Dick Gross

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I was fortunate to become a PhD student of Dick Gross in 1987. This was a time of great intellectual ferment at Harvard, when Dick's work with Don Zagier [GZ2] was beginning to assert its profound impact on the theory of elliptic curves. One of the most mysterious invariants in the subject is the conjecturally finite Shafarevich-Tate group of an elliptic curve E over $\mathbb{Q}$, which measures the difficulty of computing the Mordell-Weil group $E(\mathbb{Q})$ by Fermat's method of infinite descent. Just as fundamental is the (weak) conjecture of Birch and Swinnerton-Dyer, which equates the rank of this Mordell-Weil group with the order of vanishing of the Hasse–Weil L-function $L_E(s)$ at $s = 1$. The Gross-Zagier formula assumes that E is modular[1] and relates the first derivative $L'_E(1)$ to the height of a Heegner point arising from the theory of complex multiplication. If $L_E(s)$ has a simple zero at $s = 1$, it follows that the Mordell-Weil group $E(\mathbb{Q})$ has rank *at least one* because it contains a Heegner point of infinite order. When combined with the work of Victor Kolyvagin [Ko], whose announcement was one of the highlights of my first year of graduate studies, the Gross-Zagier formula implies that both the Shafarevich-Tate conjecture and the Birch and Swinnerton-Dyer conjecture are true for E whenever $L_E(s)$ has a zero of order at most 1 at $s = 1$. This remarkable breakthrough remains very close to the state of the art more than 30 years later, especially for elliptic curves of rank 1 over $\mathbb{Q}$.

I asked Dick to be my supervisor right after passing the qualifying exams. In the beginning, I naively thought I should apprise him weekly of my rather plodding progress. My typical questions were quite mundane, and

[1] At that time, the proof that all elliptic curves over $\mathbb{Q}$ are modular, building on the breakthrough of Andrew Wiles, was still several years in the future.

Dick encouraged me to discuss them instead with Massimo Bertolini, who at the time was a year ahead of me. This turned out to be excellent advice: Massimo (who later moved to Columbia to work with Karl Rubin) soon became a close friend and collaborator, and we wrote more than 30 papers together over the years. The advice was also liberating: not having to meet with my supervisor regularly gave me the freedom to absorb the material at my own pace and pursue the directions that tempted me most. Since I was somewhat immature mathematically, these were mostly wild goose chases that produced little concrete progress over long stretches, but enhanced my experience of mathematics as a great intellectual adventure. Dick's policy of benign neglect, which suited me perfectly, did not extend to all his students. Some preferred more discipline and the reassurance of regular meetings, and Dick was always available for them. This illustrates one of the qualities I most admire in Dick as a supervisor: his knack for bringing out the best in his disciples by adapting to their individual needs and working styles. I have tried to replicate Dick's approach in my own graduate mentoring, but it is a hard act to follow!

The first thesis problem that Dick proposed was to extend Kolyvagin's result to the Hasse–Weil L-functions of elliptic curves twisted by an unramified character, or a more general ring class character χ of an imaginary quadratic field. The goal was to parlay the non-triviality of the "χ-component" of the Heegner point into the finiteness of the index of this point in the χ-component of the Mordell–Weil group, and of the associated χ-component of the Shafarevich-Tate group.

Massimo and I solved the problem together in the summer of 1989 while attending two memorable conferences back to back. The first was a historic joint US-USSR meeting in Chicago from mid-June to mid-July, where, in the early years of *perestroika*, Western participants got to meet, in person for the first time, many scientific luminaries from what was still called the Soviet Union. One of the highlights for me was shaking the hand of Kolyvagin, whom Dick introduced to his star-struck graduate students. In his address at the conference, Kolyvagin described how the full collection of Heegner points over ring class fields of imaginary quadratic fields largely determines the structure of the Selmer group of an elliptic curve. The Chicago meeting was followed by a two-week instructional conference at the University of

Durham in England, where Dick gave a beautiful survey [Gr] of Kolyvagin's method, which still serves as the standard initiation to the subject. Massimo and I wrote up our paper [BD] during a last stop in the Parisian suburb of Jouy en Josas, where I visited my parents before returning to Harvard.

Dick's question was perfect for a beginning graduate student because, although it did not follow immediately from Kolyvagin's proof, it was very much amenable to the methods that had been introduced, and solving it did not pose insuperable barriers. Yet it also admits natural variants that are significantly more difficult and interesting. My favorite one is "what if the imaginary quadratic field is replaced by a real quadratic field?" Dick's problem remains open in this setting. My frequent obsessing about it is undoubtedly what led me, a decade later, to the notion of "Stark-Heegner points" over ring class fields of real quadratic fields [Da]. And in 2010, Victor Rotger and I answered the real quadratic analogue of Dick's question in the more tractable setting of "analytic rank zero" [DR]. Namely, we showed that the χ-part of the Mordell–Weil group of an elliptic curve is finite when the associated L-series does not vanish, for χ a ring class character of a real quadratic field. The method we followed differs substantially from Kolyvagin's and is closer in spirit to approaches of Coates–Wiles and of Kato, with an important further input from Dick's own ideas on diagonal cycles and triple product L-functions. Ultimately, several of my most significant mathematical contributions have their genesis in the modest "warm-up exercise" that Dick proposed to Massimo and me in the spring of 1989.

As a more substantial thesis problem, Dick then asked me to reflect on Kolyvagin's method in light of the "tame refinements" of the Birch and Swinnerton-Dyer conjecture that had been proposed by Mazur and Tate around 1986. Dick's idea was that these tame refinements would provide the ideal framework for understanding and organising Kolyvagin's method of Euler systems. Like much of what I gleaned in my conversations with Dick, this insight was spot-on and very fruitful. Thanks to it, the main results in my thesis were already in place by early 1990, with a year and a half to spare before graduation, which made for an unusually pleasant and relaxed final year of graduate studies.

Over the last thirty years, my mathematical interests have never strayed far from the directions that Dick opened up and initiated me to. Among the projects that have given me special pleasure the last two decades, three stand out the most. The first is a collaboration with Samit Dagupta [DD] and Rob Pollack [DDP] revolving around the Gross-Stark conjecture on derivatives of Deligne-Ribet p-adic L-functions at $s = 0$, a direction which Samit has taken much further in his more recent work with Mahesh Kakde. The second is a project with Victor Rotger [DR] on p-adic L-functions attached to triple products of modular forms and associated diagonal cycles in triple products of modular curves, inspired by Dick's extension with Steve Kudla of the Gross-Zagier formula to L-functions of automorphic forms on the product of two orthogonal groups, attached to quadratic spaces of dimensions 3 and 4. The third is an ongoing study with Alice Pozzi and Jan Vonk of singular moduli for real quadratic fields via the RM values of rigid meromorphic cocycles [DV], [DPV]. What delights me the most in the latter is the opportunity it has given me to revisit the remarkably rich and seminal work of Gross and Zagier on the factorisation of differences of singular moduli [GZ1], which lays the foundations for their groundbreaking work on derivatives of L-series [GZ2].

Like all Harvard graduate students, and perhaps even more than most, I was in awe of my supervisor, and somewhat intimidated by him. Because I saw him as an intellectual father figure more than as a friend, we never became very close. Yet the impact he has had on me is tremendous. In the words of Ralph Waldo Emerson, "Our chief want in life is somebody who will make us do what we can". As a mentor, Dick accomplished this superbly. His example has guided me and his ideas have inspired me throughout my mathematical life, and for this I am immensely grateful to him.

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