

BULLETIN (New Series) OF THE  
 AMERICAN MATHEMATICAL SOCIETY  
 Volume 1, Number 6, November 1979  
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 0002-9904/79/0000-0511/\$02.50

*Differential forms in mathematical physics*, by C. von Westenholz, Studies in Mathematics and its Applications 3, North-Holland Publishing Company, Amsterdam, 1978, xv + 487 pp., \$62.25.

An example or two will give the flavor of the subject. First, let  $M$  be an  $n$ -dimensional smooth differentiable manifold, thought of as the configuration space of a mechanical system with  $n$  degrees of freedom. Each point of  $M$  has a neighborhood with a local coordinate system  $(q^1, \dots, q^n)$ . When the system is in motion we need not only the coordinates  $q^i$  of a point of  $M$ , but also the momentum vector  $(p_1, \dots, p_n)$  at  $q$ . Thus we are led to the *phase space*, or *cotangent bundle*, of  $M$ , denoted  $T^*M$ . This space already has an interesting structure: the differential form of degree one with local expression

$$\omega = \sum p_i dq^i$$

is really a global quantity on  $T^*M$ . Its exterior derivative

$$\Omega = d\omega = \sum dp_i \wedge dq^i$$

is automatically a global quantity on  $T^*M$ , an exterior differential form of degree two (skew-symmetric covariant 2-tensor). The equations of motion of the system are described in the following way by a real function  $H$  on  $M$ , called the *Hamiltonian* of the motion:

There is a contraction process, called the *interior product*, that contracts  $\Omega$  with any vector field  $X$  on  $T^*M$  to produce a differential form of degree one (one-form for short)  $X \lrcorner \Omega$ . If  $\Omega$  is thought of as an alternating bilinear functional on vector fields, then

$$(X \lrcorner \Omega)(Y) = \Omega(X, Y)$$

exhibits  $X \lrcorner \Omega$  as a linear functional on vector fields, that is, a one-form. It turns out that  $X \rightarrow X \lrcorner \Omega$  is an isomorphism on the space of vector fields onto the space of one-forms, so there is a unique vector field  $X_H$  such that

$$X_H \lrcorner \Omega = dH.$$

A short calculation in the local coordinate system  $q^i, p_i$  yields

$$X_H = \sum \frac{\partial H}{\partial p_i} \frac{\partial}{\partial q^i} - \frac{\partial H}{\partial q^i} \frac{\partial}{\partial p_i}.$$

(A vector field here is thought of as a directional derivation on the space of real functions.) Therefore, in local coordinates, a curve  $q^i = q^i(t), p_i = p_i(t)$  is a trajectory of  $X_H$ , provided that

$$\frac{dq^i}{dt} = \frac{\partial H}{\partial p_i}, \quad \frac{dp_i}{dt} = -\frac{\partial H}{\partial q^i}.$$

These are precisely Hamilton's equations of motion of the system.

From this example we see that a lot of the structure of differential geometry: manifolds, bundles over manifolds, induced structures, vector

fields, differential forms, and invariant operations fit hand-in-glove with classical mechanics. Indeed, Hamiltonian mechanics is the subject of a substantial chapter of the book under review.

For a second example, we consider the space  $M$  of equilibrium states of a thermodynamic system, again a smooth differentiable manifold. Important real functions on  $M$  are the *pressure*  $P$ , the *volume*  $V$ , and the *internal energy*  $E$ . The physics implies the existence of an integrating factor  $T^{-1}$  for the one-form  $dE + PdV$ . This means that

$$d(T^{-1}(dE + PdV)) = 0.$$

Then  $T$  is called the *temperature* of the system; it follows that (locally) there is a function  $S$ , called *entropy*, such that

$$dS = T^{-1}(dE + PdV).$$

This topic is treated on pp. 190–193 and later on pp. 235–239, where the existence of the integrating factor is connected to the nonexistence of a perpetual motion machine.

The author's selection of material is much more ambitious than just differential forms and their applications. The book could justly be titled *Differential geometry in mathematical physics*. Besides the inevitable preliminaries on topological spaces and multivariable calculus and the standard stuff on differentiable manifolds, maps, and tangent vectors, there are substantial treatments of Lie groups, fibre bundles, tensors, forms, completely integrable systems, first order PDE's, integration of forms, de Rham and Hodge theory, connections on bundles, and Riemannian geometry.

The applications to physics cover electrodynamics, classical and relativistic, rigid body dynamics, holonomic systems, a touch of particle physics, general relativity, relativistic fluid mechanics, and the two topics I mentioned at first.

The goal of the book is stated in the first paragraph of the preface:

“The radical change of methods used in mathematical physics (quantum field theory and elementary particle physics, solid state physics, theory of dynamical systems etc.) influenced by the great power of modern mathematics calls for a monograph such as this which is aimed primarily at making available to physical scientists the mathematical machinery related to differentiable manifold ideas relevant to physics. The reader will find that the ‘geometry spirit’ working methods provide a new marvellously unified set of tools for an alternate description of natural phenomena which goes beyond the description obtained in terms of analytical methods.”

The choice of topics, high purpose, and obvious knowledge of the author, a physicist, would appear to portend a really useful contribution to the literature. I am sorry to report that, in my opinion, the book will not achieve its goal. Why? Briefly, the book makes unreasonable demands on the reader. The density of misprints and errors, serious and not-so-serious is high. I cannot believe that any mathematician read the manuscript critically, that the

author read the proofs with care, or that the editor fulfilled his responsibility. Next, hundreds of terms are defined, yet the index is inadequate to the point of being useless. Also, hundreds of special symbols are defined, but there is no index of notation whatever. Frequently you must remind yourself of a term or symbol by searching. Yet the definitions (often not italicized) are randomly scattered in *Definitions, Remarks, Examples, Propositions*, and text, so even searching is more tedious than usual. Since many potential readers of this book will come to it with previous knowledge of some or most of the standard differential geometry and will wish mainly to browse through the appealing-looking applications, some way to locate things quickly is essential—but missing.

The student or physical scientist who tries to learn the math from this book will have two other problems. First, the references to other books omit page numbers. I consider references like Chevalley [7] rather cruel. The reader is invited to hunt through terrain perhaps written in a mathematical language rather different from the author's. Second, a number of topics, particularly applications, are given before all the needed tools are developed, so forward references are given. It isn't easy, particularly for a student, to have to interrupt a derivation in midstream for material chapters later.

I hope I will be excused for including some details. Let's start with Chapter 1, *Topological preliminaries*. It introduces topological spaces both by open sets and by neighborhoods and the usual stuff on closure, interior, boundary, continuity, homeomorphism, countability,  $T_2$ , compactness, local compactness, paracompactness, connectedness, arcwise connectedness, relative topology, quotient topology, product topology, metric space, normed linear space and more. In 18 pages this is breathtaking, but still there is room for errors that already raise doubts about the mathematical reliability of the book. For instance in Example 2.3, p. 4, we are told that the set of open intervals in  $R$  is closed under union. A base is defined on p. 5, but subbase is not, yet in Proposition 6.4, p. 15, we are told that the sets making up the usual subbase of a product space is a base. Example 5.15, p. 13, offers  $\{(x, \sin 1/x) \in R^2 \mid 0 < x \leq 1\}$  as an example of a nonarcwise connected set. In my opinion this chapter should have been omitted; instead, just a couple of precise references and a page of topological notation would have been adequate.

Chapter 2, *Differential calculus on  $R^n$* , contains what one would expect on differentiable functions from an open set in  $R^n$  to  $R^m$ . Where the line is drawn can be judged from the inclusion of proofs of the chain rule and the existence of partials of a differentiable function, but statements without proofs of the symmetry of the second partials, Taylor's theorem, the implicit function theorem, etc. References for omitted proofs are sometimes given, sometimes not. A mean value theorem for  $R$ -valued functions on a convex open set in  $R^n$  is proved (p. 33) then applied (p. 37) as if true for  $R^n$ -valued functions to give an incorrect proof of injectivity of a map with nonzero Jacobian. The author might have included on p. 32 some hint at the proof that the existence of continuous first partials implies differentiability.

Chapter 3, *Differentiable manifolds*, includes charts, atlases, product manifolds, partitions of unity, and some examples. Of only 13 pages, one whole page is given to a counterexample showing that  $T_2$  is not free of charge. Why

include such pathology? I think the explanation lies in a highly contagious disease carried by mathematicians and caught by nonmathematicians who write mathematics. The symptoms are being more complete, more abstract, more pedantic, and more functorial than even the mathematicians! (A frequent side effect is finding the nonobvious trivial.) I have felt uneasy in reading von Westenholz that at times there are too many  $\forall$ 's, too many induced mappings and complicated diagrams, too many levels of structure, and too much material that he never applies. To his credit, no categories! There is too much trouble taken with  $C^k$  in Chapter 3 when only  $C^\infty$  will be used.

I suppose one might excuse inaccuracies in the three preliminary chapters on the grounds that the author just felt he had to include the material for completeness, but his heart wasn't really in it. So now let's see some of the difficulties in the heart of the subject.

The notation is often careless. Sometimes the same symbol is overworked. For instance on p. 396,  $G$  is a matrix; on p. 407,  $G$  is a group; and on p. 412 a matrix again, and all this in §2 of Chapter 12. Other times there are too many notations for the same thing. If  $\Omega$  is a 2-form and  $X$  a vector field, then

$$\Omega(X) = i_X(\Omega) = X \lrcorner \Omega$$

(pp. 396, 399, 407). On p. 399 we also have  $X(\Omega)$  introduced as a synonym for  $L_X(\Omega)$ . I find  $X(\Omega)$  and  $\Omega(X)$  in the same formulas and proofs confusing. I warn the unwary that their relation is

$$X(\Omega) = d[\Omega(X)] + X \lrcorner d\Omega.$$

On p. 407, in the midst of Hamiltonian mechanics, there is a theorem about invariant integrals on a symplectic manifold of first Betti number 0. The proof is a good example of many in the book that leave the reader not knowing where he stands. First, the condition on  $b_1$  is not mentioned in the proof, so the reader must figure out for himself how it is used. Second, a vector field  $X$  just appears in an equation in the proof—not a word explaining where it came from—and the punch line is  $X(f) = 0$ . Finally, line 5 of this 8 line proof is

$$Y \lrcorner \Omega + d(Y \lrcorner \Omega) = 0.$$

Obviously there is a misprint, but where? Perhaps it should be

$$Y \lrcorner \Omega = d(Y \lrcorner \Omega) = 0.$$

This is possible because the author does allow equations like  $a = B = 0$ , where  $a$  and  $B$  live in different spaces. Actually, the equation should be

$$Y \lrcorner d\Omega + d(Y \lrcorner \Omega) = 0.$$

A small point perhaps, but the cumulative effect of proof after proof with misprints, omissions, nonsequiturs or fatal errors is demoralizing.

On p. 395, Lemma 2.3, the key step towards an important theorem of Darboux, is followed by "Proof omitted". Unfortunately the statement of the lemma is incorrect—a correct statement and proof is in Godbillon [2, p. 118].

The inaccessibility theorem of thermodynamics is allegedly proved on p. 237. The crucial step consists (roughly) in showing that if  $\Delta$  is a linear

space of vector fields and if  $\Delta''$  is the Lie algebra  $\Delta$  generates, then any point that can be reached by a trajectory of  $\Delta''$  can be reached by a trajectory of  $\Delta$ . This is a subtle point, and by no means easy to visualize or see how to prove. Yet the author just states it as if self evident (p. 237, lines 10\*-9\*). A sketch of the proof can be found in Hermann [3, p. 249].

As I said earlier, you often just don't know where you stand. On p. 93 it is stated as an example that the Lie algebra of  $GL(n, \mathbf{R})$  is  $M_n(\mathbf{R})$  with  $[A, B] = AB - BA$ . This is not something you just see, not the first time anyhow. There is no mention of whether this is obvious, needs to be proved, or is too hard to prove here. Nothing, just the bare statement of fact. Will the student who has spent some time puzzling over the assertion be pleased when he comes to p. 102 and finds Proposition 8.11 saying the very same thing? And will he be pleased when he finds that the author has somehow forgotten to prove the main point, the commutator formula?

As another example of an incomplete proof, on p. 145 we are told we shall prove that if  $X$  is a vector field and  $\omega$  a one-form, then  $\omega(X)(x)$  depends only on  $X(x)$ . What is actually proved is that if  $X$  vanishes on a neighborhood of  $x$ , then  $\omega(X)(x) = 0$ . To complete the proof requires something like the device that if  $X(x) = 0$ , then locally  $X = \sum f_i X_i$ , where  $f_i(x) = 0$ . On p. 342, in proving a related theorem about connections, the author uses just this device, pulling it out of thin air without a word of comment.

Why does the proof on p. 107 of  $[X, Y] = (d/dt)_0 \text{ad}(\exp tX)Y$  use the Lie derivative, given first on p. 129? And why on pp. 129-130 is  $L_X Y = [X, Y]$  omitted? Why does the definition of the Hodge star on p. 148 use index raising as if we know the whole classical tensor analysis in Riemannian structures? And why are Riemannian structures used on pp. 128, 147, 195, 289 and others when we don't officially define them until p. 350 (the only index reference)? And why on p. 350 are we told  $ds^2$  is a "quadratic differential form", an animal never mentioned heretofore? And why can't the index include Divergence pp. 153, 290, Thermodynamics pp. 190, 235, Vector Field p. 71, Little group p. 112, etc.?

A few mathematical quibbles arise also. The author omits defining infinitesimal transformation in his discussion of homogeneous spaces around p. 108. The concept is used on p. 407 without previous mention as far as I can see (of course the index doesn't help); the closest thing I can find is infinitesimal automorphism defined on p. 399 and meaning something quite different.

The basic relation  $d\phi[X, Y] = [X', Y']$  for  $\phi$ -related vector fields should have been included. It would have been useful on p. 107 and elsewhere. A special case is proved on p. 95 for group homomorphisms.

The presentation of the invariant form of the Maurer-Cartan equations

$$d\omega + \frac{1}{2}[\omega, \omega] = 0$$

on p. 177 misses the point, even though presented in four ways. It appears as though  $[\omega, \omega]$  is defined so as to make the formula above a tautology. Incidentally, the author should have included something about invariant integration in Lie groups.

The proof of the change of variables formula for multiple integrals,

Proposition 3.1 on p. 268, is a nonproof as far as I can see—if you know the formula, then it is true. It is too central to the theory of integration of forms to be treated this way.

The proof of the divergence theorem on p. 291 would be much simpler and clearer if the author used orthonormal moving frames, a topic I feel he does not exploit adequately in a number of places. Also, the messy calculation on p. 290, needed for the divergence theorem is only the obvious result that  $f = \pm * d * \alpha$  follows from  $fdV = d * \alpha$ .

The discussion of Maxwell's equations on p. 329, as I read it, says that to prove Maxwell's equations it suffices to show that a certain 2-form  $\omega$  is harmonic (see lines 6\*–5\*). This means, in harmonic form language, that  $\Delta\omega = 0$  implies  $d\omega = 0$  and  $\delta\omega = 0$ , a true statement in *compact* manifolds (p. 320), but not applicable here. What is more, the issue is clouded because the Hodge theory (and indeed almost everything else metric in the book) is stated for Riemannian manifolds, whereas here we are dealing with a Lorentz metric (invariant +, –, –, –). This is an alert for those attempting the last chapter of the book, *General theory of relativity*. The author sometimes assumes that facts about Riemannian manifolds carry over automatically to the Lorentz (pseudo-Riemannian) case. Sometimes they do, sometimes they don't.

I conclude that someone who is reasonably familiar with the mathematics of this book will be able to get something out of the applications to physics, provided he works at it harder than he should have to and doesn't accept the author's mathematics at face value. I doubt very much if someone can learn the subject from this book without extensive work in other sources. (Also the author gives no exercises.) The book may repel rather than attract, exactly the opposite of its author's intent. Too much of the book was written without adequate organization and care, and I get the feeling that some sections must have been written at wide time intervals. It's a pity that the author didn't make a more palatable product. The subject will eventually be a standard tool in physics, but there is yet little material accessible to nonmathematicians. My own short book [1] doesn't begin to compare in its scope of applications to the book under review because I didn't when I wrote it and never will know physics as Professor von Westenholz does.

#### REFERENCES

1. H. Flanders, *Differential forms with applications to the physical sciences*, Academic Press, New York, 1963.
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3. R. Hermann, *Differential geometry and the calculus of variations*, Academic Press, New York, 1968.

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BULLETIN (New Series) OF THE  
AMERICAN MATHEMATICAL SOCIETY  
Volume 1, Number 6, November 1979  
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0002-9904/79/0000-0512/\$01.75

*Combinatorial group theory*, by Roger C. Lyndon and Paul E. Schupp, *Ergebnisse der Mathematik und ihrer Grenzgebiete*, vol. 89, Springer-Verlag, Berlin and New York, 1977, ix + 339 pp.