period of time. I found no errors, either mathematical or typographical, in what was admittedly a rather sporadic reading. In spite of all the detail, the format of the printed page does not tend to oppress, and the formulas and diagrams are well displayed. I should also add that I don't get universal approval when carrying on in the above strain. In my department, for example, I try to push Lang's book "Algebra" as a first year graduate algebra text. It seems to me that it proceeds at an ideal pace, and covers a great deal of material, focusing attention as it does on key ideas, and leaving to the reader what the reader should have left to him. However the book is hardly ever used here, since I am told by my colleagues that the students prefer a book in which everything is done for them. The result is that we invariably use a book in which less material is covered in more space, where subscripts flourish on superscripts, and where little attempt is made to distinguish the important from the routine. Thus I can be reasonably sure that there are those who would prefer the present book to anything I might recommend for such a course. Nevertheless, I maintain that when an author is tempted to include a minor verification, he should ask himself for whom he is doing it. If he is simply satisfying himself that the point is trivial, then he should omit it. But if he thinks he is doing some potential reader a service, he might better consider advising that reader that he is out of his depth. Traffic should not be slowed for the pedestrian walking down the white line. It should rather be suggested that he would be happier on the sidewalk.

BARRY M. MITCHELL

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*Categories of algebraic systems*, by Mario Petrich, Lecture Notes in Mathematics, vol. 553, Springer-Verlag, Berlin, Heidelberg, New York, 1976, viii + 217 pp., $10.20.

*Mal'cev varieties*, by Jonathan D. H. Smith, Lecture Notes in Mathematics, vol. 554, Springer-Verlag, Berlin, Heidelberg, New York, 1976, viii + 156 pp., $7.40.

> . . . ask not what your country can do for you;
> ask what you can do for your country.
>
> J. F. Kennedy

**1.** I was introduced to the concept of a category around 1960 by A. G. Kurosh. He pointed out that the origin of a segment of category theory and a part of lattice theory was in the observation that many results on direct decomposition of groups (e.g., the Kurosh-Ore Theorem and the Ore Theorem) depend very little on the structure of the group. The proofs can be stated very simply in terms of homomorphisms of groups and their properties (that is, in a categorical language) or in terms of the set theoretic inclusion among the normal subgroups of a group (that is, in lattice theoretic language).

Category theory was started by S. Eilenberg and S. Mac Lane [2], [3] as a tool in cohomology theory. In the fifties category theory became a subject matter to be investigated in its own right. The results of these investigations appeared in many papers (quite a few in the Reports of the Midwest Category Seminar), and finally in books, starting with P. Freyd **[4]** in 1964 and B. Mitchell **[18]** in 1965. By the time S. Mac Lane **[14]** appeared in 1971 there were at least a dozen books introducing the reader to this field.

Many of the practitioners of this new field were, just like all other mathematicians, mainly interested in what they can do to (or in) category theory–and thus developed it in directions that were mainly of interest for their collaborators. However, many perceived as their duty to undertake the job of converting the rest of the mathematical community to doing and seeing things "the categorical way". This was (and is) no easy task, especially in the early years. Most results were then only available in manuscripts that were privately circulated. There were no introductory textbooks available; many articles were written in a language that was hard for an outsider to understand. Add to that the demand enunciated by young and enthusiastic category theorists, that all of mathematics should be done their way, and it is easy to understand why the missionary-minded encountered some hostility. Maybe these early experiences explain why so many category theorists appear to be so defensive; see the Preface of B. Mitchell **[18]** for an early example and the article (especially the section on Emoting) of E. G. Manes **[17]** for a recent one.

**2.** What interests me, personally, the most is what category theory can do for algebra. The applications can be classified as follows:

A. *Categories as a language*. Category theory provides a "convenient conceptual language" as pointed out by S. Mac Lane **[14]** and provides some basic results (such as the Adjoint Functor Theorem) that are convenient to use but not indispensable. These are somewhat superficial uses of category theory.

B. *New concepts suggested by category theory*. Conceptually, it is much more interesting that category theory suggests interesting new things to look at. Take, for example, projectivity for distributive lattices; in the category of distributive lattices there are only trivial projective objects. However, a slight change in the definition of a projective object (change "epi" to "onto") gives an interesting concept yielding many interesting results (R. Balbes, A. Horn, R. Freese, J. B. Nation, and others; see, e.g., **[5]**).

C. *Categorical approach to proofs*. Since category theory provides the tools to handle and control large numbers of maps at the same time, it makes possible a "categorical" approach to problems that were handled differently before. An excellent example is the Banaschewski-Nelson proof of Walter Taylor's brilliant result on residual smallness (see **[1]** and **[20]**); although, category theory is not necessary for its presentation, it seems almost indispensable for its conception.

D. *The construction of unusual categories*. Why is it that the deeper results of category theory are hard to apply to universal algebra and lattice theory? The answer is simple. To get deeper results in category theory one needs stronger axioms, for instance that the category should be abelian. However, very few categories of algebras (and none of lattices) are abelian. Thus to apply results about abelian categories to algebras that do not form with their homomorphisms an abelian category, one has to invent artificial categories arising from them.

One such example was found by G. Hutchinson [11]. Starting from a modular lattice $L$ he constructs a category whose objects are the intervals of the lattice and whose morphisms are special diamonds of the lattice (a diamond is a five-element sublattice $\{0, a, b, c, i\}$ satisfying $a \vee b = b \vee c = c \vee a = i$, $a \wedge b = b \wedge c = c \wedge a = 0$). It turns out that under certain conditions on $L$ this category is abelian. Thus one can apply the Freyd-Mitchell Theorem on the representation of abelian categories by modules and obtain deep lattice theoretic consequences. (See also G. Grätzer and H. Lakser [7] for a further application.)

P. Gumm [8] shows another interesting example of how one can obtain from a class of algebras a category whose objects are not the algebras but funny constructs therefrom, and obtain a nice universal algebraic result (unrelated to category theory).

E. *Equational theories* (varieties, equational classes) provide another opportunity for categorical treatment of a chapter of universal algebras. This was initiated by F. W. Lawvere [12], [13] and quite fully covered in book form by E. G. Manes [16]. The objects of the corresponding category (in the finitary case) are the natural numbers and the morphisms correspond to polynomials. Thus the compositions of morphisms reflect the identities (equations) of the theory. This approach handles with ease infinitary operations and topological base sets. It seems less useful as a tool in proving theorems not connected with this approach.

F. *Full embeddings*. An interesting and deep contribution to algebra was made by the Prague group (A. Pultr, Z. Hedrlín, P. Vopěnka, V. Trnková, Z. Kučera, P. Goralčik, J. Sichler) and their collaborators. Their main interest lies in the representation of arbitrary concrete categories by classes of algebras (relational systems, graphs) with special properties. Their results found many applications in practically all branches of algebra: universal algebras, lattices, semigroups, posets, integral domains, etc. I am happy to report that a book, A. Pultr and V. Trnková [19], covering these applications is forthcoming.

3. The title of Petrich's book may be suggestive of universal algebra to some people. Quite the contrary, this book deals with very familiar concrete categories: vector spaces, semigroups, rings, lattices, projective spaces, loops. The topic of this book is how we can pass from one of these fields to another using classical algebraic and geometric constructions. It shows that the natural framework for these constructions is category theory. For instance, the classical Coordinatization Theorem of Desarguesian Projective Geometries here yields a functor and a full embedding.

The most important categories considered are: pairs of dual vector spaces, completely 0-simple semigroups, a certain category of semisimple atomic rings, subprojective lattices, and pairs of dual projective spaces.

There is a very large number (over 80) of categories considered and a larger number of functors; excellent aids are provided by the author (the diagrams at the end of the book summarizing the results, the lists of categories, functors, and special symbols, and the index) making it possible to read the book without getting lost.

Today when most algebraists specialize in one field, it is with real pleasure that I read this book that wanders the length and breadth of algebra. This is recommended reading for all algebraists. This book fits nicely in A in the scheme of §2.

**4.** The book by J. D. H. Smith deals with varieties (equational classes) of algebras with the property that in any algebra in this variety any two congruences $\Theta$, $\Phi$ permute, that is,

$$\Theta\Phi = \Phi\Theta = \Theta \vee \Phi.$$

Such varieties are called congruence permutable in the literature and Mal′cev varieties in this book.

By an easy result of Mal′cev [**15**] a variety has this property iff there is a ternary polynomial $p$ which is $\frac{2}{3}$ minority, that is, it satisfies the identities $p(x, y, y) = p(y, y, x) = x$.

The author's goal is "to show some of the things that can be done with Mal′cev varieties". It is very hard to read this book and see how it fulfills this promise without getting overly critical. To start with, this book creates the impression that the author is invading virgin territorry. This field has been visited by about as many mathematicians as dame de Récamier's salon. It is interesting that one finds no evidence of this in the References; there are only 21 items there of which only three relate to this field and they are dated 1947, 1954, and 1964. There should be a bibliography of, say, at least fifty relevant titles[1] and, in the book, a corresponding wealth of material.

On the positive side, this book introduces an idea that has not appeared in the literature: it is the concept of a centralizer of a pair of congruences; this concept specializes to the usual concept both for groups and for Lie algebras. The main results are in Chapters 3 and 4. Using the concept of centrality in Chapter 3, Ore's Theorem is improved "to the standards of group theory's best corresponding result", while Chapter 4 gives a cancellation theorem for fintite algebras (in a Mal′cev variety): if $B \times D$ and $B \times E$ are isomorphic, then $D$ and $E$ are centrally isotopic (central isotopy is stronger than isotopy). It may be interesting to note that P. Gumm in his short note [**8**] got a somewhat weaker result, proving that such $D$ and $E$ have to be isotopic. It is not clear what is the algebraic significance of getting centrality and whether it is worthwhile to get this stronger result if we have to pay such a price for it in the length of the proof.

It was reported to me by the Darmstadt group that Corollaries 224 and 225 are incorrect. Since these play a central role in subsequent developments this casts some doubt on all the results. A recent paper [**10**] of J. Hagemann and Ch. Herrmann contains a discussion of centrality in congruence modular varieties (this is much more general than congruence permutable varieties).

[1]The reader is referred to [**6**] for a bibliography of universal algebra for the period 1967–1977. There are more than 1100 items listed there.

Their results may provide the tools to correct the proofs in this book.

Finally, let us discuss the basic method employed in this book. The congruence lattice of a finite algebra in a congruence permutable variety is a finite geometric lattice. Thus it is natural to suggest that geometric methods are appropriate. This is the approach taken in P. Gumm [9]. He takes a diamond in the congruence lattice containing the two extremal congruences. This induces geometrical 3-nets and his results quickly follow from this. Even the Mal′cev polynomial will take on a natural geometric meaning. As another example, take Chapter 5. The author investigates equationally complete congruence permutable varieties generated by finite algebras. In such a variety, the free algebra on $m$ generators is always of the form $A^{d(m)}$, where $A$ is a suitable finite algebra. According to the author, an "interesting dichotomy takes place": $d$ is asymptotically exponential in $m$ or $d$ has a linear upper bound (Theorems 535 and 536). As R. W. Quackenbush pointed out to me, the first case corresponds to the congruence distributive case, when the congruence lattice of $A^{d(m)}$ is $2^{d(m)}$, while the second corresponds to the case when the congruence lattice of the free algebra is a projective geometry.

The author takes a very involved categorical approach with huge diagrams of arrows. This appears to be an example where the categorical approach hinders rather than helps. By the scheme of §2, C was the author's goal but he failed to achieve it.

The criticisms voiced in this review do not detract from the merits of this book which contains quotations from diverse sources, from G. F. Handel to the British Railways' Rule Book (1950). Maybe someday we can read a revised version in which the inaccuracies will have been eliminated and the results will be set in their proper place in the rapidly developing branch of universal algebra dealing with congruence modular and congruence permutable varieties.

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G. GRÄTZER

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*General relativity for mathematicians*, by R. K. Sachs and H. Wu, Graduate Texts in Mathematics, Springer-Verlag, New York, Heidelberg, Berlin, 1977, xii + 291 pp., $19.80.

The theory of general relativity is now more than sixty years old. During its formative years, the theory was a constant source of constructive interaction between mathematicians and physicists. Physicists drew heavily upon recent developments in differential geometry, and much research in differential geometry was stimulated by problems in general relativity. Many of the great physicists of the day (e.g., Einstein, Lorentz, Poincaré) were making fundamental contributions to mathematics, and many of the great mathematicians of the day (e.g., Cartan, Hilbert, Weyl) were making important contributions to physics. The years 1900-1930 marked a golden period in math-physics cooperation.

This pleasant state of affairs deteriorated, however, in the late 30s-early 40s. Mathematicians were moving toward a global viewpoint (manifolds, fiber bundles, cohomology) whereas physicists were content to work locally, doing all computations in a single coordinate system. Except for certain rather conjectural cosmological ideas, all the interesting physics (e.g. bending of light, perihelion precession, gravitational redshift, expansion of the universe) could be dealt with adequately without manifold theory. As the interests of geometers and relativists diverged, so of course did their languages. Differential geometers began to use invariant tensor notation and differential forms whereas physicists were content with classical tensor analysis, a language in which they were extremely fluent and which was quite adequate for the computations of interest to them. The years 1940-1970 marked a period of (comparatively) little interaction between geometers and physicists.

Now, in the 70s, the pendulum is swinging back. This is due largely to the fact that physicists have recently been applying global geometric techniques to obtain results of indisputable importance. One set of results (Hawking-Penrose [4]; see Penrose [6] for an account written for mathematicians) says that in any spacetime satisfying certain physically reasonable conditions