The Perfectoid Concept: Test Case for an Absent Theory

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Perfectoid prologue

It's not often that contemporary mathematics provides such a clear-cut example of concept formation as the one I am about to present: Peter Scholze's introduction of the new notion of *perfectoid space*. The 23-year old Scholze first unveiled the concept in the spring of 2011 in a conference talk at the Institute for Advanced Study in Princeton. I know because I was there. This was soon followed by an extended visit to the Institut des Hautes Études Scientifiques (IHES) at Bûres-sur-Yvette, outside Paris — I was there too. Scholze's six-lecture series culminated with a spectacular application of the new method, already announced in Princeton, to an outstanding problem left over from the days when the IHES was the destination of pilgrims come to hear Alexander Grothendieck, and later Pierre Deligne, report on the creation of the new geometries of their day. Scholze's exceptionally clear lecture notes were read in mathematics departments around the world within days of his lecture — not passed hand-to-hand as in Grothendieck's day — and the videos of his talks were immediately made available on the IHES website. Meanwhile, more killer apps followed in rapid succession in a series of papers written by Scholze, sometimes in collaboration with other mathematicians under 30 (or just slightly older), often alone. By the time he reached the age of 24, high-level conference invitations to talk about the uses of perfectoid spaces (I was at a number of those too) had enshrined Scholze as one of the youngest elder statesmen ever of *arithmetic geometry*, the branch of mathematics where number theory meets algebraic geometry.) Two years later, a week-long meeting in 2014 on Perfectoid Spaces and Their Applications at the Mathematical Sciences Research Institute in Berkeley broke all attendance records for "Hot Topics" conferences.

Four years after its birth, *perfectoid geometry*, the theory of perfectoid spaces, is a textbook example of a *progressive research program* in the Lakatos sense. It is seen, retrospectively, as **the right** theory toward which several strands of arithmetic geometry were independently striving. It has launched a thousand graduate student seminars (if I were a historian I would tell you exactly how many); the students' advisors struggle to keep up. It has a characteristic terminology, notation, and style of argument; a growing cohort of (overwhelmingly) young experts, with Scholze and his direct collaborators at the center; a domain of applications whose scope continues to expand to encompass new branches of mathematics; an implicit mandate to unify and simplify the fields in its immediate vicinity. Last, but certainly not least, there is the generous, smiling figure of Peter Scholze himself, in the numerous online recordings of his lectures or in person, patiently answering every question until his questioner is satisfied, still just 27 years old, an inexhaustible source of revolutionary new ideas.

For the historian of concepts (which I am not), there is only one problem with this picture: the concept of a perfectoid space is one of the most difficult notions ever introduced in arithmetic geometry, which has a long tradition of difficult notions. The reader of this essay faces a second problem: not only am I not a historian of concepts, I am by no means an expert on the kinds of mathematics that have been put together to create the new concept of perfectoid spaces. So I will have to skim the surface, at the risk of distortion; but even the surface will be unfamiliar for most readers. I hope that the technical details will nevertheless provide the reader with the means of perceiving how the somewhat extreme case of perfectoid spaces exemplifies typical aspects of mathematical concept formation: the reference to a historical background (in this case, the long practice of studying properties of equations in number theory by relating them to properties of geometric objects), the identification of a specific range of open questions in this historical context and the expectation that they would be solved by the introduction of new techniques of a specific type, and the reception and general acceptance of the new concept as **the right** one because it met expectations in spite of its many novel features and because it lent itself immediately to solving outstanding open problems.

Starting points

Perfectoid spaces stand at the crossroads where topology, Galois theory, and the study of equations by means of congruences meet. It's a busy crossroads, already occupied by *étale cohomology* and *crystalline cohomology*, both created by Grothendieck and his school; by the new number systems (*Fontaine rings*) created by Jean-Marc Fontaine in his program to devise a *mysterious functor*, a canonical operation relating the two kinds of cohomology; and by the many technical innovations introduced in order to complete Fontaine's program. The most relevant of these for Scholze's work is the *almost mathematics* of Gerd Faltings, which in turn builds on the foundational work of John Tate, 90 years old and still active. Tate is also the creator of *rigid analytic spaces*, the first of several successful attempts to create a *p-adic geometry*. Perfectoid geometry doesn't transcend these previous attempts but rather extracts those of their features that are relevant to the problems Scholze set out to solve.

I don't expect readers to possess any of the technical vocabulary introduced in the previous paragraph. Each of the technical terms, however, can be seen as the precise analogue of a familiar notion from higher-dimensional geometry in Euclidean space; for the purposes of this essay, much of the meaning of Scholze's innovations can be understood by reference to these analogies. I will start with the way differential calculus encodes topology, and specifically with *Green's theorem* in calculus in two variables (as a topic in second year calculus, no other starting point is nearly as elementary). Green's theorem relates the line integral of a differential

$$fdx + gdy$$

on a simple closed curve C with the double integral of an expression involving the partial derivatives of f and g on the region D bounded by the curve. This breaks down when D is no longer a region bounded by a simple closed curve, but rather a region with holes. The breakdown is an expression of the complicated *topology* of D — the presence of holes — but Green's theorem has a generalization — a version of *Stokes' theorem* that accounts for the holes. The generalization of these theorems to a manifold M of arbitrary dimension relates the topology of M — and more precisely, the *cohomology* of M, which is the way of keeping track of generalized holes — to the possibility or impossibility of integrating differential forms on M. For general (compact) manifolds the relation is the statement of *de Rham's theorem*. When M is the space of complex solutions (in projective space) to a system of *polynomial equations* — a (smooth, projective, complex) algebraic variety — then *Hodge theory* gives a more precise relation, where the distinction between holomorphic differentials (like dz) and antiholomorphic differentials (like $d\bar{z}$) is central. For the purposes of this discussion, the important fact about Hodge theory is that it uses differential calculus to compute the topology, or more precisely the cohomology, of a complex algebraic variety M. Another fruitful way of saying the same thing is to say that M has *two kinds* of cohomology groups — the topological one (that keeps track of holes), often called *Betti cohomology*, and the analytic one, based on the algebra of differential forms, called *de Rham cohomology*. Then the *comparison theorem* of Hodge theory — at this stage it's really de Rham's theorem — says that these are two different ways of computing the same thing.

Now forget the algebraic variety M but keep the polynomial equations used to define M. What happened to the holes? In the 1960s Grothendieck and his collaborators constructed a purely algebraic invariant[1] of an algebraic variety — not necessarily smooth, not necessarily projective, and over any coefficient field F — that recovered the topological (Betti) cohomology groups, in the process transforming the notion of *topological space* practically beyond recognition. Something was gained and something was lost. What was gained was that the cohomology groups acquired an action of the Galois group of the coefficient field F — and when F is the field of rational numbers, or a finite field, this defines an invariant of exceptional richness. The *Tate Conjecture*, which has been proved in only a few situations (notably by Tate and Faltings), asserts that this Galois representation encodes a good deal of the information about solutions of the polynomial equations and higher-dimensional generalizations of solutions (whatever one might mean by that). What was lost is that, whereas Betti cohomology groups actually count holes (more precisely, they are finitely generated abelian groups), the algebraic invariants introduced by Grothendieck and his collaborators, the *étale cohomology* groups, only count holes modulo N for all integers N — with the important proviso that N is *not divisible by the characteristic of the coefficient field* F. It is more useful to package this information by prime powers and to look at the l-adic (étale) cohomology, which puts together the étale cohomology modulo l^n for all n, where l runs through the set of prime numbers that are invertible in the field F.[2] The underlying assumption is that the number of holes in an algebraic variety, or even a 2-dimensional manifold, is a fundamental geometric property — an invariant.

Let us pause to consider that set of prime numbers l invertible in a fixed field F, since this is the most characteristic property of the field, and because the raison d'être of Scholze's work is to answer questions about equations over p-adic fields

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[1] André Weil had constructed such an invariant for varieties of dimension 1, speculated that such a theory should exist in general, and conjectured some of its desirable properties. Grothendieck proved the Weil conjectures, with the exception of the last one, which was solved in 1973 by Pierre Deligne. I was at the IHES, not yet a graduate student, the day the 29-year-old Deligne announced his solution. It was seen, correctly, as a very big deal.

[2] For a philosophically enlightening introduction to the topological approach to number theory, see §3 of C. McLarty,"What does it take to prove Fermat's Last Theorem? Grothendieck and the logic of number theory." *Bulletin of Symbolic Logic* 16.03 (2010): 359-377.

(where some standard methods of algebraic geometry don't apply) by "tilting" the equations so they are now over fields of characteristic p. In fact, the *characteristic* of F is decreed to be either the set of primes that are *not* invertible in F, if there are such, or else zero, if all primes are invertible in F. A field is of characteristic zero if and only if it contains the rational field **Q**; for example, the familiar fields of real or complex numbers are of characteristic zero. Otherwise, there is exactly one prime, traditionally denoted p, which is not invertible in F, and F is said to be of (positive) characteristic p. Since every non-zero element of F is invertible, this means that p = 0 in F, and F contains the *prime field* $\mathbf{F}_p = \mathbf{Z}/p\mathbf{Z}$ of p elements, the field whose arithmetic corresponds to the study of congruences modulo p. Scholze's initial motivation, very roughly speaking, was to study equations over certain kinds of fields of characteristic zero by reinterpreting them as equations over fields of prime characteristic p, where other methods are available.

A field F of characteristic p > 0 has two closely related properties that it does not share with fields of characteristic zero.

(i) First, polynomials of positive degree can have derivative zero: indeed, if $P(X) = X^p$, then $P'(X) = pX^{p-1} = 0$ because p = 0.[3]

(ii) On the other hand, the map *Frob* (for Frobenius) that sends $a \in F$ to its p-th power *Frob*(a) = a^p is a *homomorphism* of rings:

$$Frob(ab) = Frob(a)Frob(b) \text{ and } Frob(a+b) = Frob(a) + Frob(b)$$

for any a, b $\in$ F. The multiplicative property is obvious, the additive property follows from the binomial formula for $(a+b)^p$, because all the intermediate binomial coefficients are divisible by p. Because F is a field, only 0 has p-th power zero, and thus *Frob* is injective. The field F is called *perfect* if *Frob* is also surjective: in other words, if every element of F has pth roots (and therefore p^nth roots for all n). Scholze's term *perfectoid* is derived from this property of perfect fields.

Algebraic varieties over any field still have differential forms, as in our earlier discussion and there is an algebraic version of de Rham cohomology (whose main properties were outlined in a letter[4] from Grothendieck to Atiyah). But to work properly with differentials of degree n, one needs to be able to divide by n!, and this is impossible if the characteristic of F is a prime dividing n; alternatively, property (i) above shows that one can't integrate the differential $X^{p-1}dX$. Expanding the notion of space a bit more, Grothendieck outlined the properties of yet another algebraic cohomology theory (for smooth projective varieties), *crystalline cohomology*, whose construction was carried out in the thesis of his student Pierre Berthelot and his collaborators. It was around this time that Grothendieck abandoned the IHES and the mathematical community, but not before he had asked for an analogue of Hodge

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[3] Didn't I just say that p > 0? So how can I say p = 0? The prime number p is and remains a positive number in the field of rational numbers, but for the purposes of arithmetic in a field of characteristic p it is treated as if it were equal to 0, and indeed this is the meaning of characteristic p. There is no logical inconsistency.

[4] A. Grothendieck, On the de Rham cohomology of algebraic varieties, *Publ. math. de l'I.H.É.S.* tome **29** (1966), p. 95-103.

theory in the algebraic setting. The original Hodge theory was defined for varieties with complex coefficients; Grothendieck expected an analogue for varieties over *p-adic local fields*. For our purposes a field F is p-adic local if it is a topological field with an open subring $O = O_F$ such that F is obtained from O by allowing division by p, and such that O is a local ring (it has a unique maximal ideal m) and a discrete valuation ring (every ideal is a power of m such that $k = O/m$ is a finite field). When F is a p-adic local field, the algebraic de Rham cohomology, crystalline cohomology, and p-adic étale cohomology all have coefficients in the same kind of field, namely either F or a p-adic local field closely related to F. Grothendieck asked for a "mysterious functor" that played the role of the de Rham and Hodge theorems in relating the p-adic étale cohomology of an algebraic variety over F to its algebraic de Rham cohomology, and (when this makes sense) to the crystalline cohomology of its reduction over k. A few years earlier, Tate had introduced what are now called *Hodge-Tate structures* and, together with Jean-Pierre Serre, had formulated a different conjectural p-adic version of the Hodge theorem; Grothendieck's proposal would necessarily imply Tate's conjecture (not to be confused with "the Tate conjecture" mentioned earlier).

Fontaine introduced his new rings, including the ones called B_{dR} and B_{cris}, as a way of relating the Galois structure and the differential structure, and used them to define a candidate for Grothendieck's mysterious functor. There ensued an international contest, lasting nearly two decades, to prove that Fontaine's approach provided the mysterious functor. Along the way the field saw the growth of the new research program of *p-adic cohomology*, characterized by the proliferation of new cohomology theories with p-adic coefficients, including *syntomic cohomology*, *de Rham-Witt cohomology*, and *rigid cohomology*, and centered around the problem of realizing Grothendieck's mysterious functor.[5]

The main goal of the program was to find better ways to understand the Galois representations constructed by Grothendieck, because they are among the central objects of algebraic number theory. Results of this research program were of fundamental importance in Andrew Wiles's proof of Fermat's Last Theorem and in subsequent work that derived from that of Wiles. Especially in the hands of Richard Taylor and his students and collaborators, this line of research has developed into one of the most active research programs in algebraic number theory, overlapping with the *Langlands program* on automorphic forms — and only incidentally in geometry. This is largely responsible for the growing familiarity of number theorists with Fontaine's p-adic constructions in the years preceding Scholze's perfectoid announcements.

A word about Fontaine's rings is in order, because of their importance for the theory of perfectoid spaces. These rings, which can be thought of as alternative p-adic analogues of the complex number field, are constructed by an elaborate series of steps,[6] of which the first invariably involves taking p^nth roots for all n in a field of characteristic zero in order to define a new and perfect field of characteristic p. In

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[5] It was mysterious because Grothendieck didn't know how to define it but assumed it must exist. Then Fontaine defined a functor and over the years he and others showed that it had the expected properties.

[6] See, for example, http://en.wikipedia.org/wiki/Ring_of_p-adic_periods.

retrospect, this way of moving from characteristic 0 to characteristic p can be seen as a prototypically perfectoid thing to do. As recently as 15 years ago, any talk at a number theory seminar involving one of Fontaine's rings would normally begin with a reminder of the ring's construction. I never found this very enlightening, because it was never explained why this particular sequence of otherwise unmotivated steps yielded an interesting object — in other words, the construction did not illuminate the *concept* of the Fontaine ring. These days, the rings tend to be introduced without explanation, which I take to mean that the seminar audience has become sufficiently familiar with the rings not to need to be reminded that the speaker knows how they are put together. In practice, this means that a typical (algebraic) number theorist-in-training is expected to know a few standard properties of Fontaine's rings; one might (or might not) want to say in this connection that the number theorist-in-training has been initiated into one or more of the *language games* in which Fontaine's rings feature prominently. More to our purpose, this familiarity means that the community of participants in number theory seminars was prepared for Scholze's theory of perfectoid spaces by repeated exposure to the theory and use of Fontaine's rings, largely in connection with outgrowths of the ideas of Wiles and Taylor.

To put an end to this lengthy introduction to the background to Scholze's work, Fontaine's hope to have realized the mysterious functor was confirmed, not once but several times, giving rise to what is now generally known as *p-adic Hodge theory*. The first complete proof was obtained by Faltings; a rather different method, extending ideas due to Fontaine and William Messing, was developed by Japanese specialists, culminating in a second (and initially more complete) complete proof by T. Tsuji. Two relevant features of the Faltings approach deserve to be mentioned here. The first feature is the introduction of what Faltings called *almost mathematics*,[7] which is (very roughly) a form of commutative algebra in which certain kinds of error are systematically ignored, and in which it is shown rigorously that they do not matter to the final proof of Fontaine's comparison. The second feature is that Faltings made systematic use of constructions involving the taking of p^nth roots for all n. Faltings said at the time that his constructions were inspired by Tate's original work on p-adic Hodge theory.

Scholze's perfectoid concept

Scholze's perfectoid spaces are, in the first place, *spaces*. The notion of space in algebraic geometry has evolved through several stages since André Weil introduced abstract varieties by the gluing together of affine algebraic varieties in his *Foundations of Algebraic Geometry*. The current understanding is based on Grothendieck's framework, developed systematically by Grothendieck and Dieudonné in the *Éléments de géométrie algébrique* (EGA), following Serre's introduction of sheaf-theoretic methods. A *space* in this setting then consists of a topological space

[7] The first complete treatment is contained in O. Gabber and L. Ramero, *Almost ring theory*, *Lecture Notes in Mathematics* **1800**, Berlin: Springer-Verlag, (2003). Faltings' proof is in G. Faltings, p-adic Hodge theory, *J. Amer. Math. Soc.* **1** (1): 255–299 (1988). The "certain kinds of error" mentioned in the next sentence can be likened to measurement errors in physics. It's as if one were using two different scales to make multiple measurements of the geometry of an algebraic variety; the scales can give slightly different measurements in the intermediate steps but in almost mathematics they are treated as identical. It turns out that the final result is insensitive to this kind of error.

(the "underlying topological space," generally not Hausdorff) together with the (sheaf of) functions defined on its open sets. I pause to mention that Grothendieck's reconceptualization of topology involves a generalization of the notion of "open set" that is radically at odds with the primitive intuition of continuity that general topology was designed to formalize but that nevertheless works magnificently.

In p-adic geometry, the topology is somehow adapted to the topology of the p-adic numbers, and the functions take p-adic values. Perfectoid spaces are spaces in the setting of p-adic geometry, and more precisely p-adic *analytic* geometry. Like the p-adic numbers themselves, p-adic geometry is a form of geometry designed to study solutions of congruences modulo powers of p. The p-adic numbers themselves form a topological space, but it is totally disconnected, which means that the space can be broken continuously into arbitrarily small pieces. This is not suitable for geometry; there are too many continuous functions. One could disallow all continuous functions that are not polynomials, or quotients of polynomials; but that would yield algebraic geometry, not p-adic analytic geometry. On the other hand, allowing all analytic functions — convergent power series — would again not be geometric. Tate was the first to propose a workable compromise with his theory of *rigid analytic spaces*.

Tate's solution to the continuity problem was versatile, ingenious, and thoroughly in the spirit of Grothendieck's reformulation of topology. It also had a number of immediate applications to traditional problems in arithmetic geometry, notably the construction of the *Tate elliptic curve*, which still serves as a model for applications of Tate's rigid geometry. The field of *rigid analysis* that developed on the basis of Tate's (long unpublished) founding document has its own canon and characteristic vocabulary; especially after it was extended by Michel Raynaud and Pierre Berthelot, it has grown into a healthy subfield of arithmetic geometry, a topic with its own practice and a resource for those in neighboring fields. However, it was not the last word; specifically, it lacked a native cohomology theory along the lines of étale cohomology, and the rigid analytic version of de Rham cohomology (*Monsky-Washnitzer cohomology*, later incorporated in Berthelot's *rigid cohomology*) lacked certain desirable features. This pointed to a failure of p-adic Hodge theory to be a true analogue of complex Hodge theory. Just as complex algebraic varieties are also complex analytic manifolds, p-adic algebraic varieties are also rigid analytic spaces. Hodge's Hodge theory is based on complex analysis, but the proofs of p-adic Hodge theory were purely algebraic. In particular, there was no p-adic analytic way to interpret the algebraic invariant of étale cohomology.

Three distinct versions of étale cohomology in the setting of p-adic analysis were proposed in the early 1990s, due respectively to Kazuhiro Fujiwara, Vladimir Berkovich, and Roland Huber. Each of these approaches involved a more or less radical expansion of the kinds of spaces considered in p-adic geometry, and each of them had immediate applications.[8] Huber's theory of *adic spaces* had relatively few adherents, however, until it was revived by Scholze as the setting in which perfectoid spaces naturally coexisted with other kinds of p-adic algebraic and analytic structures.

[8] This, by the way, is where I enter, and exit, the subject. The work discussed in Chapter 9 of my *Mathematics without Apologies* was only possible because Berkovich had recently found a way to define l-adic cohomology for his analytic spaces. Fujiwara's theory played a crucial role in my subsequent work with Richard Taylor.

Before I introduce Scholze's perfectoid spaces, I need to devote a few words to the curious fact that algebraic geometry is so thoroughly algebraic that it can theoretically be developed without any reference to geometry whatsoever. The basic spaces in any version of algebraic geometry are the *affine* spaces. For Grothendieck, any commutative ring R defines an affine space — an affine *scheme* — denoted Spec(R), with the tautological property that R is the ring of functions on Spec(R). The correspondence between R and Spec(R) is an *equivalence of categories*, which means that everything you need to know about Spec(R) can be deduced from what you know about R, and vice versa.[9] Because Spec(R) is a kind of space, it has points, which are very convenient when you want to compare Spec(R) to Spec(R') for two rings R and R'. The general space in Grothendieck's version[10] of algebraic geometry is a *scheme* which is a topological space X that is covered by open sets that are *affine schemes*; in other words, each of whose points belongs to some Spec(R), or rather to a (generally infinite) collection of Spec(R'), compared in an appropriate way. One usually says that X has been constructed by *gluing* the affine schemes it contains. The geometric properties of schemes that can be reinterpreted as properties of their affine coverings — in other words, as properties of rings — are called *local*.

The prototype for constructing complicated spaces from simpler spaces is the theory of (topological or differentiable) *manifolds* in topology or differential geometry; the simpler spaces are just miniature versions of Euclidean space of dimension n, and a manifold is defined as a topological space that "looks locally" like Euclidean space. Differential geometry is to differential calculus as algebraic geometry is to ring theory.

In the various versions of p-adic geometry derived from Tate's rigid analytic geometry, spaces are constructed out of rings in the same way, except that the word *affine* is replaced by *affinoid*. Here is Scholze's definition of a perfectoid algebra (ring), copied from his article in the Proceedings of the 2014 International Congress of Mathematicians (ICM) at Seoul:

> **Definition 5.1.** A perfectoid K-algebra is a Banach K-algebra R for which the subring $R^\circ \subset R$ of powerbounded elements is a bounded subring, and such that the Frobenius map $\Phi : R^\circ/p \to R^\circ/p$ is surjective.

Everything in the definition is important, but the most important part is the last line: it says that one can take p^nth roots for all n in this ring R°/p. To begin with, this allows Scholze to introduce the *tilt* of R, denoted $R^\flat$; the process of tilting turns a perfectoid algebra of characteristic zero into a perfectoid algebra of characteristic p in such a way that all the algebraic structures relevant to étale cohomology (among others) are preserved under the operation. The geometric objects Scholze attaches to the rings R and $R^\flat$ are adic spaces in Huber's sense; these are the *affinoid perfectoid spaces* that can then be *glued* together to form general perfectoid spaces.

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[9] However, this equivalence is *contravariant*; a comparison map (ring homomorphism) R → R' is equivalent to a comparison map of schemes Spec(R') → Spec(R).

[10] There are still more general versions but this will do for now.

The tilting operation originates in the early stages of p-adic Hodge theory, and specifically in a theorem of Fontaine and Jean-Pierre Wintenberger that identifies the Galois theory of certain infinite extensions of p-adic fields with the Galois theory of what are now called their tilts, which are fields of characteristic p. This operation is applied as the first step in constructing the most familiar of the Fontaine rings. Having learned from Grothendieck that any operation on a class of rings should be viewed as a *local* version of an operation on the corresponding class of spaces, arithmetic geometers naturally want to define a class of spaces to which the Fontaine-Wintenberger tilt extends that will for this reason be recognized as **the right** class of spaces. Several attempts were made, more or less rooted in the methods Faltings introduced in his work on p-adic Hodge theory; it is now acknowledged that Scholze's perfectoid concept is **the right** one for rings, and **the right** one for gluing the local pieces together into global geometric objects. The proofs made extensive use of the Faltings "almost ring theory."[11]

"Category" is the formalized mathematical concept that currently best captures what is understood by the word "concept." Scholze defined perfectoid spaces as a category of geometric spaces with all the expected trappings, and thus there's no reason to deny it the status of "concept." I will fight the temptation[12] to explain in any more detail just why Scholze's perfectoid concept was seen to be **the right** one as soon as he explained the proofs in the (symbolically charged) suburban setting of the IHES. But I do want to disabuse the reader of any hope that the revelation was as straightforward as a collective process of feeling the scales fall from our eyes. Scholze's lectures and expository writing are of a rare clarity, but they can't conceal the fact that his proofs are extremely subtle and difficult. Perfectoid rings lack familiar finiteness properties — the term of art is that they are not *noetherian*[13]. This means that the unwary will be systematically led astray by the familiar intuitions of algebra. The most virtuosic pages in Scholze's papers generally involve finding ways to reduce constructions that appear to be hopelessly infinite to comprehensible (finite type) ring theory. This is my contribution to speculation about why Scholze succeeded so brilliantly where so many outstanding mathematicians failed.

The concept's reception

Scholze's first lectures on perfectoid spaces already included a stunning application, namely the proof of the weight-monodromy conjecture, in the special case of complete intersections in projective space (extended in the published version to complete intersections in toric varieties) over p-adic fields. As was mentioned in the Prologue, this question was left unsolved by Grothendieck and Deligne. The latter had indeed proved the complete conjecture over the characteristic p analogues of p-

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[11] As formalized and systematized by O.Gabber and L. Ramero in *Almost ring theory*, volume 1800 of *Lecture Notes in Mathematics*. Springer-Verlag, Berlin, 2003. Gabber, who is based at the IHES, attended Scholze's lectures, and I can't resist mentioning how much arithmetic geometry is indebted to Gabber's constantly insightful questioning, which has kept the field honest for over 30 years. At several points during his lectures Scholze thanked Gabber for forcing him to clarify his ideas.

[12] I don't have to fight very hard, because I only barely understand the proofs myself. The interested reader can find the details in Scholze, P., Perfectoid spaces. *Publ. Math. Inst. Hautes Études Sci.* **116** (2012), 245–313.

[13] After Emmy Noether, the founder of modern algebra.

adic fields — I can assure the reader that this was a very hard thing to do — and Scholze's methods allowed him to "tilt up" Deligne's proof for the cases he was able to settle.

Six months before his IAS lectures, Scholze was seen as an exceptional talent by many colleagues, but not by everyone; a prestigious journal foolishly rejected one of his pre-perfectoid papers (which was later published in an even more prestigious journal). His proof of the weight-monodromy conjecture was enough to guarantee that this would not happen again. My understanding was that Scholze invented perfectoid spaces in order to "tilt up" Deligne's proof of the weight-monodromy conjecture, but it was clear from the outset that the concept would have many more applications.[14] A few months after his IHES lectures, a French graduate student asked whether I would be willing to be his thesis advisor; things started conventionally enough, but very soon the student in question was bitten by the perfectoid bug and produced a Mémoire M2 — a mainly expository paper equivalent to a minor thesis — that was much too complicated for his helpless advisor.[15] By then Scholze had found two new spectacular applications that the precocious student managed to cram into his Mémoire M2, making it by far the longest Mémoire it has even been my pleasure to direct.

Although there have been more applications in the meantime — and I will mention at least one of them — I want to devote a moment to the two that were discovered in the year immediately following the IHES lectures, because they served to cement the idea that perfectoid spaces provide the right framework to adapt the constructions of complex Hodge theory in the p-adic setting. The first application was an (unexpectedly general) extension of the main theorems of p-adic Hodge theory to (proper) rigid-analytic varieties. This includes a new proof of the theorem of Faltings and Tsuji mentioned above; of the four existing proofs (the fourth, slightly later, is due to W. Niziol), Scholze's, in its large structure and in its local arguments, is the one that most closely resembles the proofs of the main theorems of complex Hodge theory. The second application, joint with Jared Weinstein, was a classification of p-divisible groups over a complete algebraically closed p-adic field. All I want to say about p-divisible groups, is that they are objects of fundamental importance in arithmetic geometry and number theory, that they are distantly related to the Abel-Jacobi theory of period integrals on algebraic curves, and that there has been a dizzying variety of classification schemes for p-divisible groups, each one useful for one purpose or another. Again, of all the classifications, the one of Scholze and Weinstein, apart from being perhaps the easiest to remember (though certainly not the easiest to prove), is the one that most closely resembles its complex counterpart. In both cases, the familiarity of the complex analogue, which is an advanced but necessary part of the training of practically any mathematician, reinforces the impression that Scholze has found the right way to think about p-adic

[14] During the breaks in Scholze's lectures there was active speculation about the applicability of his concept to one or another favorite problem.

[15] The student in question — who has taken on by a second, more competent advisor — has not yet finished his thesis but I would already count him as a member of the second perfectoid circle revolving around Scholze. The first circle, as I see it, includes Scholze's immediate collaborators and a few others; the second circle is already much broader, and there is a third circle consisting of everyone hoping to apply the concept to one thing or another.

geometry. And Scholze has himself contributed to this impression in his expository writing about perfectoid spaces.

Most of the blog posts, expository articles, and letters of reference I've seen agree that the outstanding applications of perfectoid geometry are contained in Scholze's paper entitled "On torsion in the cohomology of locally symmetric spaces." A great deal can be said about this paper, which solved several outstanding conjectures in the course of vastly improving and generalizing an earlier paper[16] on a related topic. For the purposes of this essay, the importance of Scholze's paper is that it confirmed that perfectoid geometry provides the right framework for thinking about a number of central questions in algebraic number theory and, secondarily, that the cohomology of perfectoid spaces has the right (p-adic integrality) properties for applications to such questions.[17] And it confirmed the impression that number theorists had made the right decision to devote time to learning Scholze's new framework even before it had been shown to have important applications to their field.

Discussion

I began writing this essay three years after Scholze's IHES lectures and one month after his ICM lecture in Seoul. One year earlier, I could safely assert that no one had (correctly) made use of the perfectoid concept except in close collaboration with Scholze. The Seoul lecture made it clear that this was already no longer the case. Now, after nearly a year has passed, the perfectoid concept has been assimilated by the international community of arithmetic geometers and a growing group of number theorists, in applications to questions that its creator had never considered. It is an unqualified success.

How can this be explained? Mathematicians in fields different from mine are no better prepared than philosophers or historians to evaluate our standards of significance. One does occasionally hear dark warnings about disciplines dominated by cliques that expand their influence by favorably reviewing one another's papers, but by and large, when a field as established and prestigious as arithmetic geometry asserts unanimously that a young specialist is the best one to come along in decades, our colleagues in other fields defer to our judgment.

Doubts may linger nonetheless. I don't think that even a professional historian would see the point in questioning whether Scholze is exceedingly bright, but is his work really that important? How much of the fanfare around Scholze is objectively legitimate, how much an effect of Scholze's obvious brilliance and unusually appealing personality, and how much just an expression of the wish to have something to celebrate, the "next big thing"? Is a professional historian even allowed to believe that (some) value judgments are objective, that the notion of **the right** concept is in any way coherent? How can we make sweeping claims on behalf of

[16] a joint paper by Michael Harris, Kai-Wen Lan, Richard Taylor, and Jack Thorne entitled "On the rigid cohomology of certain Shimura varieties."

[17] The authors of the paper cited in the previous footnote — no doubt like many others working on similar questions — were actively seeking a p-adic cohomology theory with just these properties.

perfectoid geometry when historical methodology compels us to admit that even complex numbers may someday be seen as a dead end? "Too soon to tell," as Zhou En-Lai supposedly said when asked his opinion of the French revolution.

It's possible to talk sensibly about convergence without succumbing to the illusion of inevitability. In addition to the historical background sketched above, and the active search for the right frameworks that many feel Scholze has provided, perfectoid geometry develops themes that were already in the air when Scholze began his career.[18] With respect to the active research programs that provide a field with its contours, it's understandable that practitioners can come to the conclusion that a new framework provides the clearest and most comprehensive unifying perspective available. When the value judgment is effectively unanimous, as it is in the case of perfectoid geometry, it deserves to be considered as objective as the existence of the field itself.

A value judgment applied to a new concept thus becomes an inflection or modality of a judgment of existence. I don't have much time for debates over the independent existence of the objects of mathematical practice, but I consider the existence of mathematical practice to be as objective as any other factual judgment. In that sense, I don't need to worry that the judgments by which my field's practice defines itself might be seen as tautological; what else could they be?

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[18] These include the *Fargues-Fontaine curve*, a unifying construction due to Fontaine and Laurent Fargues, announced just over a year before Scholze's IAS lectures and generalized by Scholze, and work of K. Kedlaya and R. Liu on *relative p-adic Hodge theory*. Scholze described both of these in detail in his 2014 ICM talk.