centred on the legacy of Grothendieck, Peter insisted that he did not know what cohomology was, and was familiar with only the pre-1950 mathematics. The timing is important: 1954 was the year of his discipleship with André Weil in Chicago, and it is exciting to speculate if it led Peter and Bryan Birch to their famous conjecture. Peter's attitude to conjectures was also old-fashioned: he insisted that a conjecture should be made only when there was solid computational or theoretical evidence for it. He was not too sure if there was enough evidence for the influential Batyrev–Manin conjecture on rational points of bounded height. I think that on this occasion he said something like "Russian has no word for evidence". He himself also worked on this conjecture. Paper [27] contains this sentence: "$Z(s)$ can be analytically continued to the entire s-plane, but only as a meromorphic function with poles in somewhat unexpected places; and though it does satisfy a functional equation of a kind, it is not one which a respectable number theorist would wish to have anything to do with".

Not at all at ease with cohomology or the Brauer–Grothendieck group, Peter tried hard to make the Brauer–Manin obstruction to rational points explicit and amenable to calculation. In [28], one of the first papers written after he left the University Funding Council, Peter came up with a new method to prove the Hasse principle for rational points on a surface represented as a pencil of curves of genus one parameterised by the projective line. His technique was involved; some of the calculations passed through an explicit proof of the Tate duality for an elliptic curve over a local field, which he rediscovered. There were incredibly involved numerical computations with explicit choices of bases of vector spaces and lots of exotic conditions. Jean-Louis and I spent a couple of years trying to make sense out of this. One night at IHES, in a moment of illumination, I understood how this convoluted number theory could be reduced to linear algebra. This led the three of us to formulate a method which, under appropriate assumptions and conditionally on Schinzel's Hypothesis (H) and the Tate–Shafarevich conjecture on the finiteness of the Tate–Shafarevich groups of elliptic curves, proved that an everywhere locally solvable surface with a pencil of curves of genus one has a rational point [14]. The key idea, entirely due to Peter, is simple: find a rational point on the base such that the fibre is an everywhere locally solvable curve of genus one, and such that a suitable Selmer group of its Jacobian is so small that, unless the fibre has a rational point, it is incompatible with the fact that the order of the (conjecturally finite) Tate–Shafarevich group should be a square. It is indeed a theorem of Cassels that the Cassels–Tate pairing on the quotient of the Tate–Shafarevich group of an elliptic curve by its divisible subgroup is non-degenerate and alternating. Our paper was a first hint that it may be reasonable to expect that (at least, some) K3 surfaces satisfy the Hasse principle when it is not obstructed by the Brauer group.

Peter used this method to prove that large families of diagonal cubic surfaces satisfy the Hasse principle [31]. The assumptions of his elegant theorem are easy to state; the result is conditional only on the finiteness of the Tate–Shafarevich group of elliptic curves, but not on Hypothesis (H). A similar result was later proved in our joint paper on Kummer surfaces, a particular kind of a K3 surface [19]. Much later, Yonatan Harpaz realised that this method can be simplified if one borrows some ideas from papers of Mazur and Rubin; as a result, the heavy linear algebra machinery of [14] was replaced by more natural arguments. This happened often with Peter's innovations: extremely complicated computations were either dramatically simplified or completely eliminated, but the main idea continued to shine. In fact, to this day this remains the only known approach to the local-to-global principle for rational points on families of curves of genus one!

One of the most influential papers of Peter written around his 80th birthday is his work [34]. Like many of his papers, it uses the aforementioned linear algebra machinery of descent. A striking feature of this paper is that in it Peter used the main theorem of Markov chains to obtain an asymptotic distribution of the 2-Selmer rank in a universal family of quadratic twists of an elliptic curve. This was never done before, but turned out to be a very useful tool. Peter was quite happy with this unorthodox invention. "I am a computer scientist at heart", he commented.

Peter was fearless in his choice of problems but was never ashamed to produce an extremely convoluted solution if necessary. He did not care about the level of the journal. Once he had a new idea how one could prove the local-to-global principle for rational points on conic bundles with any number of degenerate fibres. It did not quite work and he was only able to do the case of 6 degenerate fibres (still unsurpassed). The result was the paper [29]; when I asked him about it, he remarked with melancholy that "this paper was not supposed to be read".

In line with the classical tradition of number theory, Peter was interested in rational points on cubic and quartic hypersurfaces, on intersections of two quadrics, on pencils of quadrics, and so on. In particular, the arithmetic of diagonal quartic surfaces is a recurrent theme in his work, from his first paper [21] (published under the pseudonym of P.S. Dyer while still at Eton), revisited in [24, 18, 30, 35], until his last paper [36] – published 73 years later! "My first paper was on diagonal quartics and my last paper will be on diagonal quartics", said Peter. The prophecy has been fulfilled.

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