considerations. The topics covered by Skorohod are: measurable polynomials (this is an abstraction of the Itô-Wiener theory of homogeneous chaos in Wiener space); absolute continuity and quasi-invariance under shifts and nonlinear transformations; and surface integrals and Gauss' formula in Hilbert spaces. (The last of these topics appears here for the first time.) In contrast, the book of Badrikian and Chevet includes: GB and GC-sets, ϵ-entropy, a thorough discussion of the work of Sudakov with complete proofs (given for the first time) together with recent amplifications due to Chevet, and 0-1 phenomena and integrability properties of Gaussian measures. Of the two books, Badrikian and Chevet's is much more up-to-date and Skorohod's is much more accessible to the novice. Together they constitute a quite complete account of the state in which this art finds itself today; the one with its emphasis on computation, the other with its infatuation with generality and elegance. Unfortunately, neither one devotes any space to Feynman integration or the recent applications that this area has enjoyed in quantum field theory. On the other hand, if the success of these probabilistic techniques in physics continues, there will certainly be books forthcoming on that subject, and these books will then be appreciated for the groundwork which they have laid.

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Geometric theory of algebraic space curves, by S. S. Abhyankar and A. M. Sathaye, Lecture Notes in Mathematics, no. 423, Springer-Verlag, Berlin, Heidelberg, New York, 1974, xiv+302 pp., $11.50.

Let k be an algebraically closed field, for example $k = \boldsymbol{C}$ (the complex numbers) will do. An *affine algebraic variety* over k is the solution set of a family $\{f_\alpha(x_1, \cdots, x_n)\}_\alpha$ of polynomials in n variables (for some n) with coefficients in k. Actually, we should be more precise about where our solutions are located. If A is a k-algebra (e.g., $A = k$ itself, or $A =$ some field extension of k) then we can evaluate the polynomials $f_\alpha(x_1, \cdots, x_n)$ on n-tuples $\langle a_1, \cdots, a_n\rangle$ from A. Hence, it makes sense to consider those n-tuples from A for which all the polynomials f_α vanish. These n-tuples are the points of our variety V *with values in* A (*or rational over* A). The whole variety, V, should be thought of as the collection of all the sets, $V(A)$, consisting of the points of V with values in A for all k-algebras A.

Classically, geometers considered only the case in which the k-algebra A was a field; since the book under review adopts a classical position, we shall also restrict attention to the case when A is a field. If Ω denotes an algebraically closed field of infinite transcendence degree over k, then it turns out that all phenomena of the classical variety V may be captured in the set $V(\Omega)$. We can therefore replace the somewhat nebulous idea of the collection $V(K)$ (where K is a field over k) by the one set $V(\Omega)$. Even more marvelously, one finds that (Hilbert Nullstellensatz) $V(k)$ is dense in $V(\Omega)$, so that most properties of V may be viewed in the very concrete set of k-rational points of V, $V(k)$.

Now it is easy to see that there is a 1-1 correspondence between k-algebra homomorphisms

$$k[x_1, \cdots, x_n]/(f_\alpha(x_1, \cdots, x_n))_\alpha \to A$$

and points of V with values in A; namely, to the point $a = \langle a_1, \cdots, a_n \rangle$ we associate the homomorphism θ_a, where $\theta_a(x_i) = a_i$; and to a homomorphism θ, we associate the point $a_\theta = \langle \theta(x_1), \cdots, \theta(x_n) \rangle$. Since we are restricting ourselves to the case of points with values in a field (or, as they are called in Grothendieck's terminology, geometric points), it is clear that the ideal generated by the $f_\alpha(x_1, \cdots, x_n)$ may be replaced by its radical. When this is done, the k-algebra

$$\mathscr{A}(V) = k[x_1, \cdots, x_n]/\sqrt{(f_\alpha(x_1, \cdots, x_n))_\alpha}$$

is called the *affine algebra of* V (or the *coordinate ring of* V). Notice that $\mathscr{A}(V)$ is a finitely generated k-algebra without nonzero nilpotent elements. Such a k-algebra will be called an affine ring over k; the present book considers these objects in detail. The correspondence between ideals, $\mathfrak{A}$, which equal their own radical and affine varieties allows for easy passage between "pure algebra" and geometry; at the same time it allows for some distressing tendencies which are very evident in the book under review. (More about this later.)

A more valuable geometric theory results from considering projective varieties. Algebraically, this corresponds to passing from the above set-up to the case of graded rings and homogeneous ideals. Once this is done, there is no longer the elementary correspondence between geometric points of the variety and homomorphisms of a ring to a field (a fortunate happenstance which gives projective geometry its power and depth), but there still is a 1-1 correspondence between projective varieties embedded in $\boldsymbol{P}^n$ (projective n-space) and relevant homogeneous ideals equaling their own radicals of the ring $k[x_0, x_1, \cdots, x_n]$. (Here, relevant means that the radical of the ideal does not contain the fixed ideal $(x_0, x_1, \cdots, x_n)$.) In the present book, the authors make much use of this correspondence.

A more general geometric set-up than the projective varieties can be obtained by patching together affine varieties along open subsets (copying the manner in which one passes from open subsets of $\boldsymbol{R}^n$ to a manifold). If one removes a hyperplane section from a projective variety, one obtains an open set which is an affine variety and these affine varieties are naturally glued along their mutual intersections. Hence, the projective theory fits into the "manifold" set-up. The first person to seriously consider this method was André Weil [**W**] who called the resulting geometric objects *abstract varieties*. However, he patched along open subsets by densely defined maps (called rational maps). The first person to achieve a modern synthesis incorporating patching by everywhere defined maps and including contemporary techniques such as sheaf theory and cohomology was J. P. Serre [**S**].

His methods and points of view were greatly generalized and deepened by A. Grothendieck [**G1**], [**G2**]; it is the Grothendieck school which is in current ascendence.

Projective and affine varieties (perhaps not exactly as sketched above) were well known to the founders of and early workers in algebraic geometry (the German school of the last quarter of the last century: Brill, Noether, etc., and the Italian school straddling the turn of the century and extending up to the 1930's: Castelnuovo, Enriques, Severi, etc.). Besides being mathematicians of considerable insight and ingenuity, they did not flinch from the computation and examination of innumerable special examples to help illuminate the phenomena they uncovered. Among these phenomena was one with which the current book is principally concerned.

Suppose we consider r hypersurfaces embedded in $\boldsymbol{P}^n$ $(r<n)$. The set-theoretic intersection of these hypersurfaces will (in general) be a subvariety of codim r. One can ask for the converse: Given a subvariety of codim r in $\boldsymbol{P}^n$, is it set-theoretically the intersection of r hypersurfaces? The answer is no; however, if the answer is yes, we call the given variety a *set-theoretic complete intersection.* (An example in which the answer is no is furnished by the union of two projective planes in $\boldsymbol{P}^4$ which meet at a single point.) Observe that if $\mathfrak{A}$ is a homogeneous ideal of $k[x_0, x_1, \cdots, x_n]$ generated by r elements and if the variety, V, determined by $\mathfrak{A}$ in $\boldsymbol{P}^n$ has codim r, then certainly V is a set-theoretic complete intersection. When the conditions of the last sentence hold, we shall say that V is a (*strict*) *complete intersection.*

When the founding fathers of algebraic geometry investigated examples, they stayed mainly in the cases: curves in the plane ($\boldsymbol{P}^2$), curves in space ($\boldsymbol{P}^3$), surfaces in $\boldsymbol{P}^3$, and higher dimensional phenomena for complete intersections. They discovered that in all their examples every connected space curve was a set-theoretic complete intersection. Naturally they posed the now classical question: *Is every connected* (less generally, irreducible) *curve in $\boldsymbol{P}^3$ a set-theoretic complete intersection*? This question remains open even now. The genesis of the book under review is the fact that Abhyankar was able to make some progress towards an answer (see below).

Max Noether asserted [**N**] that every "general" surface of degree $\geqq 4$ in $\boldsymbol{P}^3$ contains only curves which are complete intersections. His proof, while ingenious, left something to be desired (from the point of view of rigor). Lefschetz [**L**] reopened the investigation in the course of his great achievement of applying algebraic topology to algebraic geometry. He proved that every nonsingular variety of dimension $\geqq 3$ which is a strict complete intersection in $\boldsymbol{P}^n$ contains only hypersurfaces which are themselves complete intersections. Moreover the same result holds for sufficiently general surfaces in $\boldsymbol{P}^n$ $(n \geqq 4)$ which are complete intersections and are not contained in any hyperplane. And, lastly, Noether's Theorem is valid. The point of all of this is that the notion of complete intersection is very geometric and natural, and that it has attracted quite serious attention.

There is another aspect of the problem of complete intersections. This has to do with vector bundles. One can think of vector bundles dynamically (as families of varying vector spaces, one over each point of a variety V) or statically (as spaces over V which are locally of the form $E \times U$, where E is a fixed vector space and U is an open subset of V—the patching being done by elements of the general linear group on E). In any case, the problem of determining all vector bundles over a given variety is very hard. In the simplest case, Serre [**S**] conjectured that: *Every vector bundle over affine n-space is trivial* (i.e., a product $E \times \mathbf{A}^n$). Algebraically this means that every finitely generated projective module over the ring $k[x_1, x_2, \cdots, x_n]$ is free. Of course, this is true and classical for $n=1$; it was proved by Seshadri for $n=2$ [**Sh**]. Serre showed that if his conjecture could be proved for $n=3$, then every nonsingular (irreducible) curve in $\boldsymbol{P}^3$ with a trivial canonical line bundle (i.e., of genus $g \leqq 1$) would be a strict complete intersection. Murthy and Towber [**MT**] proved Serre's conjecture for $n=3$; in the proof they use a result of Abhyankar's giving an explicit and workable set of three generators for the ideal corresponding to a curve in $\boldsymbol{P}^3$. The latter result is covered in the current book. Abhyankar's generators are not in general certain special linear combinations of the given generators of the ideal and this question persists for the projected curve in $\boldsymbol{P}^2$. Krusemeyer [**K**] has recently given an obstruction in $K_2(k)$ whose vanishing is necessary in order that such special combinations be available for the projected plane curve (for suitable plane curves).

Having sketched at some length the circle of ideas that are connected with the material which is discussed or ought to be discussed in the current book, we should now examine the book itself. According to the preface, the book exists to give a completely self-contained treatment of the following theorem of Abhyankar: *Every irreducible, nonsingular, space curve of genus at most one and degree at most five is a complete intersection.* As we mentioned above, Murthy's theorem is more general, but it was proved using some of Abhyankar's preliminary results.

In giving a self-contained treatment of any subject, one is aiming at the uninitiated, for (presumably) the initiates will already have much background information and the treatment can be shorter. But, if this is the case, no uninitiated reader will really understand this book and it must be judged a failure. Of course, any dedicated reader will eventually have a perfect local and logical understanding of the material, but because of the way the book is written, he will not have the faintest global picture nor will he know how the results were originally arrived at. The authors have simply ignored a coherent global exposition in favor of a notationally and terminologically detailed local exposition.

The subject matter is geometry and the title of the book is *GEOMETRIC theory of algebraic space curves*. But the authors eschew all visible geometry and hide all the connections spelled out above between their work and the rest of geometry. They write in their preface, "What we present here is a geometric argument in which we never even need a coordinate system. However, it might be difficult to convince anybody that this is geometry, for we have deliberately avoided the use of geometric terms, so that the proof may stay rigorous, self-contained, and still reasonably short. Thus we have taken the useful geometric concepts, translated them into precise algebraic terms and almost never gone back to the geometric terms."

It seems that this distressing tendency to hide all geometric antecedents results from a misreading and misinterpretation of the guiding philosophy behind Zariski's work from 1937 to the present. Zariski, as he explains in the preface to Vol. I of his collected works [**Z**], had become disaffected with and uneasy over the logical structure of the Italian contributions to geometry. In his words: "I began to feel distinctly unhappy about the rigor of the original proofs I was trying to sketch (without losing in the least my admiration for the imaginative geometric spirit that permeated these proofs); I became convinced that the whole structure must be done over again by purely algebraic methods." He also writes in the same preface, "It (his work) became strongly *algebraic* in character, both as to methods used and as to the very formulation of the problem studied (these problems, nevertheless, always have had, and never ceased to have in my mind, their origin and motivation in algebraic *geometry*)" (Zariski's italics). It was one of Zariski's achievements that he was able to use algebra as a method to prove the geometric theorems he wanted; he started a school of algebraic geometry in which the methods and problems were handled algebraically. However, it is apparent that he never intended to throw out geometry. The authors state that they give a dictionary in §43 to enable the reader to understand the geometry. Nonsense! The proper way to stay true to the Zariskian ideal would have been to emphasize the interplay of algebra and geometry, to show how the geometry translates into doable and rigorous algebra, and to have translated back when the results were obtained. This is especially true in an expository book.

The book has further problems. For one thing, the notation is overbearing. For another, the theorems are stated in a maximum of notation and a minimum of English (see, for example, Theorem 10.1, pp. 33, 34, 35, 36(!)). No one can read formulas such as occur on pp. 22, 23, 24, 25 (where whole pages are covered with formulas). Also pp. 123–129 are totally covered with formulas. The book is made bulky by constant repetition, e.g., §§18 and 19 of Chapter II (especially the top paragraphs of pp. 93 and 94). In many places the English is not smooth, e.g., a favorite construction of the authors is: $\cdots$ it follows that: upon letting $\cdots$.

It would be easy to dismiss the book for all these faults; however, this is not possible because it is written by serious mathematicians and contains important, nontrivial mathematics. Therefore, I will content myself with several suggestions which I feel might have vastly improved the book.

(1) The important §10 gives the structure of double points. The conductor is studied thoroughly, but the statements are so cumbersome that a reworking is necessary. One should give examples of high nodes and cusps—this will be easy because of the theorems of the section.

(2) While the notion of adjoint is introduced, it is not explained geometrically and its significance is buried in the morass of details and formulas of the book. This should be corrected, especially since the authors give

Abhyankar's treatment of differentials and hence spend no little space on these objects.

(3) The key points of Abhyankar's and Murthy's proofs—the projection theorems and basis theorem—should be heavily emphasized *via* examples, geometric language, pictures to indicate what is going on, and some intuition as to where the material is headed and why. For example, it would be nice to have the "cone", "plane", and "quadric" lemmas in geometric language and to have pictures and examples for all of these results. In particular, why should the reader have to wait until p. 243 for the intuition behind the word "π-quasihyperplane" when the concept itself is introduced on p. 151?

It is a shame that the authors wrote the book in such an opaque and cumbersome style. It could have been an important contribution to the literature by showing how one can apply detailed concrete computations and ideas to algebraic geometry.

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The functions of mathematical physics, by Harry Hochstadt. Pure and Applied Mathematics, Vol. 23, Wiley-Interscience, New York, 1971, xi+322 pp., $17.50.

At first sight, the theory of the special functions of mathematical physics seems to be little more than a disorganised collection of formulas. There appear to be more than fifty special functions and there is more than one definition of each one of them; for each there is a bewildering variety of