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BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 31, Number 2, October 1994

0273-0979/94 $1.00 + $.25 per page

*Dynamical systems* VIII (*singularity theory* II), *classification and applications*, by V. I. Arnol'd, V. V. Goryunov, O. V. Lyashko, and V. A. Vasil'ev, editors. Encylopaedia of Mathematical Sciences, vol. 39, Springer-Verlag, New York, 1993, 235 pp., $89.00. ISBN 3-540-53376-1

Singularity theory is not a theory in the usual (axiomatic) sense. Indeed it is precisely its width, its vague boundaries, and its interaction with other branches of mathematics and science which makes it so attractive. In particular, the subject draws on algebraic and analytic geometry, commutative algebra, and differential analysis and has applications to differential and algebraic geometry, bifurcation theory, optics, and a wide range of other topics. This width then is encouraging, indeed exciting. But a nebulous nature can lead to identity crises, and I then find it useful to think of singularity theory as the direct descendant of differential calculus. It has, for example, the same concerns with Taylor series, and one can view much current research as natural extensions of problems with which our forefathers laboured and which were considered central. The calculus is the tool, par excellence, for studying physics, differential equations in general, and the geometry of curves and surfaces; consequently, one should not be surprised that singularity theory will have applications in these fields. Indeed one might judge its vigour by its success in these areas.

Such an illustrious ancestory makes it difficult to specify a date for the subject's birth. Newton certainly produced work of importance which would be best classified as singularity theory, while Huyghens made some exciting applications in the same spirit. (Newton is one of the authors cited in the volume under review!) Indeed many of the central notions within singularity theory specialise to familiar ideas such as evolutes, duals, wavefronts, and caustics. This is of course as it should be; useful "new" theories should build on older and central ones. However, there is general agreement that the father of modern singularity theory was Hassler Whitney, a mathematician who preferred setting trends to following them. His work, initiated in the 1930s, culminated in a remarkable study of generic mappings from a surface to the plane in 1955 [6]. In this paper Whitney described the local form of a general map from a surface to a plane. Basically there are three different types: submersion, fold, and cusp. The paper is remarkable for a number of reasons. The first point to make is that it is not obvious that one can say anything useful at all about "general" or generic maps. Where are the hypotheses in Whitney's theorem? Second, the paper introduced the natural notion of $\mathscr{A}$-equivalence for map germs, that is, changes of coordinates in the source and target. Third, Whitney highlighted the notion of stability: most mappings exhibit these local types, and (this is not quite accurate) any such map if deformed a small amount does not change its qualitative appearance.

The initial work of Whitney was continued and widened by Rene Thom, using transversality as a systematic tool for investigating singularities. Indeed transversality is one of those key ideas which is so natural that its importance is easily underestimated. (In fact, the jet transversality result, although easy to prove, is at first sight rather unlikely. Indeed Whitney was disinclined to believe it before seeing the proof.) Thom also contributed the fundamental notion of an unfolding (similar ideas were emerging at the same time in algebraic geometry). Most of the deeper interesting geometrical and physical applications hinge on this idea. At that time Thom also developed his so-called catastrophe theory (CT), whose aim was to study the bifurcations of gradient dynamical systems. Thom's hope was that his results on unfoldings could be used as a tool to understand some aspects of embyrology and other portions of developmental biology. Many mathematicians were struck by the beauty and depth of Thom's results, but in retrospect one can see that a number of rather outrageous claims were made by some of the proponents of CT, together with some rather dubious applications. Nevertheless some important extensions and modifications of Thom's ideas, especially to bifurcation theory, have been made, and the controversy surrounding Catastrophe Theory should not mask Thom's central and immensely important contribution to the subject.

Around the time that Thom and others were developing Catastrophe Theory, Mather was making rapid progress with singularity theory proper. It was Mather who first proved Thom's vital unfolding theorem, and he not only systematised many of Thom's ideas but also contributed a large number of important notions and theorems of his own. The major result established by Mather provides a practical criterion for a mapping to be stable, shows that the problem is essentially a local one, and for any pair of dimensions $(n, p)$ establishes whether or not the stable mappings between any two manifolds of these dimensions are dense. Moreover, he also provides an algorithm for describing the local forms of these stable mappings. This is clearly an astonishing generalisation of the earlier work of Whitney. Indeed his six fundamental papers written between 1968 and 1971 completely revolutionised and to some extent created the analytic side of singularity theory, and it was a number of years before the rest of the community (initially Damon and Gaffney) fully absorbed Mather's ideas and moved forward in this part of the subject.

During this period of rapid advancement Arnold introduced his notions of Lagrangian and Legendrian singularities. Again the unfolding theorem is a crucial ingredient in Arnold's theory, which does indeed have intimate connections with CT. The reader is recommended Arnold's little book [1], both for a lively, personal, and (I feel) overly harsh view of CT and, more interestingly, for a nice exposition of the important contributions made by Arnold and his school to a number of interesting geometrical problems, using techniques from singularity theory. Arnold also made spectacular advances in the classification of functions and in an understanding of these objects, and in particular introduced the crucial concept of simple singularity (basically those functions whose classification does not involve continuous invariants) which has proved so fruitful.

Meanwhile another strand to the subject had developed, arising from the work of Brieskorn, Milnor, and others on isolated singularities of complex hypersurfaces (and later complete intersection singularities). Milnor in [5] proved his famous fibration theorem, which had fascinating interplays with differential topology via higher-dimensional knots, spinnable structures, and foliations. Delightful as Milnor's book is, it is nevertheless a rather misleading introduction to this area; the "correct" approach is via Picard-Lefschetz theory (as suggested by Brieskorn and developed by Arnold and his school). This side of the subject was the one that initially underwent the most rapid development, and a very large body of results was quickly established.

When reviewing singularity theory, one should distinguish three underlying areas of interest. The first is the study of functions, that is, mappings with the real or complex numbers as target. The second is the study of varieties, that is, the study of map germs $f\colon K^n, 0 \to K^p, 0$ (here $K$ is the real or complex numbers) where the key interest is in the solution set $f^{-1}(0)$. The relevant equivalence relation here is contact or $\mathscr{K}$-equivalence, introduced by Mather in the seminal papers mentioned above. The third is the study of mappings up to $\mathscr{A}$-equivalence. There are substantial differences in the technical difficulties of the problems in these three categories. For functions one can make do with changes of coordinates in the source only (right or $\mathscr{R}$-equivalence), and this and $\mathscr{K}$-equivalence are much easier to deal with than $\mathscr{A}$-equivalence. The question of stability can also be reduced to one concerning $\mathscr{K}$-equivalence, which explains why Mather could make such good progress on that front. For this and other reasons the theory of functions and varieties is better developed than that of general mappings.

The volume under consideration is the second in this series concerned with singularity theory but can be read independently of the first, although it clearly deals with more advanced topics. Given its authorship, it has a tendency to focus, understandably, on the substantial contributions of the Moscow school and reads rather like a collection of articles of the type which appear in the Russian Mathematical Surveys. The first chapter is the one I found most useful and deals with the classification of functions and mappings. It starts with a discussion of functions on a manifold with boundary, including versal unfoldings and their topology (the basis for the relative homology of a nonsingular fibre, intersection forms, monodromy, etc.). There is then a discussion of the complete intersection case. The classification of simple singularities is due to Giusti (although much of it is embedded in earlier papers of Mather on smooth stability) and is given. The local topology of complete intersection singularities is then discussed. The initial results are due to Hamm (around 1970), and a number of central results on monodromy due to Ebeling are stated. (It would be hard to better the book by Looijenga [4] as a fuller reference to this large area.) There follows a discussion of projections of curves and surfaces to the plane and an important generalisation to projections of complete intersections due to Goryunov. There is also a discussion of mappings from the line to the plane and the plane to 3-space, with lists of the corresponding simple singularities and invariants, due in the latter case to Mond. The authors provide a treatment of functions with nonisolated singularities, an important area developed by Siersma and Pellikaan, which provides useful insights into the structure of the space of functions with isolated singularity. They then describe how to obtain vector fields tangent to various bifurcation varieties. Concrete examples of these include caustics and wavefronts, and indeed Arnold initiated the study of such vector fields while solving the problem of describing generic wavefront evolution in 3-space [1]. The chapter finishes with results concerning diagrams of mappings.

The second chapter deals with applications of the theory of functions via Legendre, Lagrange, and Maxwell sets, with the first two being covered rather briefly. Key examples of Legendre singularities are duals and wavefronts. For Lagrange singularities think of caustics and the set of critical values of the Gauss map of a surface in 3-space. Maxwell sets arise as shock waves associated to certain PDEs. As one might expect, these applications gain from the results developed within singularity theory. Conversely, however, they help mold the development of the subject. (I was somewhat surprised at the brevity of the treatment of Legendre and Lagrange singularities; there is a fuller treatment in a book of the same series [3] on symplectic geometry. Nevertheless, a newcomer to the subject would be rather misled by the relative amounts of space devoted to these three topics in this volume.) The chapter ends with a discussion of gradient vector fields. One of the reasons for Thom's original classification of functions was a desire to describe the bifurcations of gradient vector fields. However, these bifurcations are not enumerated by listing versal unfoldings of the corresponding functions; and, following work of Guckenheimer, Vegter, and Khesin, one can see that the general picture is a good deal more complicated than that originally envisaged by Thom.

The third chapter focuses on certain spaces of objects containing (open) subsets possessing "good" properties. (As the simplest example consider the set of real polynomials of even degree, and as the "good" subset those which are positive away from zero.) One of Arnold's many interesting insights is the assertion that "good" states are fragile. This is best explained by an example. If in the plane the boundary between good and bad states was given by a cuspidal cubic, then one would expect at the cusp point that the locally slim subset of the plane lying inside the cusp would be the good side of the divide. A number of interesting (general) examples are given, together with information concerning the nature of the boundary and its singularities.

The fourth chapter is concerned largely with results of Vasil′ev concerning ramified integrals and Picard-Lefschetz formulae. The basic idea is to integrate forms over a cycle which varies in a family; if this family forms a closed path in some parameter space, we run into the notion of monodromy. The classical situation here is that determined by a pencil of hyperplane sections of a variety in some projective space. Finitely many of these sections will be singular/tangent. When the corresponding points on the projective line parametrising the pencil are deleted, we obtain a fibration over their complement whose fibres are the nonsingular sections. We can lift a closed path in the base space to obtain an automorphism of some fixed nonsingular section and consider its action on the homology. The result is determined by the associated Picard-Lefschetz formula. One of the first extensions was to the study of singularities of functions, and this has substantial applications to the theory of oscillatory integrals due to Arnold, Varchenko, and many others. In this chapter the corresponding theory for integrability of algebraic domains (a problem apparently first considered by Newton), for ramification of solution of hyperbolic equations, and for integrals of ramified forms are given. The prerequisites are a little wider than in previous chapters and the connection with mainstream singularity theory less pronounced, although the results are clearly of great interest.

The book closes with an interesting discussion of real deformations of singularities. Much of the motivation for singularity theory arises from what one might term "real" applications, for example, the problem of describing the caustics of a generic wavefront or (all of) the outlines of surface in general position. The key then concerns singularities of real-valued functions and mappings. On the other hand, the theory has its most elegant formulation in the complex case. The advantages of the complex numbers over the reals reduce to the usual one, namely, that the complement of a point in the complex plane is connected. So, for example, one standard way of studying a degenerate singularity is by deforming it into a collection of nondegenerate singularities. (For a function these are simply Morse singularities.) Given that the degenerate deformations form a hypersurface in the space of all such, one expects any two good deformations over the complexes to have the same form, since they can be joined by a family of good deformations. Over the reals, however, this is no longer true. (At a trivial level the polynomial $x^2$ can be deformed over the reals to one with two real or two complex conjugate roots: over the complexes these are indistinguishable.) The main results in this section are concerned with the search for local Petrovskii lacunas. Here we take a function, versally unfold it, and think of the resulting discriminant as a wavefront; a component of the complement is a lacuna if the solution of the corresponding hyperbolic equation is nonsingular. The chapter lists results on the number of local lacunas for all simple singularities and all singularities of corank 2 and Milnor number at most 11. The authors also describe a computer algorithm used to determine some of this information.

The notion of an encyclopaedia of mathematical sciences is initially very attractive. The current work would certainly prove very useful to experts in the field. I would not, on the other hand, describe it as an introduction to the subject or a useful general reference for the interested reader. In particular, the last two chapters are rather technical (I certainly found them more difficult to read than the first three). There are of course no proofs (wouldn't my undergraduate class be pleased!), but proofs do go a long way towards explaining the reasons why results hold. In fact this really is a collection of (nicely written) survey articles whose topics lie within, or are adjacent to, singularity theory, and as such I can heartily recommend it.

Finally I feel that I must mention the price. This is a Springer volume of 235 pages weighing in at $89. I am glad I had the book to review, as it is a valuable addition to my bookshelf. I doubt that I would have parted with $89 for it. I wonder how much longer we academics are going to continue to create our own resource crises? Publishing is an activity we dominate: we write the papers/books, we do the refereeing, and we do the buying. And we still allow publishers to charge us exhorbitant prices for the privilege of having our work in our own libraries.

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BULLETIN (New Series) OF THE
AMERICAN MATHEMATICAL SOCIETY
Volume 31, Number 2, October 1994

0273-0979/94 $1.00 + $.25 per page

*Evolutionary integral equations and applications*, by Jan Prüss, Monographs in Mathematics, vol. 87, Birkhäuser Verlag, Boston, MA, 1993, xxvi+366 pp., $139.00. ISBN 3-7643-2876-2

This book deals with linear integral equations of the following type:

$$u(t) = \int_0^t A(t-s)u(s)\,ds + f(t), \qquad t \geq 0 \tag{1}$$