contraction extension property in terms of the familiar Kirszbraun property (K).

(X, Y) is said to have property (K), if, for any fixed set I, and any pair of families $\{B(x_i, r_i): i \in I\}$, $\{B(y_i, r_i): i \in I\}$ of closed balls in X and Y, respectively, such that $d_2(y_i, y_j) \leqslant d_1(x_i, x_j)$ $(i,j \in I)$,

$$\cap B(x_i, r_i) \neq \varnothing \Rightarrow \cap B(y_i, r_i) \neq \varnothing.$$

Property (K) is then shown to be equivalent to the contraction extension property. This fact is helpful in showing that for a Hilbert space H, the pair (H, H) has the extension property for Lipschitz-Hölder maps. (This result is generalized to pairs (L^p, L^q) in the closing pages of the book.) Moreover, within the class of strictly convex Banach spaces no other pair (X, X) has this property. A similar result, due to S. Schönbeck, holds for pairs (X, Y) where Y is strictly convex and $\dim Y \geqslant 2$, though the proof of this fact is considerably more involved. Without strict convexity of Y the problem is, in general, rather difficult and partial solutions are, therefore, of interest. One of these due to B. Grünbaum (for $\dim X = 2$) and to S. Schönbeck, states that if X is a separable conjugate Banach space, then for (X, X) to have the contraction extension property it must be a Hilbert space or have the binary intersection property for closed balls. Along with property (K) and the above mentioned property, other intersection theorems for families of closed balls are known to be useful when dealing with the extension of contractions, and some of these are presented in that context. Briefly touched upon are the investigations of D. de Figueiredo and L. Karlovitz into the existence of contractive retractions over a closed convex subset of a Banach space, as well as those of F. Valentine on the contraction extension property of (X, X) when X is an n-sphere. In a departure from the main theme several other loosely connected topics are dealt with. Among these are the extension problem for uniformly continuous mappings, and, in a different direction altogether, a packing problem for the unit ball in L^p.

For a slim volume, about a hundred pages long, the amount of material covered is considerable, and while the authors may have omitted some topics which are relevant to the subject matter, and included others which are less so, the balance seems satisfactory. The writing is exceedingly clear and the pace easy to keep up with.

This book should help to arouse a more widespread interest in an area in which the interplay between geometry and analysis is both fruitful and pleasing. As such, it is most welcome.

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A geometric approach to homology theory, by S. Buoncristiano, C. P. Rourke, and B. J. Sanderson, London Mathematical Society Lecture Note Series, no. 18, Cambridge Univ. Press, New York and London, 1976, 149 pp., $10.95.

Let me begin by sketching some background information.

Singular homology theory was put in its present form by S. Eilenberg in 1944 [**6**]. A "singular simplex" in a given space X is a continuous map $f\colon \sigma^n \to X$ from the standard simplex σ^n to the space X; a singular chain is a formal linear combination (with integral coefficients) of singular simplexes; one goes on to define cycles, boundaries and homology groups $H_n(X)$.

The concept of "a homology theory", considered as a suitable functor H_* defined on spaces X, was axiomatised by Eilenberg and Steenrod in 1945 [**7**]. There are six axioms which are more or less formal, and a seventh which says that the homology of a point is what you would think.

The homology theories considered in this book, however, are "generalised" or "extraordinary" homology theories. Such a thing is a functor satisfying the first six axioms of Eilenberg and Steenrod, but not necessarily the seventh; if it satisfies the seventh, it is "ordinary homology". Topologists have been persuaded of the usefulness and interest of "generalised homology and cohomology theories" by many particular examples; prominent among these are the K-theory of Grothendieck, Atiyah and Hirzebruch [**2**] and the many forms of bordism and cobordism–a survey is given by Stong [**8**].

From the point of view of a homotopy-theorist, a good general understanding of extraordinary homology and cohomology theories is provided by the work of E. H. Brown [**4**], [**5**] and G. W. Whitehead [**10**]. However, to work with homotopy classes rather than actual maps always entails a certain loss of precision and control. In the most favourable cases, the homology or cohomology functor can be defined by some explicit geometrical construction, and this construction can be used to keep the requisite precision and control. Classically, in the case of K-theory one has a good geometrical construction for the contravariant (cohomology) theory, in terms of vector-bundles over X; and in the case of bordism one has a good geometrical construction for the covariant (homology) theory; a "singular manifold" in X is a continuous map $f\colon M^n \longrightarrow X$, where M^n is a manifold of the type considered. The latter idea goes back to Atiyah [**1**]. Whenever we have a convenient and direct construction of a generalised homology theory, we hope and expect to see also conformable, convenient and direct constructions of the appropriate ancillary machinery (for example, cup-products, Poincaré duality isomorphisms, cohomology operations ...).

Bordism, as described, is certainly distinct from ordinary homology; there exist homology classes $h \in H_n(X)$ which cannot be represented by any "singular manifold" $f\colon M^n \to X$; this is a theorem of Thom [9], answering a question of Steenrod. However one can recover ordinary homology by varying the definition of bordism so as to allow "manifolds" M^n with very bad singularities. It is easy to make this plausible. A singular cycle c in a space X is the image of a fairly obvious chain c' on a disjoint union of simplexes σ^n; but here c' need not be a cycle. We can make c' into a cycle if we take the simplexes σ^n and identify their $(n-1)$-dimensional faces in pairs in an

appropriate way; the result looks like a manifold so far as the simplexes of dimension n and $(n-1)$ are concerned, but the identifications on the $(n-1)$-dimensional faces may result in very complicated identifications of the lower-dimensional faces, leading to singularities.

It is a basic and valuable idea of Sullivan that one can amend the definition of "bordism" by allowing the manifolds M^n to have singularities of some precisely-controlled type; and that in this way one can get useful and interesting functors.

The basic ideas of this book can now be stated as follows. (i) By introducing a suitable notion of "bundle", namely a "mock bundle", one can give a geometrical construction for the contravariant functor pl-cobordism. The definition is in terms of pl-manifolds. (ii) One can then add knobs and gadgets to the manifolds, in the form of (a) singularities and (b) restrictions on the normal bundle. One thus gets geometrical constructions for a "bordism-type" homology functor and a "cobordism-type" cohomology functor; the authors refer to these as "geometric theories". (iii) One can carry over to these theories all the work which a reasonable man would expect. (iv) The result is general; any generalised homology or cohomology functor can be obtained as a "geometric theory".

What should one think of this programme? The aim of the book seems to be to communicate geometric ideas. The ideas of Sullivan are certainly good. The idea of a mock bundle is almost certainly good–extra evidence will come if other people can use it in other contexts besides the pl-context chosen here. The book contains a lot of geometrical ideas which seem to be appropriate. Certainly one could encourage readers to go to the book and get these ideas.

On the other hand, good mathematics depends on striking the right balance between the particular and the general. (If you concentrate on one particular case you risk proving nothing about any other case; if you erect a general theory you risk making no useful contribution to *any* particular case.) Here I want to pay due respect to the many "particular" features in the book. The authors are never short of a concrete example of interest; the discussion of Steenrod squares looks as if it *could* be used for some application, even if it is not so used in the book; and so on. Moreover, the "theorem of generality" ((iv) above) may have propaganda value; if we know that in principle every theory admits a geometrical construction, that may encourage us to seek good and useful particular constructions in particular cases of interest. But if I have to judge the strategy of this programme, I suppose it errs on the side of generality. I am sufficiently convinced that the geometrical ideas enlighten the mind, in the sense that they allow one to see that one problem is equivalent to another equivalent problem. But it is precisely the "theorem of generality" – the fact that these ideas apply to all cases both good and bad–that makes me wonder if they are of themselves sufficient to advance the study of particular problems. Two examples may help.

(i) On pp. 87–91 we have (a generalised version of) Sullivan's method for

killing elements of the coefficient ring by allowing new singularities; an exposition has been given by Baas [3]. Topologists have long wanted to know rigourously when these theories have products; and it feels like a "geometric" problem. The formal condition for a theory to have products is given on p. 86. Are we any further forward?

(ii) On p. 93 the authors mention "the theories whose coefficients are the surgery obstructions .... Of particular interest is the theory $\Omega^4(G/PL)_* \cdots$. This raises the question of whether there is a convenient description of $\Omega^4(G/PL)_* \cdots$." It's a valid problem; I understand (by private communication) that John Morgan has substantial contributions to it; but do the present authors, by their present line of thought?

Let me finish with a dialogue.

DEVIL'S ADVOCATE. What readers do the authors have in mind?

EXPERT WITNESS. The level of background knowledge assumed would seem to rule out readers outside the subject. There's also a difference between a typical expository book and a research paper in the way they rely on results proved in other places; an expository book will refer you to a textbook for the proof of some standard result, but a research paper will refer you to papers in journals for anything whose proof has been published anywhere. The authors' practice approximates to the latter way. Again, to leave the whole of a proof to the reader is one thing; to leave him to frame the definitions as well would seem to imply a mature reader. I think it's a book for experts.

D. A. Perhaps the authors wrote it to please themselves. But did I hear you imply that it's written more like a research paper than a book?

E.W. To some extent. And I note that on p. 98 the authors refer to the rest of Chapter V as "the remainder of the paper".

D.A. Couldn't the authors get it published as a paper, then?

E.W. I don't know whether they tried. But speaking as a journal editor myself, I think that if they had tried, they might have had difficulty. Journal editors have to weigh the amount proved against the number of pages.

D.A. What do the authors prove, then?

E.W. They prove you can set up a theory like this if you want to.

D.A. Do you want to?

E.W. Me personally? Not much.

D.A. Why not?

E.W. Oh dear, I suppose I have to try and answer that.

Let me remind you what J. H. C. Whitehead said about the biographies of mathematicians. "If they proved bloody good theorems you say so; if not you say what a good effect they had on their pupils." Perhaps it's like that with papers; if they prove "bloody good theorems" you say so; if not you try to weigh up the chances that they will lead to interesting and worthwhile results later. Now I find that I'm already addressing myself to the latter question: what is the chance that someone can make use of this work for some good purpose? So even if that chance is larger than I think, I must unconsciously

already have formed an opinion about the former question.

D.A. I am beginning to infer that when the London Mathematical Society decided to publish this work, they didn't seek your opinion?

E.W. I do wish you wouldn't ask me about matters which are confidential.

D.A. I go on to infer that they must have preferred some other opinion; perhaps someone better qualified by being closer to the subject or more sympathetic to it?

EXPERT WITNESS. This conjecture follows from the former one.

DEVIL'S ADVOCATE. Let us try another expert witness; they come two a penny.

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Foundations of quantum physics, by C. Piron, Mathematical Physics Monograph Series, no. 19, W. A. Benjamin, Inc., Reading, Massachusetts, 1976, xii + 123 pp., $17.50 (cloth) and $8.50 (paper).

Ever since the physicists' discovery that a logically coherent and physically acceptable treatment of atomic and subatomic systems has to be based on principles that are profoundly different from those of classical physics, the problem of understanding and clarifying these principles has engaged the attention of many mathematicians, theoretical physicists, and philosophers. That such discussions continue to go on, and often reveal new aspects fifty years after the original discoveries of the physicists, indicates the remarkable nature of these new ideas as well as the extent of their departure from classical lines of thought.

To trace the origin and development of these ideas is a formidable task; in the framework of the present review it is an impossible one. Suffice it to say that the tremendous difficulties in explaining the mass of spectroscopic data