In lieu of Birthday Greetings

B. J. Birch   Jean-Louis Colliot-Thélène
G. K. Sankaran   Miles Reid   Alexei Skorobogatov

This is a volume of papers in honour of Peter Swinnerton-Dyer's 75th birthday; we very much regret that it appears a few months late owing to the usual kind of publication delays. This preface contains four sections of reminiscences, attempting the impossible task of outlining Peter's many-sided contributions to human culture. Section 5 is the editor's summary of the 12 papers making up the book, and the preface ends with a bibliographical section of Peter's papers to date.

1 Peter's first sixty years in Mathematics by Bryan Birch

Peter Swinnerton-Dyer wrote his first paper [1] as a young schoolboy just 60 years ago, under the abbreviated name P. S. Dyer; in it, he gave a new parametric solution for $x^4 + y^4 = z^4 + t^4$. It is very appropriate that his first paper was on the arithmetic of surfaces, the theme that recurs most often in his mathematical work; indeed, for several years he was almost the only person writing substantial papers on the subject; and he is still writing papers about the arithmetic of surfaces sixty years later. Peter went straight from school to Trinity College (National Service had not quite been introduced); after his BA, he began research as an analyst, advised by J E Littlewood. At the time, Littlewood's lectures were fairly abstract, heading towards functional analysis; in contrast, Peter was advised to work on the very combinatorial, down-to-earth, theory of the van der Pol equation (the subject of Littlewood's wartime collaboration with Mary Cartwright), where a surprising sequence of stable periodic orbits arise completely unexpectedly from a simple-looking but non-linear ordinary differential equation. Lurking in the background was the three body problem, together with ambitions to prove the stability of the solar system, compare [20].

After a couple of years, Peter was elected to a Trinity Junior Research Fellowship, and became a full member of the mathematical community (he never needed to submit a doctoral thesis). In 1954, he was selected for a Commonwealth Fund Fellowship, and went to Chicago intending to work with Zygmund; but when he reached Chicago, he met Weil, who converted him to geometry; I believe that Weil was the person who most influenced Peter's mathematics. Ever since his year in Chicago, Peter has been an arithmetic geometer, with unexpected expertise in classical analysis.

Peter returned to Cambridge in 1955. In the 1950s, mathematical life in Cambridge was vigorous and sociable; everyone collaborated with everyone else. It was the heyday of the Geometry of Numbers (it was sad that so much excellent mathematical work was poured into such an unworthy subject!) and Peter joined in. In particular, he and Eric Barnes (later Professor at Adelaide) wrote a massive series of papers [5] on the inhomogeneous minima of binary quadratic forms, which completely settled the problem of which real quadratic fields are norm-Euclidean; like the van der Pol equation, this is a case where a 'discrete' phenomenon arises from a 'continuous' question. He went on to collaborate with Ian Cassels [8], trying to obtain a similar theory for products of three linear forms; their work was highly interesting, but only partially successful, and to this day there has been (I believe) no further progress on the problem.

I first came into contact with Peter in 1953, when he read my Rouse Ball essay on the Theory of Games (one of Peter's lesser interests, that does not show up in his list of publications), and I got to know him well after he returned from Chicago. Over the next couple of years, we talked a lot and he taught me to enjoy opera and we wrote two or three pretty but unimportant papers together; but at that stage, he wanted to be a geometer, and I was turning towards analytic number theory, under the influence of Harold Davenport. In my turn, I went to the States with a Commonwealth Fund Fellowship, and while I was away Peter took a post in the fledgling Computer laboratory. When I returned, I was excited by the Tamagawa numbers of linear algebraic groups, one of us (probably Peter) wondered about algebraic groups that aren't affine, and we set to, computing elliptic curves.

Those four years, from 1958–62, were probably the best of my life; they were the most productive, and I married Gina (who had a desk in Peter's office in the Computer laboratory). We were under no pressure to publish: we both had Fellowships, and knew we could get another job whenever we needed one; and we didn't have to worry about anyone else anticipating our work. In the first phase, we made a frontal assault; for the curves $E(a,b) : y^2 = x^3 + ax + b$ with $|a| \leq 20$ and $|b| \leq 30$ we computed the Mordell–Weil rank, the 2-part of the Tate–Shafarevich group, and a substitute $T(E, P)$ for a Tamagawa number $\tau(E)$, namely the product of p-adic densities taken over primes $p \leq P$ where P was as high as the market would stand. Peter did the programming, which he made feasible by dealing with many curves simultaneously; for good primes the p-adic density was of course N_p/p where N_p was the number of points mod p, and the crude methods of computing N_p for medium-sized p were nearly as fast for a batch of curves as for a single curve; there was an even better batch-processing gain in the rank computations. (For the finitely many 'bad' primes one needed so-called fudge factors, which I seem to remember were part of my job). To our delight, the numbers $T(E, P)$ increased roughly as $c(E) \log^r P$, where r was the Mordell–Weil rank of E; so we prepared [17] for publication, and proceeded to the second phase. Here, Davenport and Cassels were very helpful; urged by their prodding, we realised that, rather than considering the product $T(E, P)$ as P got large, one should be considering $L(E, s)^{-1}$ as s tends to 1 (so that $L(E, s)$ should have a zero of order r at $s = 1$). (As Weil remarked to a colleague in Chicago, 'it was time for them to learn some mathematics'.) Hecke had tamed this Dirichlet series for elliptic curves with complex multiplication, giving an explicit formula that actually converged at $s = 1$. So we approximated to the Dirichlet series $L(E, 1)$, in case E had complex multiplication and Mordell–Weil rank 1; and we got numbers that really seemed to mean something: after the junk factors had been scraped off, they seemed to be the order of the Tate–Shafarevich group divided by the torsion squared. Next, Davenport showed us how to evaluate $L(E, 1)$ explicitly in terms of the Weierstrass $\wp$-function; we computed some more, and [18], containing the main B–S-D conjectures, was the result.

In 1962 I left Cambridge to take a job in Manchester, and our collaboration became less close; we had expected to write further Notes in the series 'On Elliptic Curves', but they didn't happen. Note III might have been a plan of Peter's, to test the conjecture for abelian varieties by starting with products of elliptic curves; this turned into the thesis of Damerell, which essentially computed critical values of $L(E^{(3)}, s)$, where $E^{(3)}$ is the cube of a curve; the numbers were interesting but he was not able to interpret them. The intended Note IV was more important; Nelson Stephens was able to compute the higher derivatives $L^{(r)}(E, 1)$, where r is the Mordell–Weil rank; he was the first to obtain exact evidence for the conjectured formula, for elliptic curves of higher rank over the rationals, and indeed his thesis [93] is where it is first precisely stated. In July 1965, Peter received a letter from Weil [94] which set the tone for further progress in the area. Weil reminded us that our conjectures make sense only if the relevant functions $L(E, s)$ have functional equations, and this is likely to be true only if the elliptic curve $E/\mathbf{Q}$ is parametrised by modular functions invariant by some $\Gamma_0(N)$. So we had better be looking at modular curves! I was in Cambridge on sabbatical for the next term, so we set to work. Indeed, we worked very hard; on one occasion we were so engrossed talking mathematics after dinner, on Trinity Backs, that an unobservant porter locked us out; fortunately, we were able to regain entry by successfully charging the New Court gate.

Weil's letter led to three developments; first, modular symbols: I am pretty certain that Peter had the first idea [89], but he was very busy, so I and my students had to make them work, and Manin [91] formalised the concept. Next, the tabulation of elliptic curves of small conductor (Table I of [43]); this involved many people, starting with Peter and then me, as described in the introduction to the table. Finally, a few years later, Heegner points came on the scene.

I was most excited in our work on elliptic curves; but indeed Peter's interests in this period were exceedingly diverse. He did seminal work on his earliest love, the arithmetic of surfaces: in [15] he found the first counterexample to the Hasse principle for cubic surfaces (I think he found this example in 1959, as I reported on it in Boulder). A little later, he improved a result of Mordell, that the Hasse principle is valid for the intersection of two quadric hypersurfaces in $\mathbf{P}^n$ so long as the dimension n is large enough — this paper [19] is of interest as a very early example of Peter's technique of working out what one can prove if one assumes various useful but unprovable 'facts'; with luck, one may remove such unwanted hypotheses later. His 1969 paper [34] at the Stony Brook conference reviewed what was known, and contained new material. At last, in 1970, Peter ceased to be a lone voice crying in the wilderness, when Manin introduced the so-called Manin obstruction in his lecture at Nice [90], and went on to write his book on cubic surfaces [92]. Also in 1969–70, Colliot-Thélène went to Cambridge to work with Peter; since then the theory has flourished, as this volume amply testifies.

Meanwhile, Peter remained an analyst; in particular, Noel Lloyd was his research student between 1969 and 1972. He also became interested in modular forms for their own sake; with Atkin, he investigated modular forms on non-congruence subgroups [32]. Surprisingly, their results suggested that the power series of such modular forms should have good p-adic properties (their conjecture was proved long afterwards by Scholl). Peter corresponded with Serre, and published the basic paper on the structure of (ordinary) modular forms modulo p in the third volume of the Antwerp Proceedings [43]; this volume was of course the beginning of the theory of p-adic modular forms. Peter made yet another important contribution in the Computing Laboratory, where he was responsible for implementing Autocode for Titan.

He worried about the inefficiencies of university governance, and took an increasing interest in administrative matters. In 1973 he was elected Master of St Catherine's College, from 1979–81 he was Vice Chancellor, and from 1983–89 he was Chairman of the University Grants Committee. All this involved an immense amount of committee work, but miraculously (and with the help of Harriet, and of Jean-Louis Colliot-Thélène) he remained in touch with mathematics. When he returned to Cambridge in 1989 he resumed full-time research, principally on the arithmetic theory of surfaces, but also on analysis.

Oxford, 10th Dec 2002

2 Peter Swinnerton-Dyer's work on the arithmetic of higher dimensional varieties by Jean-Louis Colliot-Thélène

In parallel to his well-known contributions to elliptic curves, modular forms, L-functions, differential equations, bridge, chess and other respectable topics, Peter has a lifelong interest in the arithmetic geometry of some – at first sight – rather special varieties: cubic surfaces and hypersurfaces, complete intersections of two quadrics defining a variety of dimension ≥ 2, and quartic surfaces.

I happened to spend a year in Cambridge when I started research, and Peter passed on to me his keen interest in the corresponding diophantine questions. I am thus happy to report here on Peter's past and ongoing work on these problems. As will be clear from what follows, Peter, at age 75, is still doing entirely original innovative research.

Much of the progress achieved in arithmetic geometry during the twentieth century has been concerned with curves. For these, we now have a clear picture: for genus zero, the Hasse principle holds; for genus one, many problems remain, but we have the Birch and Swinnerton-Dyer conjecture, and we hope that the Tate–Shafarevich groups are finite; for genus at least two, Faltings proved the Mordell conjecture.

In higher dimension the situation is much less clear. For the three types of varieties mentioned above, one is still grappling with the basic diophantine questions: How can we decide whether there are rational points on such a variety? Is there a local-to-global principle, or at least some substitute for such a principle? What are the density properties of rational points on such varieties (in the sense of the Chinese remainder theorem)? Can one "parametrize" the rational points? Can one estimate the number of rational points of bounded height?

The time when varieties were classified according to their degree, as in Mordell's book, is long gone, and one may view the varieties just mentioned as belonging to some general classes of varieties. One general class of interest is that of rational varieties (varieties birational to projective space after a finite extension of the ground field). A wider class, whose interest has been recognized only in the last ten years, is that of rationally connected varieties. These are now considered as the natural higher dimensional analogues of curves of genus zero. Nonsingular intersections of two quadrics (of dimension ≥ 2) are rational varieties, hence rationally connected; so are nonsingular cubic surfaces. Higher dimensional cubic hypersurfaces are rationally connected. Nonsingular quartic surfaces are not rationally connected, but there are interesting density questions for rational points on them.

Until 1965, there were two kinds of general results on the arithmetic of rational varieties. One series of works, going back to the papers of H. Hasse in the twenties (local-to-global principle for the existence of rational points on quadrics), was concerned with homogeneous spaces of connected linear algebraic groups. A very different series of works, going back to the work of G. H. Hardy and J. E. Littlewood, proved very precise estimates on the number of points of bounded height (hence in particular proved existence of rational points) on complete intersections when the number of variables is considerably larger than the multidegree.

There had also been isolated papers by F. Enriques, Th. A. Skolem, B. Segre, L. J. Mordell, E. S. Selmer, F. Châtelet, J. W. S. Cassels and M. J. T. Guy. Peter himself made various contributions to the topic in his early work: he produced the first counterexamples to the Hasse principle and to weak approximation for cubic surfaces [15], he extended results of Mordell on the existence of rational points on complete intersections of two quadrics in higher dimensional projective space [19], and he proved the Hasse principle for cubic surfaces with special rationality properties of the lines.

Over the years 1965–1970, after some prodding by I. R. Shafarevich, Yu. I. Manin and V. A. Iskovskikh looked at this field of research in the light of Grothendieck's algebraic geometry. They did not solve all the diophantine problems, but they put some order on them. A typical illustration was Manin's appeal [90] to Grothendieck's Brauer group to reinterpret most known counterexamples to the Hasse principle, including Peter's.

I spent the academic year 1969/1970 in Cambridge – I was hoping to learn more about concrete diophantine problems, not the kind of arithmetic geometry I was exposed to in France. Professor Cassels advised me to take Peter as a research supervisor. I was first taken aback, because, ignorant as I was, the only thing I knew about Peter was that he had written a paper entitled “An application of computing to number theory”, and I was not too keen on computing. I wanted concrete diophantine equations, but with abstract theory. I nevertheless asked Peter, and this was certainly one of the most important moves in my mathematical career.

In those days, Peter was neither a Sir nor a Professor. He was known to Trinity students as "The Dean", whose function I understand was to preserve moral order among the students. To this he contributed by serving sherry ("Sweet, medium or dry?") each evening in his small flat in New Court. Sherry time was the ideal time to ask him for advice, mathematical or other – I do not remember Peter as a great addict of long sessions in the Mathematics Department. Well, at least one could enjoy his beautifully prepared lectures (the young Frenchman enjoyed the very clear, classical English as much as the mathematics). Peter was well known for his wit, and Swinnerton-Dyer quotations and stories abounded. His students enjoyed his avuncular behaviour – he was not a thesis adviser in the classical sense – and at the same time one vaguely feared him as the possible mastermind of many things going on in Cambridge. (His masterminding was later to extend to a wider scene – I remember Spencer Bloch being rather impressed by a 1982 newspaper representation of Peter Swinnerton-Dyer portrayed as King Kong climbing up one of London University's main buildings.)

One day in April 1970, on Burrell's walk, I asked Peter for a research topic. He mentioned the question of understanding and generalizing some work of François Châtelet, who had performed for cubic surfaces of the shape $y^2 - az^2 = f(x)$ (with $f(x)$ a polynomial of the third degree) something which looked like descent for elliptic curves – Peter also had handwritten lists of questions on a similar process for diagonal cubic surfaces.

In July 1970 I went back to France, and learned "French algebraic geometry" with J.-J. Sansuc. He and I discussed étale cohomology and Grothendieck's papers on the Brauer group, but I kept on thinking about Châtelet surfaces and Peter's questions. In 1976–77, Sansuc and I laid out the general mechanism of descent, which appeals to principal homogeneous spaces (so-called torsors) with structure group a torus (as opposed to the finite commutative group schemes used in the study of curves of genus one). One aim was to find the right descent varieties on Châtelet surfaces (and to answer a question of Peter, whether descent here was a one-shot process, as opposed to what happens for elliptic curves). The theory was first applied to more amenable varieties, namely to smooth compactification of tori. As far as Châtelet surfaces are concerned, there were two advances: In 1978, Sansuc and I realized that Schinzel's hypothesis (a wild generalization of the twin prime conjectures) – also considered much earlier by Bouniakowsky, Dickson, and Hardy and Littlewood – would imply statements of the type: the Brauer–Manin obstruction is the only obstruction to the Hasse principle for generalized Châtelet surfaces, namely for surfaces of the shape $y^2 - az^2 = f(x)$ with $f(x)$ a polynomial of arbitrary degree (over the rationals). The second advance took place in 1979: following a rather devious route, D. Coray, J.-J. Sansuc and I found a class of generalized Châtelet surfaces for which the Brauer–Manin obstruction entirely accounts for the defect of the Hasse principle.

During the period 1970–1982, Peter was busy with any number of different projects: the Antwerp tables on elliptic curves [45], understanding Ramanujan congruences for coefficients of modular forms [43], [51], writing, jointly with B. Mazur, an influential paper [37] on the arithmetic of Weil curves and on p-adic L-functions, proving (jointly with M. Artin [46]) the Tate conjecture for K3 surfaces with a pencil of curves of genus one (a function-theoretic analogue of the finiteness of the Tate–Shafarevich group), and also writing a number of papers on differential equations. He also wrote a note on the number of lattice points on a convex curve [33], which was followed by papers of other writers (W. M. Schmidt, E. Bombieri and J. Pila). The ideas in those papers now play a rôle in the search for unconditional upper bounds for the number of rational points of bounded height (work of D. R. Heath-Brown).

During that period, Peter also contributed papers on rational varieties: he gave a proof of Enriques' claim that del Pezzo surfaces of degree 5 always have a rational point [42], he wrote a paper with B. Birch producing further counterexamples to the Hasse principle [47] and he wrote a paper on R-equivalence on cubic surfaces over finite fields and local fields [56]. This last paper used techniques specific to cubic surfaces to prove results which have just been generalized to all rationally connected varieties by J. Kollár and E. Szabó, who use modern deformation techniques. That paper and a later one [87] on a related topic exemplify how Peter is not deterred by inspection of a very high number of special cases.

Indeed it is Peter's general attitude that a combination of cleverness and brute force is just as powerful as modern cohomological machineries. As the development of many of his ideas has shown, cohomology often follows, and sometimes helps. As we say in France, "l'intendance suit".

Let me here include a parenthesis on Peter's ideal working set-up. Sitting at a conference and not listening to a lecture on a rather abstruse topic seems to be an ideal situation for him to conceive and write mathematical papers. The outcome, written without a slip of the pen, is then imposed upon the lesser mortal who will definitely take much more time to digest the contents than it took Peter to write them.

In 1982, I spent another six months in Cambridge. I did not see Peter too often, as I was rather actively working on algebraic K-theory, not a field which attracts his attention. However, shortly before I left Cambridge, in June 1982, Peter invited me for lunch at high table in Trinity, and while reminding me how to behave in this respectable environment, he inadvertently mentioned that he could say something new on descent varieties attached to Châtelet surfaces – the topic he had offered to me as a research topic 12 years earlier. If my memory is correct, what he did was to sketch how to prove the Hasse principle on the specific intersections of two quadrics appearing in the descent process on Châtelet surfaces, the method being a reduction by clever hyperplane sections to some very special intersections of two quadrics in 4-dimensional projective space. Sansuc and I quickly saw how the descent mechanism we had developed in 1976–77 could combine with this new result. This was to develop into a Comptes Rendus note of Sansuc, Swinnerton-Dyer and myself [57] in 1984, then into a 170 page paper of the three of us in Crelle three years later [61]. Among other results, we obtained a characterisation of rational numbers that are sums of two squares and a fourth power, and we proved that over a totally imaginary number field two quadratic forms in at least 9 variables have a nontrivial common zero (this is the analogue of Meyer's result for one form in 5 variables). An outcome of the algebraic geometry in our work was a negative answer (joint work of the three of us with A. Beauville [59]) to a 1949 problem of Zariski: some varieties are stably rational but not rational.

Around 1992, the idea to use Schinzel's hypothesis to explore the validity of the Hasse principle (or of its Brauer–Manin substitute) was revived independently by J-P. Serre and by Peter [67]. In that paper, conceived during a lengthy coach trip in Anatolia, Peter simultaneously started developing something he calls the Legendre obstruction. In many cases, this obstruction can be shown to be equivalent to the Brauer–Manin obstruction, but Peter tells me there are cases where this yields information not reachable by means of the Brauer–Manin obstruction. In 1988, P. Salberger had obtained a remarkable result on zero-cycles on conic bundles over the projective line. The paper involved a mixture of algebraic K-theory and approximation of polynomials. Peter saw how to get rid of the K-theory and how to isolate the essence of Salberger's trick, which turned out to be an unconditional analogue of Schinzel's hypothesis. This was developed in papers of Peter, in a paper with me [66] and in a paper with A. N. Skorobogatov and me [73]. The motto here is: it is worth exploring results conditional on Schinzel's hypothesis for rational points, because if one succeeds, then one may hope to replace Schinzel's hypothesis by Salberger's trick and prove unconditional results for zero-cycles.

Up until about ten years ago, work in this area was concerned with the total space of one-parameter families of varieties which were close to being rational. In 1993 Peter invented a very intricate new method, which enables one to attack pencils of curves of genus one. In its general form, the method builds upon two well-known but very hard conjectures, already mentioned: Schinzel's hypothesis and finiteness of Tate–Shafarevich groups of elliptic curves. The original paper [69], in Peter's own words, looks like a series of lucky coincidences and "rather uninspiring" explicit computations (not many of us have the good fortune to come across such series). It already had striking applications to surfaces which are complete intersections of two quadrics.

It took several years for Skorobogatov and me to get rid of as many lucky coincidences as possible (one instance being a brute force computation which turned out to be Peter's rediscovery of Tate's duality theorem for abelian varieties over local fields). The outcome was a long joint paper of the three of us [74] in 1998. In that paper Peter's original method is extended beyond rational surfaces: the method can predict a substitute of the Hasse principle and density results for rational points on some elliptic surfaces (surfaces with a pencil of curves of genus one). This came as quite a surprise.

Since 1998, Peter has been developing subtle variants of the method, with application to some of the simplest unsolved diophantine equations: systems of two quadratic forms in as low as 5 variables [69], [74], [84], diagonal quartics [80] (hence some K3 surfaces, whose geometry is known to be far more complicated than that of rational surfaces); diagonal cubic surfaces and hypersurfaces over the rationals [85]. The first two applications assume Schinzel's hypothesis and finiteness of Tate–Shafarevich groups, but [85] (on diagonal cubic surfaces) only assumes the latter finiteness: this theorem of Peter's on diagonal cubic surfaces, both by the result and by the subtlety of the proof, is certainly the most spectacular one obtained in the area in the last ten years. For instance, under the finiteness assumption on Tate–Shafarevich groups, the local-to-global principle holds for diagonal cubic forms in at least 5 variables over the rationals.

In 1996, rather wild guesses were made on two different topics: For which varieties do we expect potential density of rational points? For varieties over the rationals with a Zariski-dense set of rational points, what should we expect about the closure of the set of rational points in the set of real points (question of B. Mazur)? Peter had the idea to call in bielliptic surfaces to produce unexpected answers to the second question. Skorobogatov and I elaborated, and applied the mechanism to get rid of preliminary guesses for the first question. This led to a joint work between the three of us [70]. There has been recent (conjectural) progress on an answer to the first question (work of complex algebraic geometers). The same bielliptic surfaces were later used by Skorobogatov (1999) to produce the first ever example of a surface for which the Brauer–Manin obstruction is not the only obstruction to the Hasse principle. This has led to further developments by D. Harari and Skorobogatov (descent under noncommutative groups).

Peter also contributed two papers [65], [77] to a topic which has seen quite some activity over the last ten years: the behaviour of the counting function for points of bounded height on Fano varieties. He pointed out the way to the correct guess for the constant in the standard conjecture (later important work in this area was done by E. Peyre and others). The lower bound he obtained (jointly with J. B. Slater [77]) for cubic surfaces is still one of the best results in this area.

The line of investigation Peter started in 1994 with the paper [69] is very delicate, and while his 2001 paper on diagonal cubic surfaces [85] is quite a feat, I am sure that Peter will produce much more in this exciting new direction. I am confident that he will keep on being as generous with his ideas as he has always been and that he will allow some of us to accompany him along the way.

Orsay, the 13th of February, 2003

3 Peter Swinnerton-Dyer: Geometer and politician by G.K. Sankaran

Peter Swinnerton-Dyer's interest in algebraic geometry derives arguably from its relation to number theory, and from the formative period he spent with André Weil in Chicago in the 1950s, but he has also made important contributions to geometry over algebraically closed fields. Probably his most notable technical result of a purely geometric nature is the proof (described elsewhere in this preface by Jean-Louis Colliot-Thélène) that stable rationality does not imply rationality [59]. This was, probably, contrary to the expectations of the majority of algebraic geometers at the time; though, as often happens, it is hard with hindsight to imagine why anybody ever thought the opposite was true. The result, published in French in a joint paper with Beauville, Sansuc and Colliot-Thélène, uses a wide range of techniques from different parts of algebraic geometry: torsors, linear systems with base points, Prym varieties and singularities of the theta divisor. It arose, however, out of arithmetic work with Sansuc and Colliot-Thélène. Many of Peter's arithmetic results have a geometric flavour, especially his work with Bombieri and with Artin; and it is now appreciated among geometers that arithmetic information can be made to yield geometrical or topological information (in addition to the well-known consequences of the Weil Conjectures). Rational and abelian varieties particularly feature in his work: these topics are represented in this volume by the papers of Reid and Suzuki and of Sankaran respectively.

Within algebraic geometry, however, Peter's chief influence has been as teacher, expositor, supplier of encouragement and enthusiasm, and éminence grise. He recognised, at a time when few in Britain were more than dimly aware of it, the power of the French school of algebraic geometry of Weil, Serre and Grothendieck. In the 1970s he encouraged his then student Miles Reid to visit Paris and learn directly from Deligne. The flourishing state of

British algebraic geometry at the present day owes much to this development, and to Peter's encouragement and direction of later students. His Cambridge Part III courses have been a source of inspiration to many, and his book on abelian varieties and his account of the basic facts of Hodge theory have been of great service to even more.

Many of Peter's multifarious activities are completely unrepresented in this book. The purpose of the rest of this note is to allude to some of them. I am not the best person to write such a note (that would be Peter Swinnerton-Dyer): I have drawn on my memories of conversations with many people, among them Carl Baron, Arnaud Beauville, Bryan Birch, Béla Bollobás, Jean-Louis Colliot-Thélène, James Davenport, Nicholas Handy, Richard Pinch, Colin Sparrow, Miles Reid, Pelham Wilson, Rachel Wroth and, above all, Peter Swinnerton-Dyer.

Mathematically the most obvious of Peter's other activities is his substantial contribution to the theory of differential equations, including a paper with Dame Mary Cartwright published only in Russian [55]. He is still active in differential equations. Readers of the present volume will have no difficulty in finding more information about this part of Peter's work. Slightly further afield, Peter was a member of the computing group in Cambridge in the 1960s, in the days of the Cambridge University computer TITAN. The original operating system for this famous machine, known as the Temporary Supervisor, was written by Peter single-handed, and it worked. He wrote the computer language Autocode for the same machine, and most Cambridge mathematicians of the 1960s had their first programming instruction in this language. Who could ask for anything more?

Peter, then Dean of Trinity College, was elected Master of St Catharine's College in 1973 and remained there for ten years. Littlewood is said to have greeted the news with Clemenceau's remark on hearing that the pianist Paderewski was to be Prime Minister of Poland: 'Ah, quelle chute!'. But St Catharine's afforded Peter considerable scope, and by all the numerous accounts I heard, as a later Fellow of St Catharine's, he was highly successful. The head of a Cambridge College (of Oxford I cannot speak) is commonly all but invisible to the students, and in some cases even to the Fellows. Peter was not: he has never been averse to the company of students and he was even willing to do College teaching. As he could and would teach almost any course in the Mathematical Tripos, the task of the Director of Studies (who is responsible for arranging for the students to be taught) was occasionally much simplified.

While at St Catharine's he served as Vice Chancellor of the University. This is now a full-time post held for a long period, but at the time the Vice Chancellor was chosen from among the heads of the various colleges and served for two years only. The role of the Chancellor (then, as now, the Duke of Edinburgh) is purely ceremonial, and the Vice Chancellor is in effect at the head of the University. It is a job for a skilled diplomatist. Cambridge University is a highly visible organisation, under constant and occasionally hostile scrutiny by newspapers and television. Internal matters can lead to very acrimonious public debate, and in extreme cases, which are quite common, the Vice Chancellor is expected to reconcile the factions. During Peter's term of office there was one especially well-publicised dispute about whether a tenured post should be awarded to a particular person. It was clearly impossible to satisfy all parties, but Peter nevertheless managed to bring the matter to a conclusion without offending anybody further. Who could ask for anything more?

Peter left St Catharine's to take up a post as Chairman of the University Grants Commission, a semi-independent Government body which was charged with deciding how Government funding ought to be apportioned among different universities. He had already written an influential, and in some quarters unpopular, report on the structure of the University of London, and was thus well known to be of a reforming cast of mind. He was also widely assumed to be in general political sympathy with the government of the time (otherwise, the reasoning ran, why did they appoint him?); but this was far from the case. He was nevertheless able to use his position to defend the reputation of the universities for financial responsibility, and in particular to establish the principle that research is a core activity for any university and therefore merits funding on its own account, independently of teaching. The price to be paid was investigation by government of the research activities of universities. Peter is thus often held responsible for, or credited with, the Research Assessment Exercise, which attempts to grade British university departments (not individuals) roughly according to the quality of research that they produce, and then hopes that they will be funded accordingly. The system is agreed to be imperfect, but it is easier to think of worse alternatives than better ones.

Peter's first involvement in politics dates from early in his tenure as Master of St Catharine's. The Member of Parliament for Cambridge resigned his seat and a by-election had to be held. Among the candidates was a representative of the Science Fiction Loony Party, whose aim in standing was to have some fun, and if possible to do better than the extreme right-wing candidate. Candidates in British parliamentary elections are required to pay a deposit of a few hundred pounds, returnable if they receive a certain proportion (then one-eighth) of the votes cast. In this case there was no prospect of that, so the deposit was, in effect, a fee: Peter, a wealthy man, paid it. He explained that the candidate "deserved every possible support, short of actually voting for him". Later his own name was mentioned as a possible parliamentary candidate, on behalf of the more serious but probably less entertaining Social

Democratic Party formed by Roy Jenkins and other disaffected members of the Labour Party in 1981. Nothing came of the plan, if it ever existed. The SDP seems, understandably, to have been unable to believe that all Peter's activities were the work of one man, and on occasion sent him two copies of the same letter, one for Swinnerton and one for Dyer.

Peter is a strong Chess player. Even when Vice Chancellor, he used to put in occasional appearances at the Cambridge University Chess Club, playing five-minute against undergraduates. The story is that when appointed to a Trinity research fellowship, he was strongly advised to cut down the time he spent on Chess; and that his interest in Bridge dates from this time. He was to become a very strong Bridge player. He was a member of the team that won the British Gold Cup in 1963, and he acted as non-playing captain of the Great Britain Ladies' Bridge team.

On leaving UGC (by then renamed UFC) Peter resumed work as a mathematician as if nothing had happened. He also continued his life of public service, working on behalf of such diverse institutions as the World Bank and the Isaac Newton Institute: he is still frequently to be found at the latter, at least.

Peter's work at UGC/UFC was recognised by the award of a knighthood (a KBE, to be precise). The editors of this volume tell me that "how did Swinnerton-Dyer get his title?" is a frequently asked question after seminars in places such as Buenos Aires and Vladivostok: at the risk of spoiling the fun, here is an explanation.

Peter is a baronet: he is also a knight. A baronet is entitled to call himself "Sir", and when he dies his eldest son, or some other male relative if he has none, inherits the title. It is only a title: it does not give him a seat in the House of Lords, and never has. Baronetcies were invented by King James I, early in the seventeenth century, as a way of raising money: they were simply sold. Later baronetcies were awarded for actual achievement, but the oldest ones are purely mercenary affairs. Since no baronetcies have been created for many years, all current baronets have inherited their titles rather than earning or buying them. A knight is also entitled to call himself "Sir", but the title dies with him. Knighthoods, which are still awarded in quite large numbers, are for specific personal achievements: they are given by the Queen on the recommendation of the Prime Minister. By the time he was knighted, Peter was already a baronet, so already entitled to call himself "Sir Peter" (not "Sir Swinnerton-Dyer"). For this reason he is sometimes referred to as $(\text{Sir})^2$ Peter, although strictly speaking "Sir" is idempotent: he is technically Professor Sir Henry Peter Francis Swinnerton-Dyer, FRS, Bt., KBE.

Bath, 12th Feb 2003

4 Peter Swinnerton-Dyer, man and legend by Miles Reid

I was supervised by Peter as a second year Trinity undergraduate. From then on, I was among the many Cambridge students who were occasionally invited for sherry at 7:30 pm (before Hall at 8:00 pm). For me and many other middle-class students of my generation, this provided an education into hitherto unsuspected areas of culture, such as good quality sherry, opera, college politics, famous math visitors, the workings of the British upper classes, etc. Peter is 16th Baronet Swinnerton-Dyer, and his family was an illustration that the feudal system was still alive and well, in Shropshire, at least in 1949: he had an elegant clock on the mantlepiece of his Trinity New Court apartment, with an inscription

> "Presented to Henry Peter Francis Swinnerton-Dyer Esq by the tenants, cottagers and employees of the Westhope estate on the occasion of his coming of age".

Peter's legendary status was already well established – as a sample of the stories in circulation, when Galois theory was introduced as a Part II course lectured by Cassels, Peter claimed that the whole course could be given in 4 hours, and made good his claim one evening between 10 pm and 2 am. Another story about bridge, that I heard from Peter himself: At a tournament, Peter called over the referee, told him formally that he was not making an error or oversight, then bid 8 clubs. Although this bid is impossible, he had calculated that he would lose less going down in it than allowing his opponents to make their grand slam. He knew the fine wording of the rules of bridge, and the match referee was forced to accept the impossible bid, since it was not made by error or oversight; the rules were subsequently changed to block this obscure loophole.

At that time Peter was Dean of Trinity; the position included disciplinary control of students. Those caught walking on the grass in College would be sent to Peter, and would in theory be fined in multiples of 6/8 (that is, 6 shillings and 8 pence, a third of a pound). In my case, for a particularly unpleasant misdemeanour, my sentence was to wash Peter's car.

Peter had an affinity with math students, and would drop in on friends in the evening to see if there was a conversation going on; I can well believe that student company was more fun than that of the senior combination room. He would often join in conversations, or dominate them – his predilection for that well-turned phrase certainly had a lasting effect on my literary pretensions. (For example: Would he send his son to Eton? "Certainly, it has advantages both in this life, and in the life that is to come.") Or, he would sometimes simply be comfortable among student friends and nod off to sleep (presumably this mainly happened after wine in the Combination room following High Table dinner). On one occasion, we played the board game Diplomacy from after dinner until breakfast the following morning – with great cunning and skill, Peter unexpectedly murdered me treacherously at about 6:00 am. Outside board games, Peter was extremely generous with friends and colleagues – many of us were invited to accompany him on a trip to the opera in London, or on a car trip to Norway, Paris or Italy, with appropriate stops to appreciate the great cathedrals and the starred restaurants of the Michelin guide.

As a PhD student I started to get more specific mathematical benefit from Peter's advice. He helped Jean-Louis Colliot-Thélène and me set up a seminar to study Mumford's little red book, and was always in a position to illustrate our questions with some example from his own research experience, although his background in Weil foundations meant that there was always the added challenge of a language barrier. The subject of my thesis (the cohomology of the intersection of two quadrics), given to me by Pierre Deligne, turned out to be closely related to Peter's work with Bombieri on the cubic 3-fold [22]. Peter was also in the thick of the action surrounding modular forms at the time of the 1972 Antwerp conference [43]–[45].

From 1978, when I got married and left Cambridge for Warwick, my contact with Peter became less frequent. A few years later, Peter married the distinguished archeologist Harriet Crawford (reader at UCL and author of 3 books in the current Amazon catalogue). Together with everyone else in British academia, I was frequently aware, often through the media, of his activities as Vice Chancellor of Cambridge, as Chairman of the University Grants Committee, as the person who persuaded the conservative government of Mrs Thatcher ("We shall not see her like again!") to accept research as the main criterion for judging the quality of universities, and in numerous other capacities. As a member of the British Great and Good, he chaired any number of committees or public enquiries, investigating anything from parochial malpractice at British universities (see

http://www.freedomtocare.org/page37.htm),

to the disastrous storm of 16th October 1987 (this on behalf of the Secretary of State, see Meteorological Magazine 117, 141–144). I met him, for example, in Japan on a mission to investigate the state of university libraries.

At about the time Peter retired from the UGC, Warwick University had the foresight to offer him the position of Honorary Professor. He has visited us on many occasions in this capacity, both on Vice Chancellor's business and for mathematical visits, on each occasion giving us the full benefit of his wit and wisdom (for example, his scathing comments on teaching assessment in universities: "The Teaching Quality Assessment was an extremely tedious farce, bloody silly"). On several occasions he has given two mathematical lectures on the same day, one in Diophantine geometry and another in differential equations, before taking us all out to a very good dinner.

Peter is more active in research than ever at age 75, and in closer contact with us at Warwick: he has repeated his lecture series "New methods for Diophantine equations" (first given in Arizona in December 2002) as a Warwick M.Sc. course, driving over each week and meeting us for lunch in a Kenilworth pub, at which Peter takes two pints of cider to put him in good voice for the afternoon lectures.

I close with some Swinnerton-Dyer quotes:

- To have a computer job rejected by the EDSAC 2 Priorities Committee, "You had to be both stupid and arrogant – neither alone would do it."
- On meeting Colin Sparrow in King's Parade "I have been made Chairman of UGC. Waste of a knighthood!"
- "They aren't true, of course, but one believes them at least as much as one believes the Thirty-Nine Articles of the Church of England."
- In Trinity College parlour with Alexei Skorobogatov (in connection with the dogma of the Orthodox Church): "In order to become a clergyman in the Church of England you need to believe only one thing – that it is better to be wealthy than poor."

Warwick, 21st Feb 2002

5 Editor's preface to the volume by Alexei Skorobogatov

The papers in this volume offer a representative slice of the delicately intertwined tissue of analytic, geometric and cohomological methods used to attack the fundamental questions on rational solutions of Diophantine equations. A unique feature of the study of rational points is the enormous variety of methods that interact and contribute to our understanding of their behaviour: to name but a few, the Hardy–Littlewood circle method, the geometry of the underlying complex algebraic varieties, arithmetic and geometry over finite and p-adic fields, harmonic analysis, Manin's use of the Brauer–Grothendieck group to define a systematic obstruction to the Hasse principle, the theory of universal torsors of Colliot-Thélène–Sansuc, and the analysis of Shafarevich–Tate groups. It is no exaggeration to say that pioneering work of Peter

Swinnerton-Dyer was an early example of many of these techniques, and a source of inspiration for others. The contents of this volume, that we now describe, reflect this vast influence.

Analytic number theory

The paper by **Enrico Bombieri** and **Paula B. Cohen** "*An elementary approach to effective diophantine approximation on* $\mathbf{G}_m$" concerns approximations of high order roots of algebraic numbers, with applications to Diophantine approximation in a number field by a finitely generated multiplicative subgroup. Such results can be obtained from the theory of linear forms in logarithms, whereas Bombieri's new approach is based on the Thue–Siegel–Roth theorem. The main improvement comes from a new zero lemma that is simpler than the lemma of Dyson employed up to now. The results sharpen Liouville's inequality for rth roots of an algebraic number a. More precisely, the authors obtain a lower bound for the distance $|a^{1/r} - \gamma|$, where γ is an algebraic number, and $|\cdot|$ a non-Archimedean absolute value.

Roger Heath-Brown's paper "*Linear relations amongst sums of two squares*" is an inspiring example of what analytic methods can do for the study of rational points. The main result of the paper is an asymptotic formula for the number of integral points of prescribed height on a class of intersections of two quadratic forms in six variables. This formula accounts for possible failures of weak approximation. The result is a significant advance in the state of knowledge on density of rational points, for existing methods (such as the circle method) provide asymptotic formulas given by the product of local densities. Heath-Brown determines the additional factor that reflects the failure of weak approximation — a conclusion that was hitherto inaccessible. Such a result should provide a stimulus to establish analogous conclusions for a broader range of examples. The proof involves descent to an intersection of quadratic forms, to which analytic methods can be applied. The analysis here is delicate, and motivated by earlier work of Hooley and Daniel.

Diophantine equations

Andrew Bremner's short note "*A Diophantine system*" finds infinitely many nontrivial $\mathbf{Q}$-rational points on the complete intersection surface given by

$$x_1^k + x_2^k + x_3^k = y_1^k + y_2^k + y_3^k \quad \text{for } k = 2, 3, 4.$$

Trivial solutions to this system, with the second triple a permutation of the first, are of no interest, but only one nontrivial rational solution was previously known. The proof is the observation that the hyperplane section $x_1 + x_2 + y_1 + y_2 = 0$ gives an elliptic curve of rank 1.

In "*Valeurs d'un polynôme à une variable représentées par une norme*", **Jean-Louis Colliot-Thélène**, **David Harari** and **Alexei Skorobogatov** consider the Diophantine equation $P(t) = N_{K/k}(z)$, where $P(t)$ is a polynomial and $N_{K/k}(z)$ the norm form defined by a finite field extension K/k. The paper builds on previous work by Heath-Brown and Skorobogatov, who combined the circle method and descent to prove results on rational solutions of this equation for $P(t)$ a product of two linear factors and $k = \mathbf{Q}$. It studies in detail the Brauer group of a smooth and proper model of the variety given by $P(t) = N_{K/k}(z)$, with k an arbitrary field, and calculates it explicitly under some additional assumptions. On the other hand, when $k = \mathbf{Q}$ and $P(t)$ is a product of arbitrary powers of two linear factors, the Brauer–Manin obstruction is proved to be the only obstruction to the Hasse principle and to weak approximation. This leads to some new cases of the Hasse principle.

The consensus among experts seems to be that the failure of the Hasse principle for rational surfaces can be characterised in terms of the Brauer–Manin obstruction (this is far from being settled; possibly the closely related problem for zero-cycles of degree 1 has more chances of success). Recent work of Skorobogatov shows that this fails for some bielliptic surfaces; the paper of **Laura Basile and Alexei Skorobogatov** "*On the Hasse principle for bielliptic surfaces*" explores this area, providing positive and negative results as testing ground for a future overall conjecture.

In his contribution "*On the obstructions to the Hasse principle*", **Per Salberger** gives a new proof of the main theorem of the descent theory of Colliot-Thélène and Sansuc. Surprisingly, this new approach avoids an explicit computation of the Poitou–Tate pairing at the crucial point of the proof, relying instead on standard functoriality properties of étale cohomology. One of the results was obtained independently by Colliot-Thélène and Swinnerton-Dyer, following Salberger's innovative 1988 paper. It is interesting to note that whereas Colliot-Thélène and Swinnerton-Dyer extended Salberger's original method, in the present paper Salberger uses for the first time Colliot-Thélène and Sansuc's universal torsors to prove results about zero-cycles. This demonstrates in a striking way that universal torsors are well adapted not only for rational points, but also for zero-cycles. This approach may eventually advance our understanding of the following question of Colliot-Thélène: is the Brauer–Manin obstruction to the existence of a zero-cycle of degree 1 the only obstruction, if we assume the existence of such cycles everywhere locally?

Shafarevich–Tate groups

Neil Dummigan, **William Stein** and **Mark Watkins'** paper "*Constructing elements in Shafarevich–Tate groups of modular motives*" gives a criterion for the existence of nontrivial elements of certain Shafarevich–Tate groups. Their methods build upon Cremona and Mazur's notion of "visibility", but in the context of motives rather than abelian varieties. The motives considered are attached to modular forms on $\Gamma_0(N)$ of weight > 2. Examples are found in which the Beilinson–Bloch conjectures imply the existence of nontrivial elements of these Shafarevich–Tate groups. Modular symbols and Tamagawa numbers are used to compute nontrivial conjectural lower bounds for the orders of the Shafarevich–Tate groups of modular motives of low level and weight ≤ 12.

Tom Fisher's paper "*A counterexample to a conjecture of Selmer*" answers the following question. Let K be a number field containing a primitive cube root of unity, and E an elliptic curve over K having complex multiplication by $\sqrt{-3}$. Is the kernel of this complex multiplication on the Shafarevich–Tate group of E over K of square order? The answer is positive if E is defined over a subfield $k \subset K$ such that $[K : k] = 2$, $K = k(\sqrt{-3})$, assuming that the Shafarevich–Tate group of E over k is finite. Examples show that without this assumption the answer can be negative. These results play an important rôle in the new method for proving the Hasse principle for pencils of curves of genus 1, first used by Heath-Brown and then artfully employed by Swinnerton-Dyer in his recent paper on the Hasse principle for diagonal cubic forms.

In "*On Shafarevich–Tate groups of Fermat jacobians*", **William McCallum** and **Pavlos Tzermias** find all the points on the Fermat curve of degree 19 with quadratic residue field; these turn out to be the points previously described by Gross and Rohrlich. The result about rational points is an application of the following result about the Shafarevich–Tate groups. For an odd prime p, let F be a quotient of the pth Fermat curve by μ_p, and let J be the jacobian of F. Then J has complex multiplication by the ring of integers of the cyclotomic field $K = \mathbf{Q}(\zeta_p)$. The authors prove that in certain cases there are nontrivial elements of order exactly $(1 - \zeta_p)^3$ in the Shafarevich–Tate group of J over K.

Zagier's conjectures

In his paper "*Kronecker double series and the dilogarithm*", **Andrey Levin** gives an explicit expression for the value of a certain Kronecker double series at a point of complex multiplication as a sum of dilogarithms whose arguments are values of some modular unit of higher level. This result can be interpreted in the spirit of Zagier's conjecture. The special value of the Kronecker double series is equal to the value of the partial zeta function of an ideal class for an order in an imaginary quadratic field. The values of the modular unit mentioned above belong to the ray class field corresponding to this order. This gives an explicit formula for the value of a partial zeta function at $s = 2$ as a combination of dilogarithms of algebraic numbers.

Complex algebraic geometry

In "*Cascades of projections from log del Pezzo surfaces*", **Miles Reid** and **Kaori Suzuki** weave a fantasy around the fascinating old algebraic geometric construction (del Pezzo, 1890) of the blowup of $\mathbf{P}^2$ in $d \leq 8$ general points and its anticanonical embedding. Some natural families of del Pezzo surfaces with quotient singularities are organized in 'cascades' of projections, similar to the way that the classic nonsingular del Pezzo surfaces are obtained by successive projections from the del Pezzo surface of degree 9 in $\mathbf{P}^9$ (in other words, $\mathbf{P}^2$ in its anticanonical embedding). Apart from their geometric beauty, these examples illustrate the technique of 'unprojection', a good working substitute for an as yet missing structure theory of Gorenstein rings of small codimension, and a possible tool to eventually construct one. The authors also sketch a program for the study of singular Fano 3-folds of index ≥ 2 according to their Hilbert series, modelled on the 2-dimensional case.

Gregory Sankaran studies the bilevel structures on abelian surfaces first introduced by Mukai. Given a $(1, t)$-polarized abelian surface A, a bilevel structure on A consists of a (canonical) level structure on A and a (canonical) level structure on the dual variety $\widehat{A}$, which also carries a natural $(1, t)$-polarization. The corresponding moduli problem gives rise to a Siegel modular threefold $\mathcal{A}_t^{\mathrm{bil}}$. Mukai proved the rationality of these moduli spaces for $t = 2, 3$ and 5. He also related them to the symmetry groups of the Platonic solids and to projective threefolds with many nodes. In "*Abelian surfaces with odd bilevel structure*" Sankaran proves that $\mathcal{A}_t^{\mathrm{bil}}$ is of general type for odd $t \geq 17$. A result of Borisov says that $\mathcal{A}_t^{\mathrm{bil}}$ is of general type for all but finitely many t. Borisov's method, however, gives no explicit bound.

Imperial College, Mon 24th Feb 2003

Bryan Birch,
Mathematical Institute,
24-29 St Giles',
Oxford, OX1 3LB, UK
e-mail: birch@maths.ox.ac.uk

Jean-Louis Colliot-Thélène,
C.N.R.S., U.M.R. 8628,
Mathématiques, Bâtiment 425,
Université de Paris-Sud,
F-91405 Orsay, France
e-mail: colliot@math.u-psud.fr

Miles Reid,
Math Inst., Univ. of Warwick,
Coventry CV4 7AL, England
e-mail: miles@maths.warwick.ac.uk
web: www.maths.warwick.ac.uk/~miles

G.K. Sankaran,
Department of Mathematical Sciences,
University of Bath,
Bath BA2 7AY, England
e-mail: gks@maths.bath.ac.uk

Alexei Skorobogatov,
Department of Mathematics,
Imperial College London,
South Kensington Campus,
London SW7 2AZ, England
e-mail: a.skorobogatov@ic.ac.uk

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