But this is another story, better left to the reviewer of Volume III. As a preparation to all this new "microlocal" world, Chapter VII of Volume I presents a detailed treatment of the stationary phase formula (and a proof of the Malgrange preparation theorem, eventually needed in the microlocal reduction to standard forms). Chapter VIII is entirely devoted to the wave-front set, which is the central notion of (the first) microlocalization. Chapter IX looks at the analytic wave-front set and introduces a definition of hyperfunctions in the spirit of Martineau.

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FRANCOIS TREVES

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Shape theory, by S. Mardešić and J. Segal, North-Holland, Amsterdam, The Netherlands, 1982, xv + 378 pp., $81.50, Dfl 175.00. ISBN 0-4448-6286-2

The appearance of a book on shape theory provides the reviewer with the opportunity of assessing where shape theory came from, and what of value is coming out of it.[1]

1. A little history: Čech homology 1928–1968. In the late twenties, there was point set topology and there was algebraic topology, but the correct relationship between the two subjects had not yet become clear. In those days, algebraic topology meant, in the main, the homology theory of simplicial complexes with integer coefficients. The topological invariance of this theory was more or less established, but the restriction of the theory to polyhedra appeared to the point set topologist to be arbitrary and ugly.

Then, in 1928, Alexandroff [2] discovered a theorem which, with hindsight, can be seen to express the proper relationship between the two subjects. The

[1] References refer to the bibliography of the book under review.

principle underlying Alexandroff's theorem is this: *very general topological spaces can be regarded as limits of simplicial complexes.* More precisely, Alexandroff showed that a compact metric space X is always homeomorphic to the inverse limit of an inverse sequence of finite complexes, and that the Betti numbers obtained for X in this way are independent of which inverse sequence is used. Once this was understood, the discovery of Čech homology was inevitable.

Alexandroff's principle, so familiar now, was considered at the time to be radical and important. Here is how Lefschetz hailed it in the introduction to his 1930 book: "The topological theory of complexes acquires thus a fundamental importance; it is in truth the necessary first step in a general study of metric spaces."

How dated that quotation sounds! Things developed in quite a different way. Point set topologists and algebraic topologists began to drift apart in the thirties. Alexandroff may have delineated their common frontier, but neither school found the border territory to be very interesting.

Thus it happened that the development of Alexandroff's idea took place haphazardly over the next forty years. Perhaps it is worthwhile to recall some of the highlights.

Originally, Alexander duality was expressed in terms of Betti numbers. Once cohomology was formulated, the duality could be put in terms of Čech cohomology. It was then clear that the dual statement relating Čech homology of the compact set to cohomology of the complement is false, so Steenrod [**2**] introduced "Steenrod homology" as a corrective. In the mid forties, Čech homotopy groups were introduced in the thesis of Lefschetz's student Christie [**1**] in order to state and prove a "Čech Hurewicz Theorem". Marston Morse published long papers in the late thirties generalizing "Morse Theory" to the case of a lower semicontinuous real valued function on a metric space, using Čech homology. The Vietoris-Begle Theorem (see Spanier [**1**]) related Čech cohomology of the point-inverses of a map to the cohomology morphism induced by that map: this, as well as work of Eilenberg-Wilder in the forties, anticipated the modern theory of cell-like maps.

But all these were fringe topics. Increasingly, the business of algebraic topology was complexes and computation. Besides which the issue of appropriate generalization became more problematic when Eilenberg introduced singular homology, applicable to all spaces and owing nothing to Alexandroff's principle of approximation. Eilenberg and Steenrod restored the Alexandroff idea somewhat in their aesthetically influential book [**1**]. They pointed out that Čech homology with integral coefficients fails to be exact, but that this defect disappears when the coefficients form a field or a compact abelian group.

More is going on here than a fiddle with coefficients. Two ways of explaining it emerged in the sixties. First, there was Milnor's explanation [**2**, **3**]. Milnor showed that Steenrod homology theory on compact metric pairs is exact — indeed it satisfies all the Eilenberg-Steenrod axioms, and is characterized by these together with two other axioms. Alexandroff's principle had been sharpened by Čech in the *continuity property* that the homology of the inverse limit is the inverse limit of the homology. Now Milnor showed that the deviation of Steenrod homology from the continuity property can be measured conveniently by $\varprojlim^1$, the derived limit: there is a short exact sequence

$$0 \to \varprojlim_i{}^1 \{ H_{n+1}(X_i) \} \to {}^S H_n(X) \to \check{H}_n(X) \to 0$$

where ${}^S H(X)$ and $\check{H}(X)$ denote Steenrod and Čech homology of $X \equiv \varprojlim_i \{ X_i \}$, the inverse limit over compact polyhedra X_i. If the coefficients form a field or a compact abelian group, the $\varprojlim^1$ term simply vanishes, making Steenrod homology and Čech homology the same.

The second explanation, equally correct, but different in emphasis, appeared in an unexpected place. Developing an idea of Grothendieck, Artin and Mazur [1] extended homotopy theory from complexes to inverse systems of complexes for use in algebraic geometry (ètale homotopy). They observed that if one is willing to live with and use the inverse systems, rather than passing to inverse limits, so that the Čech homology group is replaced by a Čech homology pro-group ($\equiv$ inverse system of groups), there results an exact pro-homology functor. Čech's mistake, from this point of view, lay in passing too rapidly to the inverse limit. The topologist's desire for a homology group rather than a pro-group is merely a prejudice. This idea was to reappear in shape theory.

Meanwhile, back in point set topology, the Alexandroff principle was remembered, but not central. It appeared most directly in dimension theory. It appeared from time to time in the topological study of continua and their fixed point properties: some of the pathological examples were usefully considered as inverse limits, while for others the inverse limit point of view, while theoretically there, was not useful. Continua theorists in the forties were often concerned with various exotic definitions of "component", but there was so little contact with the algebraic topologists that nobody from that school seems to have considered the 0th Steenrod homology group as being generated by components of some sort. (In recent times this has been studied — see Krasinkiewicz and Minc [**1**].)

Another group, notably led by Bing, was studying upper semicontinuous decompositions of 3-space in their attempt to settle the celebrated classical questions of 3-dimensional topology. Perhaps their attitude to the Alexandroff principle can be illustrated by the dictum, attributed to Bing by some of his students, that "inverse limits are only good for proving theorems about inverse limits".

In 1960, Case and Chamberlin [**1**] caused a stir among point set topologists by describing a one-dimensional continuum whose Čech homology, cohomology and homotopy groups are trivial, but which possesses an essential map onto the figure eight. Even Milnor's exact sequence above, with $n = 0$, does not explain this. But $\varprojlim^1 \pi_1$ is nontrivial. The concept of $\varprojlim^1$ for nonabelian groups appeared in Bousfield-Kan [**1**] in the early seventies, and has been appropriated by shape theory.

2. Shape theory and geometric topology. The highly selective history given in §1 ignores large parts of topology to which Alexandroff's principle is less relevant. And the term "point set topologist" is overused. By 1960 the point set topologists had split into three rough groupings: the general topologists (who in modern times are intellectually close to the logicians), the ultra-classical topologists such as continua theorists, and the geometric topologists.

Geometric topology, as I use the term, is the study of those properties of euclidean space and of finite- and infinite-dimensional manifolds which are invariant under homeomorphism (as against diffeomorphism, piecewise linear homeomorphism, or homotopy equivalence). The rapid growth of piecewise linear topology during the sixties made new tools available to geometric topology. Although not confined to low dimensions, nor to the United States, the practitioners of gemetric topology in the sixties were mostly associated with the school led by Bing. In the work of these people the concept of *cell-like set* recurred, i.e., a compact subset of a manifold which can be contracted in any neighborhood (although it might not be contractible in itself). In 1968, Karol Borsuk published his solution to a problem which had puzzled him for many years: the problem of what ought to play the role of "map" and "homotopy class of maps" in a Čech homotopy theory of compact metric spaces. He called his new category the shape category. What caught the interest of geometric topologists was the fact that, in the shape category, a compact space is equivalent to a point if and only if it is cell-like. Since shape theory agreed with homotopy theory on nice spaces (ANR's), it looked as if shape theory should replace homotopy theory in the study of manifolds whenever a non-ANR compact subset crops up. And in geometric topology one cannot avoid such sets.

EXAMPLE (SIEBENMANN [2]). For $n \geqslant 5$ a self-map of a closed n-manifold is uniformly approximable by homeomorphisms if and only if all its point-inverses are cell-like sets. There are 3- and 4-dimensional analogues due to Armentrout and Quinn respectively.

The connections between shape theory and geometric topology multiplied. Borsuk's own illustrations, while clever, tended to be rather too classical. But Chapman's result in 1971 *that two compact subsets of an end-slice of the Hilbert cube are shape equivalent if and only if their complements are homeomorphic* [**1**] caused considerable interest. In present-day geometric topology, shape theory is taken for granted as a useful tool. A very good sampler of this interplay is Volume 870 of Springer Lecture Notes, edited by Mardešić and Segal [**6**].

3. Whose child is that? It was argued in §1 that Alexandroff's principle lies neither in point set topology nor in algebraic topology, but on the boundary of both. In modern translation: shape theory lies neither in geometric topology nor in homotopy theory ....

This has led to misunderstanding. Geometric topologists dislike the *theory* in shape theory. For example, an important distinction was developed in the seventies between the shape category and the strong shape category (suffice it to say that Čech homology is an invariant of the former while Steenrod homology is an invariant of the latter). There is no difficulty in phrasing this distinction in a pleasing geometrical way — but try to get a geometric topologist to listen while you explain the correct underlying homotopy theory! On the other hand, homotopy theorists show little interest in shape theory, even in its homotopy theoretic aspects as expounded, say, in Edwards-Hastings [**1**]. Perhaps this is because they perceive shape theory as merely an application of ideas from homotopy theory which were developed for other, more exciting, purposes. But that is to judge shape theory as part of homotopy theory, which it is not.

Misunderstanding also arises from duplication of discovery. For example, Borsuk's two innovations were: the fundamental class ($\equiv$ morphism in the shape category), and the idea of a compact set being movable. It became clear after a while that fundamental classes are essentially morphisms in the pro-homotopy category of Artin and Mazur [**1**], and that movability is a geometric condition on a compact set which assures the vanishing of all $\lim^1$ terms. (Čech homology is exact on the subcategory of movable pairs and shape morphisms.) This is not to imply that Borsuk knew of these connections. He was uncomfortable with abstract ideas. He wanted to "see" his algebraic topology. He rediscovered these concepts geometrically and found use for them. As a second example, we could cite the Whitehead-Theorem-in-proper-homotopy-theory, proved independently by Farrell-Taylor-Wagoner and by E. M. Brown (neither reference is cited in the book under review), which reappeared as the Whitehead-Theorem-in-shape-theory (see Chapter II, §5 of the book). Again there was pay-off. The shape versions explain what is really going on.

The point is that many of the new-sounding terms and insights of shape theory either have been anticipated by, or throw new light on, other aspects of topology. Therefore it is particularly important that a book on shape theory should interpret the subject to the various older constituencies in topology. Shape theory may be nobody's child, but it can claim kinship with many.

4. Prospects. The reader might ask: apart from being a development of Alexandroff's principle, what has shape theory contributed to topology as a whole? And which developments look promising? Fair questions, but who can answer them fairly?

By definition, all that is purely shape theory is to be excluded. Among the new theorems in topology motivated by shape theory, I particularly like: Ferry's theorem that *a topological space homotopically dominated by a compact metric space is homotopy equivalent to a compact metric space* [**3**], the theorem independently proved by Freyd-Heller [**1**] and Dydak-Minc [**Dydak 9**] that *there exists an unsplittable free-homotopy idempotent*, and the theorem of Hastings-Heller [**2**] that *every homotopy idempotent on a finite-dimensional complex splits*. (It would take another essay to say why I like them.)

Among the counterexamples on the borderline of shape and geometric topology, Taylor's *cell-like map which is not a shape equivalence* [**1**] is justly celebrated. It combines homotopy theory of Toda [**1**], or Adams [**1**], with clever geometry to sink a knife into naive analogies between finite and infinite dimensions. Ferry's proof [**2**] that *the compact spiral, although shape equivalent to the circle, is not equivalent to the circle by "cell-like expansions and collapses"*, when set side by side with his earlier theorem [**3**] that *two compacta homotopically dominated by complexes are homotopy equivalent if and only if they are equivalent by "cell-like expansions and collapses"*, raises questions about the role of compactness in homotopy theory, and about the analogy with Whitehead torsion, which remain to be answered. The example of Duvall and Husch [3] of *an n-dimensional compactum which does not embed up to shape in* $\mathbf{R}^{2n}$ because its $\lim^1 \pi_1$ is nontrivial is an unexpected twist in generalizing a PL theorem of Wall and Stallings about embedding polyhedra up to homotopy type: the known case $n = 1$ (solenoid) might have been exceptional but is not: they have examples for $n = 2^k$.

Next, connections outside topology. There have been several links, still tenuous, with functional analysis: one constitutes the background for Taylor's counterexample cited above; another is the work of Kaminker-Schochet [1] on K-homology and Brown-Douglas-Fillmore theory; a third is the recent work of Effros and Kaminker on C^*-algebras and shape theory: the survey article Edwards-Hastings [4] discusses the first two of these. There is a branch of category theory called "categorical shape" possessing a large literature whose value I cannot judge. And then there is the "missing link": it is very surprising that shape theory, being so fundamentally concerned with compact sets, has had no significant interaction with dynamical systems, home of the strange attractor. Perhaps this is because shape theory is too coarse a tool for detecting dynamically interesting phenomena. Or perhaps the right contacts have not been made between the two areas.

Finally, methods: many people know that *the methods of shape theory are also the methods of proper homotopy theory*. As far as I can see, this fact is not mentioned in the book under review, though the methods are presented in appropriate generality. In my opinion, there is more of interest in ends of open manifolds than in compact sets, so I believe that the future of the subject lies primarily in its methods rather than in the theory as presently formulated.

5. The book. The editors of this Bulletin tell their reviewers that "a good book review is a chatty expository essay .... a book is an excuse for a review." I have tried to supply such an essay. I will end with some comments on what the book under review is and is not.

In the authors' words: "The main purpose of this book is to present a systematic introduction to shape theory providing necessary background material, motivation and examples." The book does this in a thorough and scholarly way. But the prospective reader should note the word "introduction" in that sentence. Only the elementary parts of the subject are discussed in detail. The more difficult topics, both homotopy theoretic (e.g. strong shape) and geometric, are dealt with in brief surveys which lack proofs but direct the reader to the literature. In particular, this hard-covered book on shape theory does not contain a proof of Chapman's "complement theorem", stated in §2, which was a milestone in the subject (to be strictly accurate, "if" is proved but not "only if"). Nor are proofs given of any of the theorems and examples I have italicized in §4 (again, to be accurate, the Freyd-Heller-Dydak-Minc theorem is not stated in the book, but its proof is buried in another context on pp. 214–218). The authors justify these omissions on grounds of space-limitation and because these results "depend on theories and techniques from other branches of topology and have already been adequately presented elsewhere".

With the exception of the Chapman theorem, the results italicized in §4 are only available in raw research-paper form. There is a real need, for instance, for an exposition of Taylor's example which does not require the reader go through "$J(X)IV$" by Adams. This material can be given a reasonably self-contained treatment, as can the other examples I have cited of major results in the subject. And all the results cited were available to experts, at least in preprint, before the book was completed. One must conclude that the primary purposes of this book are the education of students and the outlining of the literature. It is not a definitive exposition of the subject, and it is not addressed to the larger topology community.

A striking feature of the book is its huge bibliography which shows that the authors know the kind of history I tried to sketch in §§1 and 2, as well as the recent literature. However, the history is relegated to "who did what when" notes at the end of each chapter (very accurate notes, by the way). Perhaps a broad sweep of history is not to the authors' taste, but I think the subject calls for it. After all, shape theory is hardly elegant mathematics, not even when well written, as in this book. It is technical mathematics, and technical mathematics needs all the justification available.

In summary, this is a book which presents its subject well, but in a rather narrow framework. It is accessible to students at a relatively early stage of their studies, and will direct them to, but not guide them through, the more advanced topics. Those who use elementary shape theory in their work will find this book a convenient reference source.

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Riesz spaces II, by A. C. Zaanen, North-Holland Mathematical Library, Vol. 30, North-Holland Publishing Company, Amsterdam, 1983, xi + 720 pp., ISBN 0-4448-6626-4

The analytic theory of Riesz spaces, which is the study of linear mappings between Riesz spaces, was initiated by F. Riesz in his 1928 address to the International Congress of Mathematicians held at Bologna. In his address, Riesz emphasized the important role played in analysis by partial order and indicated how classical results concerning functions of bounded variation were related to their order structure. His ideas led to the foundation of the theory of vector lattices, or Riesz spaces as they are known nowadays, with fundamental contributions from H. Freudenthal and L.Kantorovitch in the middle thirties. Freudenthal's contribution was the abstract spectral theorem which bears his name, a theorem whose formal resemblance to the spectral theorem for