Review* of the Collected Works of John Tate

J.S. Milne

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For several decades it has been clear to the friends and colleagues of Tate that a "Collected Works" was merited. The award of the Abel Prize to Tate in 2010 added impetus, and finally, in Tate's ninety-second year we have these two magnificent volumes,[1] edited by Barry Mazur and Jean-Pierre Serre. Beyond Tate's published articles,[2] they include five unpublished articles and a selection of his letters, most accompanied by Tate's comments, and a collection of photographs of Tate. For an overview of Tate's work, the editors refer the reader to [4]. Before discussing the volumes, I describe some of Tate's work.

1 Hecke L-series and Tate's thesis

Like many budding number theorists, Tate's favourite theorem when young was Gauss's law of quadratic reciprocity.[3][4] When he arrived at Princeton as a graduate student in 1946, he was fortunate to find there the person, Emil Artin, who had discovered the most general reciprocity law, so solving Hilbert's ninth problem. By 1920, the German school of algebraic number theorists (Hilbert, Weber,...) together with its brilliant student Takagi had succeeded in classifying the abelian extensions of a number field K: to each group I of ideal classes in K, there is attached an extension L of K (the class field of I); the group I determines the arithmetic of the extension L/K, and the Galois group of L/K is isomorphic to I. Artin's contribution was to prove (in 1927) that there is a *natural* isomorphism from I to the Galois group of L/K. When the base field contains an appropriate root of 1, Artin's isomorphism gives a reciprocity law, and all possible reciprocity laws arise this way.

In the 1930s, Chevalley reworked abelian class field theory. In particular, he replaced "ideals" with his "idèles" which greatly clarified the relation between the local and global aspects of the theory. For his thesis, Artin suggested that Tate do the same for Hecke L-series. When Hecke proved that the abelian L-functions of number fields (generalizations of

*Written for the Bulletin of the AMS. All footnotes were omitted from the version submitted to the AMS. The published version includes errors introduced by the copy editor. CW = Collected Works of Tate. Slightly revised March 4, 2017; May 24, 2017.

[1] Tate was born March 23, 1925, and so his ninety-second year ended on March 22, 2017. The review was written from the pdf files. The volumes did appear in early 2017, and they are magnificent.

[2] Some, including all of Tate's Bourbaki talks, have been newly typeset (papers 11, 22, 23, 32, ...).

[3] Tate 2011, p. 445 (Interview with Abel Laureate John Tate. Notices Amer. Math. Soc. 58 (2011), no. 3, 444–452).

[4] Michael Weiss (Woit blog, Oct 17, 2016): John Tate shared with our undergraduate number theory class in the spring of 1977 (then designated Math 101) that as a teen he came across Quadratic Reciprocity and immediately closed the book to attempt a proof himself. For several months, Prof. Tate recalled, he persevered but without success. Prof. Tate wistfully noted to the class that by contrast the young Gauss had developed four different proofs. He then passed around Disquisitiones Arithmeticae in Latin. John never spoke to us of his own enormous contributions to generalized reciprocity laws.

Dirichlet's L-functions) have an analytic continuation throughout the plane with a functional equation of the expected type, he saw that his methods applied even to a new kind of L-function, now named after him. Once Tate had developed his harmonic analysis of local fields and of the idèle group, he was able prove analytic continuation and functional equations for all the relevant L-series without Hecke's complicated theta-formulas.

As Kudla writes:[5]

> Tate provides an elegant and unified treatment of the analytic continuation and functional equation of Hecke L-functions. The power of the methods of abelian harmonic analysis in the setting of Chevalley's adèles/idèles provided a remarkable advance over the classical techniques used by Hecke. ... In hindsight, Tate's work may be viewed as giving the theory of automorphic representations and L-functions of the simplest connected reductive group $G = \mathrm{GL}(1)$, and so it remains a fundamental reference and starting point for anyone interested in the modern theory of automorphic representations.

Tate's thesis completed the re-expression of the classical class field theory in terms of idèles. In this way, it marked the end of one era, and the start of a new

2 Galois cohomology and the Tate-Nakayama isomorphism

Tate was awarded the Cole Prize by the AMS in 1956 for his paper determining the higher dimensional cohomology groups of class field theory.[6] For this, Tate observed that it is possible to regard the homology groups of a finite group as cohomology groups with negative coefficients and so obtain a sequence of cohomology groups indexed by $\mathbb{Z}$. He then constructed a canonical sequence of isomorphisms

$$H^i(G,\mathbb{Z}) \to H^{i+2}(G,C), \quad i \in \mathbb{Z},$$

whenever G is a finite group and C is a G-module satisfying certain hypotheses. If G is the Galois group of a finite extension L/K of number fields, then the idèle class group C of L satisfies the hypotheses, and so Tate's theorem describes the groups $H^i(G,C)$ in terms of the known groups $H^i(G,\mathbb{Z})$. Tate's isomorphism becomes Artin's reciprocity isomorphism when G is abelian and $i = -1$.

Tate's isomorphism was generalized by Nakayama, and then again by Tate [8], and is now called the Tate-Nakayama isomorphism. It has become a basic tool in algebraic number theory.

Tate continued his study of the applications of the cohomology of finite groups to arithmetic, and he introduced the cohomology of profinite groups (which he initially called groups of Galois type). He proved duality theorems, results on Euler characteristics, and he introduced and studied the notion of the cohomological dimension of fields. He announced

[5] Kudla, S. In: An introduction to the Langlands program. Edited by Joseph Bernstein and Stephen Gelbart. Birkhäuser Boston, Inc., Boston, MA, 2003, p.133.

[6] Fifth award: The 1956 Frank Nelson Cole Prize in Number Theory John T. Tate for his paper, The higher dimensional cohomology groups of class field theory, Annals of Mathematics, Series 2, volume 56 (1952), pp. 294-297. (AMS website)

some of his results in his 1962 ICM talk, but the material grew faster than his ability to write it up.[7] Fortunately, others (Serre, Lang, ...) accomplished this task for him. Tate's article [11] is a brief introduction to this material.[8]

3 Local class field theory and Lubin-Tate spaces

By the mid 1950s, abelian class field theory could be considered a mature field. However, some problems remained. One was that of explicitly generating all the fields shown to exist by the theory. For number fields, this is Hilbert's twelfth problem, for which there is still only a partial solution. For local fields, the problem was spectacularly solved by Lubin and Tate.

Tate's student Lubin had completed his thesis on one-parameter formal Lie groups in 1963. In early 1964, Tate wrote:

> [From Lubin's results] we get a homomorphism $G_K \to U_K$ = units in K. Restricting this to the inertia group G_{K_u} we get a homomorphism $G_{K_u} \to U_K$, which is probably canonical by the unicity remarks above, and which it is impossible to doubt is in fact the *reciprocity law isomorphism* (or its negative!) ... The miracle seems to be that once one abandons algebraic groups, and goes to formal groups, the theory of complex multiplication applies *universally* (locally). (Letter to Serre, January 10, 1964; CW, L8.)

Local class field theory provides, for each prime element π of a local field K, a totally ramified abelian extension K_π of K such that every abelian extension of K is contained in the composite of K_π with an unramified extension. For each π, Lubin and Tate give a remarkably simple construction of a tower of extensions of K whose union is K_π.[9]

Lubin and Tate went on to study the moduli spaces of one-parameter formal Lie groups. These turned out to be more important than the authors imagined at the time. The towers obtained by adding level structures were studied by Drinfeld.[10] The "Lubin-Tate spaces" obtained in this way form a local analogue of Shimura varieties, and played a role, for example, in the proof by Harris and Taylor of nonabelian local class field theory for GL_n (the local Langlands conjecture for GL_n).[11]

[7] I am writing down Galois cohomology, but infinitely slowly. I am distracted by sheaves and schemata and students (letter to Serre November 17, 1958). In my old age I am becoming unhappy about not publishing anything. I plan to spend the next week writing up my proof of Néron's quadraticity of height... After that I might have the courage to take up the cohomology again (letter to Serre, July 15, 1963). Elsewhere, Tate said: "I am not a very prolific writer. I usually write a few pages and then tear them up and start over..." (Tate 2011, p. 449).

[8] Artin and Tate gave a seminar on class field theory in 1951-1952. Serge Lang, who had arrived at Princeton a year after Tate, took notes for all but the first four chapters. The various chapters were eagerly awaited as they were typed up. The complete notes were only distributed (by the Harvard mathematics department) in 1961, having been delayed by a discarded project to incorporate Tate results on the higher cohomology groups. The notes were revised by Tate, and published as a book in 2009, fifty-eight years after the original seminar.

[9] When Bott proved his periodicity theorem, he had been so happy that he wrote Serre a long letter *in French*. In a letter to Serre announcing his results with Lubin, Tate said "I would write in French if I were Bott". His letter to Serre announcing his proof of the isogeny conjecture over finite fields (paper 20) *was written in French.* (Letters to Serre March 3, 1964 and February 17, 1966, and comments by Serre p. 856, p. 861.)

[10] Drinfeld, V. G. Elliptic modules. (Russian) Mat. Sb. (N.S.) 94(136) (1974), 594–627, 656.

[11] CW, Paper 24, and the comments of Tate.

4 Abelian varieties and the Artin-Tate conjecture

For a complex analyst, an abelian variety is a complex torus $A = \mathbb{C}^g/\{\text{lattice}\}$ admitting an embedding into projective space by theta functions. In giving substance to his proofs of the function field analogues of the Riemann and Artin conjectures (announced in 1940), Weil developed the theory of abelian varieties over *arbitrary* fields (1948)[12]. In the subsequent years, much of the theory of elliptic curves was extended to abelian varieties of arbitrary dimension. For example, every embedding of an abelian variety into projective space defines a theory of heights on the abelian variety, but an abelian variety has many such embeddings. Néron conjectured that there is a canonical height satisfying a certain quadraticity condition, and Tate found a remarkably simple proof of Néron's conjecture.[13]

In an important series of papers (1959–65), Cassels reworked the arithmetic theory of elliptic curves over number fields. In particular, he proved some duality theorems, including one showing that the mysterious Tate-Shafarevich group of the curve has order a square if finite. Tate extended the results of Cassels to abelian varieties over number fields.

As a result of some of the first computer experiments, Birch and Swinnerton-Dyer were led to conjecture that the rank of the group of $\mathbb{Q}$-points of an elliptic curve over $\mathbb{Q}$ is equal the order of the zero of its L-function at $s = 1$ (BSD1). Later they found a conjectural formula for the first Taylor coefficient of the L-function in terms other arithmetic invariants of the curve, including the order of the Tate-Shafarevich group and the discriminant of the height pairing (BSD2). Tate recognized that the natural setting for the conjectures is an abelian variety over a global field, and his proof of Néron's conjecture allowed him to state them in this generality.

For a surface X fibred in elliptic curves over a curve over a finite field, Tate recognized that the function field version of (BSD1) for the generic fibre implied that the rank of the Picard group of X equals the order of the pole of its zeta function at $s = 1$. This is one of the observations that led him to the Tate conjecture (see the next section). With Michael Artin, he examined the significance of (BSD2) for the surface. This led them to state the Artin-Tate conjecture which relates the behaviour near $s = 1$ of the zeta function of a surface over a finite field to other arithmetic invariants of the surface including the order of its Brauer group.

5 The Tate conjecture

The Tate conjecture occupies the same central place in arithmetic algebraic geometry that the Hodge conjecture occupies in complex algebraic geometry. For an algebraic variety over $\mathbb{C}$, the Hodge conjecture characterizes the $\mathbb{Q}$-subspace of the usual cohomology groups conjecturally spanned by the classes of algebraic cycles.[14] For an algebraic variety over a

[12]Weil, André. Variétés abéliennes et courbes algébriques. Actualités Sci. Ind., no. 1064, Hermann & Cie., Paris, 1948.

[13]Tate discovered his proof while teaching a course on abelian varieties in Fall 1962, and included it in the course. He explained it in various letters, but didn't published it himself. See footnote 7. In a letter to Serre (October 24, 1962), Tate said that he had written Néron "a proof of his Edinburgh conjectures which is so trivial I can hardly believe it". In the letter, he left the proof as an exercise (with hints) for Serre.

[14]Mumford and Tate tried to prove the Hodge conjecture for abelian varieties by showing that the $\mathbb{Q}$-algebra of rational (p, p) classes is generated by those of type $(1, 1)$, for which the conjecture was known, but Mumford found a counterexample to this. When Tate told Weil of the example, he remarked that it is a special case of a slightly more generic example, namely, a 4-dimensional family of examples, and then said: "As you and

general algebraically closed field k, the Tate conjecture characterizes the $\mathbb{Q}_\ell$-subspace of the ℓ-adic étale cohomology conjecturally spanned by the algebraic classes.

Consider an algebraic variety X (smooth and projective) over an algebraically closed field k. An algebraic cycle is defined by finitely many polynomials having only finitely many coefficients, and so it and its cohomology class are defined over a subfield of k finitely generated over the prime field. The Tate conjecture says that this condition can be used to characterize the $\mathbb{Q}_\ell$-space spanned by the algebraic classes. There are many variants and special cases of the conjecture. One is the relation between the Picard number of a surface over a finite field and its zeta function noted above. Another (the isogeny conjecture) is that for abelian varieties A and B over a field k finitely generated over the prime field, the map

$$\mathrm{Hom}(A,B)\otimes\mathbb{Z}_\ell \to \mathrm{Hom}(T_\ell A, T_\ell B)^{G_k}$$

is an isomorphism; here $T_\ell A$ is the first ℓ-adic homology group of A (its Tate group) and G_k is the absolute Galois group of k. A third, particularly favored by Tate, is the following statement:

> If X is a regular scheme of finite type over $\mathbb{Z}$, then the order of $\zeta(X,s)$ at the point $s = \dim X - 1$ is equal to $\operatorname{rank} H^0(X,\mathcal{O}_X^*) - \operatorname{rank} H^1(X,\mathcal{O}_X^*)$.

Tate accumulated evidence for his conjectures during 1963,[15] spoke about them at conferences in late 1963 and mid 1964, and published them in the proceedings of the first conference [7]. When asked about the origin of the Tate conjecture, Tate responded[16]:

> Early on I somehow had the idea that the special case about endomorphisms of abelian varieties over finite fields might be true. A bit later I realized that a generalization fit perfectly with the function field version of the Birch and Swinnerton-Dyer conjecture. Also it was true in various particular examples which I looked at and gave a heuristic reason for the Sato-Tate distribution.[17] So it seemed a reasonable conjecture.

In perhaps his most beautiful paper, Tate proved the isogeny conjecture over finite fields in 1966, and Artin and Swinnerton-Dyer proved Tate's conjecture for elliptic $K3$ surfaces over finite fields in 1973. Since then there has been little progress — it took another forty years and the efforts of several mathematicians to complete the proof of the Tate conjecture for $K3$ surfaces over finite fields. The Hodge conjecture is known in degree 2, and there is

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Mumford seem to believe Hodge's conjecture, it is now up to you to exhibit algebraic cycles corresponding to these abnormal classes. As I incline to disbelieve it, I shall rather attempt to show that there is no such cycle." (Letter from Tate to Serre, February 2, 1965.) The attempt of Mumford and Tate was fruitful, since it led them to introduce the Mumford-Tate groups, which "have emerged as the principal symmetry groups in Hodge theory" (Green, Mark; Griffiths, Phillip; Kerr, Matt. Mumford-Tate domains. Boll. Unione Mat. Ital. (9) 3 (2010), no. 2, 281–307).

[15] In his course in Fall 1962 (p56) Tate states as a "wild conjecture" that, for a surface over $\mathbb{F}_q$, the multiplicity of q as a root of $P_2(X,t)$ is the rank of the Picard group, and he briefly relates the conjecture to BSD. He explains the same conjecture in his 1962 ICM talk. In Tate's correspondence with Serre, the Tate conjecture is first mentioned in the letter of June 11, 1963, in which there is a mysterious THE CONJECTURE, which is never stated precisely, and is presumed to be known to Serre. It is clear from the letter than Tate already has most of the ideas he was to explain in his two conference talks.

[16] Tate 2011

[17] Sato found this distribution experimentally, and then moved on to other topics. Their names were linked again when Sato and Tate were jointly awarded the Wolf prize in 2002/3.

strong evidence for the Tate conjecture in the same degree, but beyond that both statements are generally considered to be open questions. However, the conjectures guide later research. For example, the form of the conjecture of Langlands and Rapoport,[18] which is basic to an understanding of the zeta function of a general Shimura variety, is shaped by the two conjectures.

6 Rigid analytic spaces and the Tate curve

The topology on the field $K = \mathbb{Q}_p$ of p-adic numbers is very different from the real topology. For example, every disk in K can be written as a disjoint union of arbitrarily many open-closed smaller disks, and so any local definition of "analytic function" leads to too many functions. Moreover, it is not possible to represent an elliptic curve as a quotient of K by a discrete lattice because K has no discrete subgroups.

However, there is an alternative representation of an elliptic curve E over $\mathbb{C}$, namely, $E(\mathbb{C}) = \mathbb{C}^\times/q^\mathbb{Z}$ for some $q \in \mathbb{C}$. When Tate took q to be an element of a p-adic field K with $|q| < 1$ he was "amazed and thrilled" to find that, suitably normalized, the classical power series converge and give an elliptic curve E over K whose points satisfy $E(K) = K^\times/q^\mathbb{Z}$. Curves arising in this way are called *Tate curves*. Higher dimensional analogues of Tate's construction have been found, and are essential to an understanding of the compactification of the moduli schemes of abelian varieties.[19]

Although this only realized $E(K)$ as a quotient of abstract groups, Tate understood from the start that the quotient should live in some category of "analytic spaces":

> Finally, and most important, this last theorem and probably many other things that are hard to prove at present, would become obvious if one really had a theory of analytic and meromorphic functions in complete non-archimedean fields ... How does one get around the total disconnectedness to get some kind of *global* theory? One really must try to make sense out of Krasner's stuff. I have not yet had the courage, however. But everything points to the existence of $\mathfrak{p}$-adic analytic continuation (letter to Serre, August 4, 1959).

When consulted by Tate, Grothendieck was initially negative:

> Nor do I have the impression of having understood this theorem at all; it does nothing more than exhibit, via brute formulas, a certain isomorphism of analytic groups (letter to Serre, October 19, 1961).

Only when Tate began constructing an underlying theory of analytic spaces did Grothendieck become more positive and helpful.[20]

So how did Tate resolve the problem noted above? He first defined a local theory in which the objects are ringed spaces $(X, \mathcal{O}_X)$ whose underlying set X has a p-adic topology. He then defined a collection of "admissible" open subsets and "admissible" open coverings. Although the admissible open sets and coverings don't form a topology in the usual sense, they satisfy the conditions necessary to support a sheaf theory (they form a Grothendieck

[18] Langlands, R. P.; Rapoport, M. Shimuravarietäten und Gerben. J. Reine Angew. Math. 378 (1987), 113–220.

[19] See Tate's letter to Serre, August 4, 1959, and Tate's comments CW p. 647.

[20] Correspondance Serre-Tate p. 791.

topology). In this way, Tate was able define global objects and a "locally" defined notion of analytic function: a function is analytic if it becomes analytic on the open subsets of some *admissible* covering. An acyclicity theorem of Tate shows that this gives a good theory.

Tate reported on his work in a series of letters to Serre, who had them typed and distributed by I.H.E.S.[21] They soon attracted the attention of the German school of complex analytic geometers and already by 1984 a comprehensive account of the theory required a book of over 400 pages. Tate's notes were eventually published as [9].[22] There have been a number of extensions of Tate's theory (Raynaud, Berkovich, ...), and p-adic analytic spaces are now as much a part of the landscape of arithmetic geometry as real analytic spaces.

7 The many other aspects of Tate's work

The above describes a part of Tate work during the 1950s and 1960s. Tate also worked on many different aspects of elliptic curves throughout his career, the K-theory of number fields, the Stark conjectures, commutative and noncommutative algebra, and other topics.

I now discuss the Collected Works (CW).

8 CW: the unpublished papers

These include: (paper 13) a three-page proof of the Riemann hypothesis for function fields (Weil's theorem) using ideas of Mattuck, Tate, and Grothendieck; (22) the notes of a seminar held as part of the 1964 Woods Hole conference in which Lubin, Serre, and Tate discuss some of their results on formal groups and abelian varieties, including Tate's proof of the Serre-Tate lifting theorem (see also letter L8 and comments); (38) a note in which Tate computes a certain subgroup (the tame kernel) of the K_2 of a number field; (51) a seven-page proof of the $\mathfrak{p}$-adic case of the Harish-Chandra transform for GL_r; (53) a manuscript in which Tate states and partially proves a conjecture extending the theorems of Shimura and Taniyama on abelian varieties with complex multiplication to all automorphisms over $\mathbb{Q}$.

9 CW: the letters

Two of the letters are to Dwork. In the first, Tate describes a parameterization of the p-adic points of an elliptic curve, and states a relation between a constant occurring in the parameterization and the zeta function of the curve. This was the spark that led to Dwork's remarkable p-adic proof of the rationality of the zeta function of a variety over a finite field. This is well explained in Tate's comments on the letter and in [2].

A letter to Springer explains Tate's proof, using Exts, of the arithmetic duality theorems for finite modules announced in his 1962 ICM talk [6]. Another to Birch discusses what is now called the Birch-Tate conjecture, and a letter to Atkin describes the construction by Tate and his students of the first example of an Artin L-series known to be holomorphic even though no power of it is a product of abelian L-series.

[21] "with(out) his permission" Serre-Tate correspondence, April 1962.

[22] Having been received by Inventiones math. on "November 31, 1962".

The remaining letters are a selection from the remarkable trove [1]. To comment on these would require an essay in itself.[23]

10 CW: Tate's comments on the papers

Some are corrections. All but three of these can be considered misprints. Of the three, one is a muddled proof of a (correct) lemma (CW I, p. 116),[24] one is a slightly misstated corollary (CW I, p. 424),[25] and only one can be considered serious. In his 1962 ICM talk [6], Tate overstated what his methods give concerning the strict cohomological dimension of the various Galois groups attached to number fields (CW I, p. 180).[26][27]

Some discuss the origins of papers,[28] for example ([10]),

> This is my first of several papers on K_2 of fields. I was introduced to the subject by Hyman Bass in the Fall of 1968, when we were both in Paris. He explained K_2 of fields to me via Steinberg symbols and the results of Calvin Moore described in §2 of this paper, then asked if I could see what $K_2(\mathbb{Q})$ was. I was able to answer his question...

While most of us are inclined to inflate our contributions, Tate's inclination seems to be the reverse. For example, he writes concerning [5]:

> This paper was written by Serre. He probably put my name on it because we had discussed some of the contents. But the discussion was very one-sided. He did most of the talking and I the listening.

Tate's letter to Serre, June 14, 1967, suggests a somewhat greater engagement. Tate had received "état 0" of the paper written by Serre and writes "I am now about ready to tackle the état 1 of our paper". What follows is a series of comments and criticisms ("concerning

[23] The comments on the letters by Serre in the Correspondance and by Tate in CW complement each other in interesting ways.

[24] 1957 Homology of Noetherian rings... The proof of Lemma 5 is obscure (the lemma is credited to Eilenberg, and the proof is an "outline shown me by Zariski"). However, the lemma is certainly true. The lemma says that if a certain homology group vanishes, then a ring is regular.

[25] 1970a (with Oort): Note for the last corollary (p. 21): Jean Roubaud pointed out to me that this corollary is not correct. The hypothesis should be that the narrow class number of the ring R be prime to $p-1$, for that is what is required to ensure that there is no non-trivial cyclic etale extension of R of degree dividing $p-1$. (CW p. 424).

[26] 1962d ICM talk. "As a by-product of the proof, one finds that the group G has strict cohomological dimension 2 for all primes l such that $lk_S = k_S$, except of course if $l = 2$ and k is not totally imaginary." In the CW, this passage is crossed out, and it is explained in detail in the comments. The statement is correct if S contains all primes.

[27] I'll add one. In his ICM 1962 talk, (3.3), Tate correctly states that the pairing on the Tate-Shafarevich group defined by a principal polarization is alternating when the polarization is defined by a divisor rational over the base field k. In his Bourbaki talk (1966, p306-06; CW Paper 23, p. 224) he omits the condition, and incorrectly states that the order of the Tate-Shafarevich group of a Jacobian is a square.

[28] For example (paper 6),

> Coming home from a party...

which, while amusing, fails to mention that Tate gained his sudden insight only after he had been "working on [the problem] for several months, off and on" (Tate 2011, p.450).

your idiotic lemma 1"), including the suggestion to replace "almost good reduction" with "potentially good reduction" (accepted).

Readers may be surprised to learn that Tate's contribution to a 2006 paper with Barry Mazur and William Stein [3] was "in doing the computer experiments discussed in section 6". However, Tate's letters to Serre (CW I, L18, L19, L20) reveal that as early as 1979, he was programming on a "hand-held Hewlett-Packard calculator on which one could write and run programs of 50 or fewer steps."

Concerning his great paper [7], Tate writes:

> This paper contains many optimistic thoughts, which have become known as the "Tate conjectures".

This is far from the exuberant way in which Tate announced the conjectures in a letter to Serre more than five decades earlier:

> At any rate, I am by now utterly convinced of the truth of THE CONJECTURE.

References

[1] *Correspondance Serre-Tate. Vol. I, II.* Documents Mathématiques (Paris), 13,14. Société Mathématique de France, Paris, 2015. Edited, and with notes and commentaries by Pierre Colmez and Jean-Pierre Serre.

[2] Nicholas M. Katz and John Tate. Bernard Dwork (1923–1998). *Notices Amer. Math. Soc.*, 46(3):338–343, 1999. CW 73.

[3] Barry Mazur, William Stein, and John Tate. Computation of p-adic heights and log convergence. *Doc. Math.*, (Extra Vol.):577–614, 2006. CW 78.

[4] J.S. Milne. The Work of John Tate. In Helge Holden and Ragni Piene, editors, *The Abel Prize 2008–2012*, pages 259–340. Springer, Heidelberg, 2014.

[5] Jean-Pierre Serre and John Tate. Good reduction of abelian varieties. *Ann. of Math. (2)*, 88:492–517, 1968. CW 33.

[6] John Tate. Duality theorems in Galois cohomology over number fields. In *Proc. Internat. Congr. Mathematicians (Stockholm, 1962)*, pages 288–295. Inst. Mittag-Leffler, Djursholm, 1963. CW 18.

[7] John Tate. Algebraic cycles and poles of zeta functions. In *Arithmetical Algebraic Geometry (Proc. Conf. Purdue Univ., 1963)*, pages 93–110. Harper & Row, New York, 1965. CW 21.

[8] John Tate. The cohomology groups of tori in finite Galois extensions of number fields. *Nagoya Math. J.*, 27:709–719, 1966. CW 25.

[9] John Tate. Rigid analytic spaces. *Invent. Math.*, 12:257–289, 1971. CW 36.

[10] John Tate. Symbols in arithmetic. In *Actes du Congrès International des Mathématiciens (Nice, 1970), Tome 1*, pages 201–211. Gauthier-Villars, Paris, 1971. CW 35.

[11] John Tate. Galois cohomology. In *Arithmetic algebraic geometry (Park City, UT, 1999)*, volume 9 of *IAS/Park City Math. Ser.*, pages 465–479. Amer. Math. Soc., Providence, RI, 2001. CW 74.