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Nonlinear Systems Theory. By JOHN L. CASTI. Academic Press, Orlando, FL, 1985. xii + 261 pp. $45.00. ISBN 0-12-163452-3. Mathematics in Science and Engineering, Vol. 175.

The author begins the preface of this book by quoting Wittgenstein's dictum, "whereof one cannot speak, thereof one must be silent." He then goes on to explain that "until very recently it was mathematically impossible to speak meaningfully of the system-theoretic properties of nonlinear systems." But things have changed for the better, and the current volume "repesents an attempt to satisfy Wittgenstein's criterion by showing that is is *now* possible to 'speak' about nonlinear systems."

The author does indeed "speak" about an impressively large number of things, ranging from the definition of elementary mathematical objects such as "equivalence relations" (p. 31), "groups" (p. 26) and "manifolds" (p. 36) all the way to "automorphic functions" (p. 50), the "estimation Lie algebra" of a nonlinear filtering problem (p. 185), "finite determinacy of jets" (p. 215), "chaos and strange attractors" (p. 226) and "the germ of the elliptic umbillic catastrophe" (p. 249). Whether what he says about them is correct or intelligible is, however, a different matter. Actually:

1. The exposition is fragmentary. It is as though pieces had been randomly selected from mathematics books and articles and transcribed, without anyone's checking whether the omitted fragments contained information crucial for the understanding of what was kept in.

2. Even more seriously, it is as if the text of the transcribed fragments had been altered and rendered either unintelligible or wrong. In almost every page I looked at, in almost every definition, explanation or proof, as well as in the problems and exercises, I found serious conceptual errors. These are of at least three kinds, namely,

a. Mathematical assertions that are clearly incorrect, such as the statement that the directional derivative of a real-valued function in the direction of a vector field is a row vector-valued function (cf. below).

b. Incorrect mathematical assertions that are not explicitly made in the text but are clearly implied (e.g., when the author states—incorrectly—a theorem for the case when the state space M is a compact Lie group G, and then says that "as a special case we can consider $M = GL(n, \mathbf{R})$," we can reasonably assume that the author believes that $GL(n, \mathbf{R})$ is compact, even though this is not said).

c. Statements that are most probably incorrect but are not so clear that we can tell for sure, as on p. 55, lines 7 and 8, where the author says that there is a one-to-one correspondence between a Grassmann manifold and the space of all linear maps between two linear spaces. It is clear that he is not just saying that the two objects have the same cardinality, but he is talking about a specific correspondence probably described by an earlier construction. This would probably mean that the correspondence would actually establish a homeomorphism between the two spaces, contradicting the fact that the Grassmann manifold is compact. (I had to insert the word "probably" because it is impossible to figure out which of the maps listed earlier by the author is the one that is supposed to be the said correspondence.)

d. Cases where the basic grammar of mathematics gets distorted beyond recognition, so that we find, for instance,

- references to things such as "the set of all elements of G that satisfy the following properties," followed by a list of statements that do not look at all like properties of elements of G (cf. for instance the definition of "module" quoted below, which presents the same logical problems as, say, defining "the set of all integers that satisfy $a + b = b + a$ and $a + (b + c) = (a + b) + c$");
- sentences whose meaning gets lost because quantifiers are in the wrong place (as in the definition of "tensor product" quoted below), or because

some other logical connective is omitted or in the wrong place (cf. the discussion below of the definition of "equivalence relation");

- systematic failure to distinguish between properties of a set, properties of relations on the set, and properties of elements of a set, as in Problem 4, p. 63, where the author defines a set S (the set of all systems with m inputs, p outputs, and n-dimensional state space) and asks the reader to prove that a certain binary relation between elements s of S is an equivalence relation on s (sic) if and only if the two elements that the relation connects are completely reachable and completely observable. (This presents the same logical difficulty as if you were given a certain binary relation on the set of integers and then were asked to prove that it is an equivalence relation if and only if the integers are prime.)

To illustrate Point 1 take, for instance, the development of "some ideas from differential geometry," which begins in §III of Chapter Two. The reader is told what a differentiable manifold is, by means of the usual definition in terms of charts. But the definition involves the concepts of homeomorphism and diffeomorphism, neither of which is explained. More seriously, *the definition is never put to any use.* (We are never given an example of how to define charts to show that something is a manifold. And, when the basic concepts of Lie derivative or Lie bracket are defined, the definition is simply by means of a formula involving coordinates, as if we were in Euclidean space. The author omits to mention the most important conceptual point of the whole theory, namely, that certain things can be defined in terms of a particular chart but turn out not to depend on the chart, and this is what makes them interesting.) Then the unimodular and orthogonal groups are said to be examples of manifolds, though no indication is given of how we might go about defining charts for them. Then the reader is given a definition of a tangent vector—which is incorrect; cf. below—as a map on the set of smooth functions on the manifold, but he is not told what a smooth function on a manifold is. (Strangely, when the author introduces tangent vectors on p. 38, he uses the definition as differential operators, although his goal is to talk about vector fields and integral curves. Yet later, on page 153, when he actually wants to differentiate a function in the direction of a vector field, he tells us that, to do this, "we need the idea of Lie differentiation, a multidimensional generalization of the directional derivative." He then defines the directional derivative of a function ϕ in the direction of a vector field h by means of a formula (cf. below) and uses the notation $L_h\phi$ for the Lie derivative. He seems unaware of the fact that, according to his own definition, $L_h\phi$ is just $h\phi$.)

Next vector fields are defined. (The definition is not entirely correct, since the author introduces the notation T_mM for the tangent space to the manifold M at the point m, and then says that "a vector field v on M is now understood to be a map $m \to T_mM$." Also, on p. 40 we have a "vector field $v{:}M \to T_mM$." What the author really means is "a map that to each $m \in \mathrm{M}$ assigns a member of T_mM.") Next we are told what it means for a curve to be an "integral manifold" of a vector field, but the definition involves the concept of "tangent vector to a curve," which is not defined. Next Lie brackets of vector fields are defined. The section ends there, the chapter continues with two more sections on other topics, and then in the problems the reader is asked to prove, among other things (a) that the gradient of a function on "a Riemannian manifold with metric G" is well defined, (b) that the equivalence relation between smooth maps that is used to define "germs" is indeed an equivalence relation, and (c) that the space of k-jets of diffeomorphisms leaving a point fixed is a group.

This is done despite the fact that the reader is not told what a Riemannian metric, a Reimannian manifold, a smooth map, or a diffeomorphism are. Chapter 2 is followed by a chapter with an "algebraic flavor," and then we get to Chapter 4, where some concepts such as reachability are defined. Exponentials of vector fields suddenly show up, in the form of a reference to "the smallest subgroup of diff(M) that contains exp(tf) for $f \in \{f_i\}$," where f_i is a family of vector fields. This happens despite the fact that we were never told what the exponential of a vector field is, or what the flow is, or what a diffeomorphism is. (Perhaps the reader is supposed to know that the exponential notation refers to the flow of the vector field, and that the flow is defined using the integral curves. But, if the reader is already supposed to know that, what is the point of devoting one page to the definition of integral curve?) The Lie bracket is defined again, and we are told that the vector fields on a manifold form a Lie algebra. (The concept of a "Lie algebra" is not defined. In Chapter 2 there is a section called "Lie algebras of vector fields," but Lie algebras of vector fields, or abstract Lie algebras, are not defined in that section or anywhere else in the book.) No mention is made of the Jacobi identity, but we are told that the Lie algebra generated by a set F_0 consists of the linear combinations of elements of the form $[f^1, [f^2, \cdots [f^i, f^{i+1}] \cdots]], f^i \in \mathrm{F}_0$, a fact which is by no means obvious, and depends on the Jacobi identity. After more results on reachability, we suddenly find, on page 127, an example of "systems on Lie groups." No explanation is given of what a Lie group is. The author's idea of a system on a Lie group is, simply, to take a system defined by a collection of vector fields on the group. He does not require that these vector fields be somehow related to the structure of the group, e.g., by being translation invariant. Thus he is really talking about arbitrary systems on a manifold that happens to be diffeomorphic to a Lie group, even though the theorems he quotes are not valid in this generality. (For instance, it is not true that, if you have an arbitrary collection of vector fields, that are not necessarily translation invariant, then the Lie algebra they generate "determines a Lie subgroup.") In the example, he states a reachability condition for systems on compact Lie groups. He says that the system is reachable if and only if the Lie algebra generated by its vector fields is the Lie algebra of G. This does not make sense unless the vector fields are translation invariant, a fact that is never mentioned. (Just in case you think this is an oversight, the author's precise statement is that reachability holds if and only if "$\{p(x), g^i(x)\}_{\mathrm{LA}}$ is the Lie algebra of G for all $x \in \mathrm{G}$." The notation "$\{ \cdots \}_{\mathrm{LA}}$" means "Lie algebra generated by." If the vector fields are really meant to be invariant, but the author merely forgot to say it, then the words "for all $x \in \mathrm{G}$" are incomprehensible.) Since the Lie algebra of a Lie group is not defined either, the uninformed reader is likely to be confused. Moreover, even if the vector fields are translation invariant, the theorem is false unless the group is connected. Perhaps *this* is an oversight. Yet the author applies the theorem, which is only true for compact connected Lie groups, but is stated by him as true for compact Lie groups, to the group $GL(n, R)$, *which is neither connected nor compact,* and happily concludes that the reachable set from the identity (which is obviously connected) is equal to the group (which is not) if a certain Lie algebra equals the algebra of the group. In addition, the author states his Lie algebra condition as the requirement that the Lie algebra generated by certain elements *be equal to the group.* Strange as it may seem, this makes his result true in the end. It is *never true* that the Lie algebra generated by some vector fields equals the group $GL(n, R)$, and it is *never* true either that the reachable set equals the group, so both conditions really are equivalent after all. Could this be what the author was trying to say? Let us quote one more "geometric" example. Thom's theorem on the classification of elementary catastrophes is quoted on p. 218, and on p. 249 the reader is asked to prove that a certain *function* V_0 "is the germ of the elliptic umbillic catastrophe." Leaving aside the minor (?) detail that a *function* is not the same as a *germ*, there is the problem that the words "elliptic umbillic" are never explained. (When Thom's theorem is stated, only the formulas for the elementary catastrophes are given, not the names.)

Now, this reviewer has no quarrel with a book in which some things are assumed to be known by the reader, and then used to explain some other things. It would be perfectly all right to write a book about nonlinear systems which takes for granted familiarity with manifolds, vector fields, and Lie brackets, and explains how these things can be used to prove nice theorems in control theory. Or one could write a book in which the background on manifolds is explained first. What I find hard to understand is the reason for giving a *partial* account that omits so much that it is unintelligible, and then *doing nothing with it.* A reader who already knows correct forms of the definitions or theorems may be able, with some effort, to reconstruct them from the versions given in the book. A reader who does not know them will find it hard to figure out what the correct statements might be. But neither reader will learn anything.

The preceding analysis was primarily intended to establish the first of our two points, namely, that the exposition as it stands consists of fragments of meaningful texts, but the fragments by themselves do not add up to a coherent whole. We now proceed to the other main point, i.e., our assertion that the exposition contains numerous errors of several kinds, as indicated above. This will be illustrated by means of a list of representative examples (to which should be added our earlier discussion of "systems on Lie groups"). But, before reading the list, the reader must be aware that the examples have *not* been selected from sections or remarks that are marginal to the author's main interest. The author himself says in his preface (page x) that:

> "This book is totally algebraic and geometric in flavor, totally eschewing methods of classical and functional analysis for the study of nonlinear processes. I make no apologies for slighting analysis in favor of algebraic and geometric approaches to the basic questions of systems theory. Prejudices are prejudices, and mine are such that I feel algebraic-geometric ideas have much greater potential for unlocking the mysteries of nonlinear processes than do the tools of analysis."

And, a few lines later:

> "Personally, I have always found probability theory and stochastic processes to be tediously and insanely boring and of little intellectual interest (this is not to deny their obvious practical utility, only their intrinsic value as an expression of the way things *are*), and have resolutely tried to avoid their suffocating and obfuscating effect whenever possible."

Even though this reviewer finds the author's stated "prejudices" unwarranted, and his gratuitous attack on other disciplines offensive and unnecessary, an effort has been made to be fair to him. *The list that follows has been compiled with scrupulous respect for the author's prejudices.* As the reader will see, all our examples are "algebraic" or "geometric."

1. *The definition of "group isomorphism" on p.* 26 *is wrong.* The author forgets to require that the map be surjective. This omission will make it quite hard for the reader to appreciate the meaning of the state space isomorphism theorem for systems on a group, quoted by the author on p. 27.
2. *The definition of "ring" on p.* 27 *is wrong.* The author fails to require that the addition operation be commutative.
3. *The definition of "ideal" on p.* 28 *is confusing, and quite possibly wrong* (but not so clear that we can tell for sure whether it is wrong). The author is

talking about a ring R with operations $\circ$ and $*$, and says: "Let I be a subset of R which is closed under both of the operations $\circ$ and $*$, i.e.,

$$i_1, i_2 \in I, \quad r_1, r_2 \in R \Rightarrow (r_1 * i_1) \circ (r_2 * i_2) \in I.$$

The set I is called an *ideal* of R. Intuitively, an element of I absorbs the product of itself with any element of R." Notice, to begin with, that the author says two different things, which are inequivalent and at least one of which is false. The things coming before and after the "i.e." are not equivalent. The former certainly is not a correct definition of an ideal (e.g., it would make the positive integers into an ideal of the integers). Whether the latter is correct is harder to determine. It clearly is correct for rings with identity, and it clearly is not correct for general rings, for it fails to require that the negative of an element of the ideal also be in the ideal. What is *not* clear is which of the two cases the author has in mind. It is one aspect of his peculiar expository style that we can seldom tell exactly what assumptions are being made about the objects under discussion. For instance, "ring with unit" has been defined before. Should we assume that, from that moment on, the author is only considering rings with identity?

4. *The definition of "subring" on p.* 28 *is also wrong.* The author fails to require that the additive inverse of an element of the subring also be in the subring. His definition amounts to saying that a subring is a subset that contains the identity and is closed under addition and multiplication. (Now the definition does not work even for rings with identity since, for instance, it would make the nonnegative integers into a subring of the integers.)
5. *The definition of "module" on p.* 28 *is unintelligible.* Here it is, quoted in full:

   Given a ring R and an abelian group G, the R-module M consists of the set of elements of G satisfying

   (i) $\quad r(g_1+g_2)=(rg_1)+(rg_2),$

   (ii) $\quad (r_1+r_2)g=(r_1 g)+(r_2 g),$

   (iii) $\quad (r_1 r_2)g=r_1(r_2 g),$

   (iv) $\quad 1g=g,$

   for all $r, r_1, r_2 \in R$, $g \in G$.

   So every time you have a ring and an a group, even if there is no given binary operation connecting them, this gives rise to *the* R-module (sic). And the group is not asked to be abelian. And the reader will of course find it impossible to figure out what it means for an element of G to satisfy properties (i)–(iv).
6. *The definition of "equivalence relation" on p.* 31 *is wrong.* The author states the usual three conditions of reflexivity, symmetry and transitivity, but each of the first two loses an implication, so that the first two properties become:

   (i) $\quad s \in S, \qquad (s,s) \in \lambda$ (*reflexive*),

   and

   (ii) $\quad (s_1, s_2) \in \lambda, \qquad (s_2, s_1) \in \lambda$ for all $s_1, s_2 \in S$ (*symmetric*).

   (Here λ is the relation. Notice that (ii), as stated, just says $\lambda = S \times S$. The transitivity condition, by contrast, is stated correctly.)

7. *The definition of "tensor product" on p.* 34 *is wrong.* The author says that "given k-vector spaces V_1, V_2, V_3, and V_4, together with the bilinear map B: $V_1 \times V_2 \to V_3$, we call (V_3, B) a *tensor product* for V_1 and V_2 if" and then states the usual requirements that the image of B span V_4, and that, for every bilinear map $V_1 \times V_2 \to V_4$, the map should factor uniquely through B and a linear map $V_3 \to V_4$. This almost sounds like the real things, except that, in the correct definition of tensor product, V_4 is *not* given, and the factorization condition is required to hold for all V_4's.
8. *The definition of "tangent vector" on p.* 38 *is wrong.* The author defines it to be a map on the space of C^∞ functions on the manifold that satisfies the usual properties. But he takes the map to be into $\dot{R}^n$ rather than R.
9. *The definition of "Lie bracket" given on p.* 43 *contradicts the one given on p.* 116. The former corresponds to setting $[f, g] = (\partial f/\partial x)g - (\partial g/\partial x)f$, while on p. 114 we find $[p, g] = (\partial q/\partial x)p - (\partial p/\partial x)q$. (Of course, authors sometimes change notational conventions, but they usually tell the reader. In our case, the reader is told that both definitions are the same. The formula on p. 114 is preceded by the words "in Chapter 2 we introduced the Lie bracket of two vector fields $p(x)$ and $q(x)$ on M as." (This refers to page 43.))
10. *On page* 43, *an example is given of a set of three vector fields which is said to form a Lie algebra.* And, on p. 44, two vector fields are said to "form an involutive Lie algebra." A Lie algebra of vector fields is, of course, in particular a real vector space, and so it cannot consist of two or three elements.
11. *On p.* 153, *we are told that the Lie derivative $L_h\phi$ of a function ϕ in the direction of a vector field h is a row vector-valued function.* The author's reasoning is as follows: he defines $L_h\phi = (\partial\phi/\partial x)h$, and says that, "since $\partial\phi/\partial x$ is a row vector, then $L_h\phi$ is a row vector-valued function." The truth is different: the Lie derivative is a scalar function. The author forgot to notice that the row vector $\partial\phi/\partial x$ is multiplied by the column vector h, so that the result is a scalar.
12. *The real-analytic analogues of the theorems of Frobenius and Chow are wrong.* The author quotes Frobenius' theorem on p. 117, as the statement that, if an involutive collection of smooth vector fields is such that their linear span has constant dimension, then there are maximal integral manifolds through each point. He then goes on to say that in the real-analytic case the condition on the constancy of dimension is automatically satisfied, and so the theorem applies. Again, the truth is quite different: in the real-analytic case the dimension can certainly vary, as can be seen by trivial examples, such as the vector fields $[1, 0]$ and $[0, x]$ in the plane. However, *in the real-analytic case the integral manifolds exist anyhow*, a result proved by R. Hermann that has by now become one of the basic tools of nonlinear system theory. The author repeats his incorrect statement on page 118.

One could go on and on. And perhaps the reader may be wondering whether we have just been nitpicking. Maybe when the author talks about two or three vector fields forming a Lie algebra he *really* means to say that "the linear span is a Lie algebra." Maybe the erroneous definitions of tangent vector, vector field etc. are just typographical errors. In some cases this may be so, but there are many cases where a typographical explanation of the error is out of the question. (What could the author really have meant by his definition of "module," or by the statement that the Lie derivative is a row vector, or by his discussion of systems on Lie groups?) And the number of such errors is so large that the typographical explanation, even if it is plausible for some of them in isolation, ceases to make sense for all of them put together. The examples given above are only a very small, partial list, which the reviewer compiled by opening the book at random at various places. To test the validity of the conclusions reached from the first pages chosen, the reviewer selected—also randomly—pages in other parts of the book, and in every case numerous examples like the ones quoted above were found. They clearly add up to a pattern. Unfortunately, a review of a book like this one cannot list all the errors, with an explanation, for that would make the review much longer than the book itself. The reader who is still in doubt is urged to look at the book and see that our examples are representative of what is found in every paragraph.

Most of the proofs and many of the problems are also wrong or unintelligible. For instance, we are asked in Problem 2, p. 62 to prove that, if A is a square matrix, then the matrices X such that $AX+XA'=0$ form a Lie algebra. (For a trivial counterexample, let A be a 4 by 4 diagonal matrix with diagonal entries $1, 1, -1, -1$.) Four problems later, the author asks us to prove that the set of *all* fractional linear transformations $x \to (ax+b)/(cx+d)$ (with *no* condition on a, b, c, d) forms a group.

And even those problems that are right are useless. Let us say that something really is a module. How can a reader be expected to be able to *prove* that, using the definition of module given in the text and quoted by us above (Problem 3, p. 62)? And how is the reader going to attack the problem of showing that the k-jets of diffeomorphisms that leave a point fixed form a group (Problem 10, p. 64)? And, to do a problem that deals with the Lie algebras G_2, Sp(8) and Sp(16) (even though the reader is never told what those algebras are), is the reader supposed to derive inspiration from the author's virtuoso treatment of Lie groups (Problem 14, p. 144)? The list of problems appears to have been compiled by selecting more or less at random results from books and research papers in various areas of mathematics that have some remote connection with the subject matter of this book. The author merely inserted the words "prove that." (Take, for instance, the problems on nonlinear filtering. There are four of them, namely, Problems 17–20 on pp. 185–186. Each one asks the reader to "prove" something. In three of the four, the result to be "proved" is a deep, difficult theorem that has appeared recently in the research literature. Problem 17 is a result of V. Benes on the existence of finite-dimensional filters for some nonlinear problems that are "gauge equivalent" to linear ones, and Problems 18 and 20 are results on nonexistence of finite-dimensional filters for the cubic sensor problem and another filtering problem. The result of Problem 18 requires a deep algebraic fact, namely, the nonexistence of nontrivial homomorphisms from the Lie algebra of differential operators with polynomial coefficients to any Lie algebra of vector fields on a finite-dimensional manifold. The reader is asked to "prove" this as part (a) of the problem.)

But by now it should be clear that all this is irrelevant. This book is not on or about mathematics or mathematical techniques. It belongs to a different category altogether, namely, that of Science as Incantation (SAI). An SAI book basically says something like "gee, scientists are doing great things with impressive names, and they are likely to enable us to manage the economy, and understand the origin of the universe and the universal togetherness of life!" Then follow the invocations of entropy, quantum physics, Zen Buddhism, the interrelatedness of everything, the whole as more than the sum of the parts, everything as a system, perhaps with some chaotic or catastrophic behavior thrown in, and it is all so scientific! In those books, *words*, taken from science, are used; but they do not mean anything. We would not object to Shirley MacLaine's *Out on a Limb* on the grounds that, when the author talks about "cosmic forces," or "energy that functions on an invisible and undetectable dimension," she does not quite mean the same things by "force," "energy" and "dimension" as physicists do. In this book the words do not mean much either. And we are not *really* expected to sit and read and try to understand the definitions and proofs and do the problems. How could we? We are only supposed to marvel at the words, at all those goups, rings, ideals, modules, commutative diagrams, differentiable manifolds, unimodular groups, orthogonal groups, vector fields, Lie brackets, Lie algebras, algebraic varieties, Zariski open and closed sets, rational mappings, submersions, Grassmanians, automorphic functions, center manifolds, singularity theory, unfoldings, catastrophes, germs, jets, codimension, determinacy, Lyapunov functions, hyperbolic equilibria, bifurcations, limit cycles and many more beauteous things from the brave new world of science. The reader is presented with a collection of scattered items labelled "definitions," "theorems," "proofs," "examples" and "problems," which presumably are supposed to provide a glimpse of the universe of nonlinear systems. However, nothing in this book will convey any indication that underlying these items there may be a logically coherent structure, in which words have precise meanings and statements can be proved. At best, the book might perhaps induce a sense of wonder in readers who find big words fascinating.

But, if this is what the book sets out to accomplish, I do not believe it will succeed. *Out on a Limb* at least has an entertaining story to tell, and presents a picture of the universe which is almost certainly false but has an undeniable appeal. The best this book has to offer is the message, stated on p. 1, that "a rolling ship, a national economy and a supersonic aircraft" have something in common, namely, "they are all inherently nonlinear processes which are treated as linear phenomena until catastrophic events force managers and analysts alike to explicitly acknowledge the effect of the nonlinear structure on the system behavior." And, in case you expect to be told that now, thanks to system theory, we finally know how to manage an economy, all you will get is the much less reassuring message that we can now have "some degree of confidence in approaching several classes of nonlinear problems." This is illustrated by five examples, labelled "the nonlinear pendulum," "urban population migration," "neutron transport in a rod," "predator-prey relations" and "missile warfare." But the reader should not expect that these labels correspond to the real phenomena with the same name, any more than the author's "modules" or "Lie groups" correspond to the mathematical objects so named. For instance, the "urban migration model" is a simple system of two differential equations, which purports to describe the evolution of the number of white and blacks in a neighborhood, given that (a) whites tend to migrate away at a rate μ in order to find a better place to live, and (b) added to this there is also "bigotry migration." It is assumed that as whites move away blacks tend to occupy their place. Not surprisingly, we find out that asymptotically the neighborhood becomes all black, and if you want to change that you have to find a way to make μ negative. Then the author states an "interesting question" about this "model," namely, whether it is possible to determine the total numbers of whites and blacks from the sole knowledge of the percentage of whites in the whole population. Since the model is nonlinear, it is not surprising that the answer might be yes. Why this question might be interesting, I simply do not know. The author says that "this is a problem of nonlinear observability which will be considered in Chapter 5." Not uncharacteristically, the author does *not* consider this problem in Chapter 5, leaving the reader forever in doubt as to whether one can indeed determine the number of whites and blacks. But he does instead resume discussion of "urban migration" in Chapter Four. The power of system theory is illustrated by showing that, if the rate μ can be arbitrarily manipulated (including making it negative and arbitrarily large in size, and varying in time), then you can attain any prescribed white/black ratio. (This is called *c-o-n-t-r-o-l-l-a-b-i-l-i-t-y*. Wow!)

The other examples of applications are of similar depth. So the book is not likely to enlighten nonmathematicians who are trying to learn about techniques to handle nonlinear problems. Nor it is likely to be appreciated by mathematicians for its mathematical content. As a contributor to the area of mathematical systems theory for many years, I fear that the existence of this book might do considerable harm to the field, by giving some outsiders the false impression that the standards in our area are low. And I cannot help worrying about the nature of the refereeing process that makes a respected publisher accept a book in which almost every page is plagued with errors that would be spotted by anyone with a modest mathematical background. Perhaps they expected the book would appeal to the SAI audience. But SAI readers know better. If you are not worried about intellectual rigor, *Out on a Limb* is much more entertaining and has a vision. You may not learn much that is true from it, but at least you will have some fun.

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