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BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 1, Number 3, May 1979

0002-9904/79/0000-0212/$02.75

*Algebraic geometry*, by Robin Hartshorne, Graduate Texts in Mathematics 52, Springer-Verlag, New York, Heidelberg, Berlin, 1977, xvi + 496 pp., $24.50.

After its inception as part of Bernhard Riemann's new function theory, Algebraic Geometry quickly became a central area of nineteenth century mathematics. The theory of "abelian functions" (that is, meromorphic functions on an algebraic complex torus) was regarded as an acme of function theoretic thought and at the very heart of this beautiful creation of the German school. It was also recognized that earlier mathematicians such as Legendre, Euler, Abel, Gauss, and Jacobi (to name a few of the more outstanding) had made substantial contributions to the subject, albeit in a purely function-theoretic and analytic disguise. Moreover, number theorists and arithmeticians began to sense connections between their interests on the one hand and function theory on compact Riemann surfaces on the other. (Riemann had proved such surfaces to be algebraic.) In this regard, there was a whole flurry of contributions by the likes of Eisenstein, Kummer, Kronecker, Weber, Fueter, and Hensel–not to mention the redoubtable Hilbert.

Algebraic Geometry was a very active area in the late nineteenth century, especially with the added significant results of Picard, Hurwitz, Klein, and Poincaré. The German School of Brill and Noether created a predominantly geometric theory of one-dimensional algebraic varieties (that is, of two-dimensional spaces over the reals which arise as the set of zeros of complex polynomials either in ordinary space or in projective space). They understood how to "resolve" singularities (so that the resulting variety would be a one-dimensional complex analytic manifold in the present day sense), how to compute the numerical invariants associated to their varieties, and they began to scratch the surface in a theory of higher dimensional phenomena.

Starting about 1890, the brilliant Italian School studied mainly algebraic varieties of dimension two. The phenomena here were much more complicated and bewildering than in the case of compact Riemann surfaces. Nevertheless, with a staggering geometric insight, no doubt sharpened by encounters with innumerable examples, the Italian school uncovered the major phenomena and introduced extremely fruitful methods into the subject. They discovered numerical criteria for when the function theory on an algebraic surface was the same as the function theory on the projective plane (Castelnuovo's criterion of rationality), numerical criteria for when the function theory on an algebraic surface was the same as that on a projective fibre bundle (with fibre $\mathbf{P}^1$) over a compact Riemann surface (Enriques' criterion of ruledness), numerical criteria for the contractibility of a curve in a surface to a point so that the resulting surface remained a manifold, and they gave a classification of surfaces by the values of certain invariants.

Unfortunately, their results were complicated by the fact that they regarded two algebraic varieties as "the same" when they possessed the same field of meromorphic functions (birational equivalence) rather than when they were geometrically isomorphic. While the Italians recognized this and attempted to deal with it, they were only partially successful, and this only for algebraic surfaces. Moreover, this difference between algebraic geometry and the other geometric theories then in formation led to a serious lack of communication between algebraic geometry and these other geometric theories. It caused algebraic geometry to lose its central place in mathematics. There was also a problem of rigor in the proofs of the Italian School. The Italian proofs were permeated with strong geometric inventiveness and insight, but they sometimes lacked crucial details and frequently contained appeals to geometric intuition. Corrective measures were instituted by Zariski, Weil and others as we shall explain later.

Despite the above problems, cross-fertilization still took place. Poincaré made a beginning, but it fell to Lefschetz to "plant the harpoon of algebraic topology into the body of the whale of algebraic geometry" [3]. Almost simultaneously, E. Artin translated Riemann's famous hypothesis to the case of algebraic curves over a finite field, and he succeeded in proving it for the simplest curve, the projective line. Fifteen years later, Hasse proved it for an elliptic curve (i.e., the analog of a one-dimensional torus). Then, in 1940, A. Weil, who had already done fundamental work in the area where algebraic number theory and algebraic geometry meet, announced a proof valid for all curves. The proof was algebro-geometric but it used constructs and ideas of the complex-analytic case (intersection theory, homology theory, and complex tori). In a brilliant *tour-de-force*, Weil provided the foundations of an intersection theory [6], and succeeded in constructing the analogs of tori and the relevant homology theory necessary for his proof [7], [8]. Moreover, a few years later (1949) he was led to his celebrated conjectures on analogous questions for higher dimensional varieties.

While all this was taking place on the number theoretic front, Zariski, in the early 1930s, undertook to summarize and codify the Italian contributions to surface theory. In his words, "I succeeded, but at a price", [9]. The price was his personal loss of confidence in the validity of the Italian proofs and his consequent resolve that the whole edifice had to be rebuilt on purely algebraic foundations. But his loss was our gain. The required commutative algebra was largely nonexistent at the time; so, Zariski created, and stimulated others to create, large chunks of the current subject of commutative algebra. He redid the Italian theory from the ground up and succeeded in:

(a) giving the first complete proofs of the resolution of singularities for dimensions two and three by purely algebraic methods (in characteristic zero);

(b) constructing a theory of birational transformations ("Zariski's Main Theorem") and an algebraic theory of when a variety was a manifold;

(c) creating a beautiful theory of holomorphic functions (over arbitrary fields) and analytic continuation along algebraic subvarieties–culminating in a proof of the connectedness principle;

(d) proving the Castelnuovo rationality criterion, the Enriques ruledness criterion, and the theorems on minimal models for surfaces–all by purely algebraic means valid in all characteristics;

(e) stimulating a group of extremely gifted students to make wonderful contributions of their own.

These successes also came at a price. For, all of Zariski's results were heavily algebraic in nature and some argued that they were excessively algebraic. Lefschetz [3] remarked that while he had contributed to "algebraic GEOMETRY", the modern school (Zariski and Weil) seemed to be studying "ALGEBRAIC geometry". This further increased the distance between the great geometric creations of the twentieth century–differential geometry and differential and algebraic topology–on the one hand, and algebraic geometry on the other. No less a contributor than David Mumford has remarked that as a student he struggled to "see any geometry at all behind the algebra" [**9**, (introduction)]. How much more must have been the confusion of less gifted and less committed individuals? The centrality of algebraic geometry had been further eroded by lack of communication.

Nevertheless, important progress was made, in the geometric spirit, on the complex analytic side. This was done notably by Hodge (early 1940s), by Kodaira and Kodaira-Spencer (late 1940s and throughout the 1950s), and by the infusion of newly developed algebraic and geometric topology into algebraic geometry. Hirzebruch's proof of the general Riemann-Roch Theorem (1953) spearheaded this infusion. Algebraic Topology had also invaded pure algebra, precipitating a revolution of sorts, yielding a flurry of new results, and establishing a new area: homological algebra. Moreover, the theory of sheaves was making an impact in the theory of several complex variables, and, in retrospect, it was clearly time for a *dénouement* by synthesis. We did not have long to wait.

The publication of J.-P. Serre's landmark paper *Faisceaux algébriques coherents* [**5**] was the beginning of the latest era of algebraic geometry. Serre defined varieties on the model of manifolds and showed how sheaves and cohomology could be used with the ordinary Zariski topology to prove generalizations of old results and deep new results. He stressed the point of view of geometric isomorphism as opposed to birational equivalence, and in so doing brought algebraic geometry much closer to the other geometric theories. In 1957, Grothendieck [**1**] succeeded in giving a purely algebraic proof, valid in all characteristics, of a significant generalization of Hirzebruch's Riemann-Roch Theorem. (An independent proof was also given by Washnitzer.) Along the way Grothendieck created $K$-theory.

Simultaneously, Grothendieck began a systematic rewriting of the foundations of algebraic geometry and a deepening of its results as well as an infusion of entirely new techniques. His aims were many fold:

(a) To include in as natural a geometric setting as possible (i.e., similar to the other great geometric theories) all the classical and new results proved in an algebraic manner independent of fields and considerations of characteristic;

(b) to have a sufficiently broad sweep that number theoretic questions would be included–one would have to include both the reduction of varieties from characteristic zero to characteristic $p$ and the lifting of varieties in the opposite direction;

(c) to introduce in a deeper way than had already occurred the methods of algebraic topology (homology, cohomology, and homotopy) into algebraic geometry;

(d) to be able to use methods from other geometric theories and from analysis in algebraic geometry–for example, deformation theory, vector fields and vector bundles, and a suitable theory of jets;

(e) to construct a "good" cohomology theory having all the usual formal properties so that Weil's blue print could be followed for the proof of the Weil conjectures (this unites aims (b) and (c)).

I think it must be conceded that Grothendieck and the school he created have accomplished these aims. Grothendieck's construction of the algebraic fundamental group, the broadening and deepening of this theory to a theory of *étale* homotopy (by Artin and Mazur among others), Grothendieck and Artin's construction of *étale* cohomology and the application to the proof of the first three Weil conjectures, Deligne's proof of the Weil-Riemann hypothesis (fourth Weil conjecture), Mumford's invariant theory and the consequent solution of many moduli problems, and Hironaka's proof of the resolution of singularities for *all* dimensions in characteristic zero are achievements of which twientieth century mathematics may be proud. Algebraic Geometry, with its incredible riches, has been drawn close once again to the main geometric flow of our century. I think it must be further conceded that Algebraic Geometry again occupies a central place in present day mathematics–all the more so since the wheel has come full circle and methods of algebraic geometry such as localization and completion have invaded algebraic topology (thanks to Sullivan, Quillen, *et al.*). There is now clear promise of substantial achievements ahead and we live in a time of mathematical excitement.

In view of the incredible history of algebraic geometry, of the plethora of its techniques and methods, and of the fantastic interconnections it has with the widest and broadest areas of mathematical endeavor, how is one to write a text on the subject which will give a fair hint of the above and yet which will not be so formidable in either breadth or depth as to overwhelm its readers and defeat its very purpose? David Mumford put it very beautifully in his preface to the new edition of Zariski's book on surfaces **[10]**: "The many changes in mathematical taste and terminology and our limited knowledge of the literature have made all but impossible our task of satisfactorily updating Zariski's definitive account of the classical theory of algebraic surfaces . . . . . . Is any potential reader skilled enough to be familiar with all the diverse foundations and abstract tools referred to in these appendices, patient enough to unwind the tangled relationships between old and new lines of argument, indulgent enough to forgive the gaps and gross oversimplifications caused by our parochial point of view and interested enough to want to read our hodge-podge that jumps back and forth between references and brief allusions?" How much more difficult must be the problem when the audience is to consist of graduate students just feeling their way into the geometric currents of the 20th century, and when the subject matter is to be algebraic geometry as a whole? Clearly, compromises must be made and limits must be set in any such exposition. But the fundamental problem remains and, in an area of mathematics as important as algebraic geometry, it cannot be cavilierly tossed aside.

I am happy to report that Hartshorne has done an outstanding job in meeting these challenges. First, I feel that he has made very reasonable compromises. He starts by assuming a modicum of algebraic sophistication of his readers and he points for justification to the existence of several excellent texts on algebra and commutative algebra. When, some years ago, Zariski endeavored to write a volume on algebraic geometry, he had to append so much commutative algebra that soon the "tail had wagged the dog". The result was his two volume treatise with Samuel on commutative algebra. Hartshorne's practice is to state the specialized results he needs *as he needs* *them* and to refer the reader to a number of the standard and readily available texts for the proofs. Most readers will not need this help often, but it is comforting to have precise references when necessary. Next, Hartshorne stays in the Noetherian case which is the important case for most applications. Finally, in the statements and proofs of the difficult global theorems (coherence of higher direct image sheaves, comparison theorems for formal cohomology, Serre Duality Theorem, Stein factorization, connectedness principle, and Zariski's Main Theorem), he makes a projectivity hypothesis. The projective case contains the essence of these theorems, is not at all trivial, and involves fairly representative methods of proof. Moreover, the student can specialize to readily visualized cases of these theorems and can ponder their meaning without the excess baggage of ultra sophisticated methods designed to handle the most general case.

Secondly, Hartshorne bounds his exposition by repeatedly emphasizing the case of ordinary projective varieties over an algebraically closed field. Not only is this historically the most important case, but it has the added advantage that concrete computations and pictorial representations may be made. Time and again after the proof of some general fact, Hartshorne will give examples of the theorem in various concrete cases. Often, these examples will have been treated earlier by cruder methods. The comparisons are then drawn, and the student is elevated ever so gently to a higher stage of understanding.

The book begins with a chapter on varieties. The basic phenomena and definitions are covered in a very computational way, and the translation from geometry to algebra and back is started. The student begins to sharpen his geometric intuition. Many exercises are included–most are not difficult, but almost all of them point out an important moral. The next two chapters entitled *Schemes* and *Cohomology* are the heart of the book. They introduce the modern terminology and methods. Large efforts are made through examples in the text and the exercises to connect the new language and concepts with the more inituitive approach of Chapter I. If I have any qualms with the content of the book, they are mainly concentrated in Chapter III (on *Cohomology*). There is much emphasis on derived functors and categories which I feel could have been relegated to an appendix or referred to sources as in done with the algebraic preliminaries. Moreover, some effort is spent on a vanishing theorem of Grothendieck whose use in the book is very minimal (since the thrust of the theorem takes one outside the category of quasi-coherent sheaves where almost all the geometry takes place). But these quibbles are ones of opinion only, they do not vitiate the fact that the important cohomological theorems are treated clearly and in detail. The examples and exercises are integral parts of the exposition (no pun intended) and make the chapter very satisfying indeed. This is especially true in the sections on flat and smooth morphisms, for here the classical intuition needs shoring up.

An excellent feature of the book is the last two chapters entitled *Curves* and *Surfaces*, respectively. By coming down from the abstract to the classical and concrete, Hartshorne shows the power and beauty of the methods developed in Chapters II and III. More than that, the use of these methods in explicit situations of high interest imbues them with a life which the reader will hardly fail to appreciate and will not soon forget. Also, the very beautiful theory of curves and surfaces is developed smoothly and with a minimum of technical difficulty. Of course, only selected topics of surface theory can be covered. Nevertheless, Hartshorne is able to discuss material on ruled surfaces, monoidal transformations (including the factorization theorem, resolution of singularities, and Castelnuovo's criterion for contractibility of a curve), cubic surfaces in $\mathbf{P}^3$ (culminating in the 27 lines), and some remarks on the classification of surfaces. Once again, the exercises provide essential enrichment and the key to understanding.

There are three short appendices entitled: *Intersection theory*, *Transcendental methods*, and *The Weil conjectures*. They are elegantly written and provide a glimpse of material beyond the confines of the book and near the current frontiers of the subject. I would have liked a lengthened treatment in all three–they consist of exciting and important mathematics. Each appendix cries out for a book length treatment of its own–perhaps someone will take up the challenge. Also, I would have liked two more appendices on the style of the included three. In my view, they should have been on *Deformation theory* and *Group schemes*.

If I may be permitted a personal note at the close of this review, I should like to relate that in 1963–1964 I began to write a book on algebraic geometry in order to fill the gap which then existed. Independently and simultaneously, David Mumford began working on such a project. The plan of these books (in a gross way) and of Hartshorne's book was entirely similar: a concrete treatment of classical algebraic geometry without sheaves, a treatment of sheaves, schemes, and cohomology, and finally applications to a smooth treatment of more classical and special topics such as curves and surfaces. To my knowledge, Hartshorne came to his idea of the book independently, and I guess there were others who quietly toyed with similar ideas. Mumford's project first produced his Harvard notes and later (I suspect) his beautiful book on curves on a surface [4]. Finally, he has begun the publication of what appears to be a series of books on algebraic geometry of which *Complex projective varieties* is the first installment. Of the other silent authors (if they exist) I know nothing; of myself, I was stricken with what I may call "writer's overconscientiousness". This largely mental ailment has been described by Loren Eiseley in his autobiography [2]. While I do not wish to claim the grandiose pattern he describes for my own little project, let me quote: "At this point two things threaten the researcher (writer). First, he may become so lost below ground, trail leading on to trail, that he may never emerge to publish. He may be stricken by a phobia of incompleteness. . . . The drone of that buzzing fly, the publisher, recedes into the distances of the future. . . . Second, as the accompaniment of this retreat, he may no longer care to organize this precious knowledge or fix it into a pattern. . . . Publish? There is not time. . . . " So it happened with me, by book was always two-thirds finished.

In the end, I do not mind. Hartshorne has survived the inevitable attack of this disease. He has produced a book faithful to the original plan–a book of which he may be proud and for which we must be grateful. It is destined to become the standard reference for young workers in and students of algebraic geometry. They will be well served. Because algebraic geometry is important for so many fields from partial differential equations through complex analysis to number theory and algebra, this book belongs on every mathematician's shelf. We owe Hartshorne our thanks.

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BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 1, Number 3, May 1979

0002-9904/79/0000-0213/$01.75

*Infinitary combinatorics and the axiom of determinateness*, by Eugene M. Kleinberg, Lecture Notes in Math., vol. 612, Springer-Verlag, Berlin, Heidelberg, New York, 1977, 150 pp.,$8.30.

Many questions of mathematical interest cannot be answered on the basis of ZFC, the standard axiomatization of set theory. Notable examples are the Continuum Problem, and the problem of the Lebesgue measurability of PCA sets of reals. (PCA sets are the projections of complements of analytic subsets of $\mathbf{R}^2$.) However, as Gödel suggested in [1], it may be possible to settle such problems by extending ZFC. Gödel hoped to find new axioms with the same "intrinsic necessity" as those of ZFC. Failing this, he hoped that "there might exist axioms so abundant in their verifiable consequences, shedding so much light upon a whole field, ... that, no matter whether or not they are intrinsically necessary, they would have to be accepted at least in the same sense as any well-established physical theory." One might call the search for and study of such axioms "Gödel's programme"; it is the antithesis of Hilbert's programme.

Work on this program has concentrated on two sorts of hypotheses, the first sort asserting the existence of certain large cardinal numbers, and the second the determinateness of certain definable games. Both sorts can be viewed as extrapolations of principles inherent in ZFC, though neither has