Book reviews and abstracts

Book reviews

UNCERTAINTY AND INFORMATION: Foundations of Generalized Information Theory, by George J. Klir. Wiley-Interscience, Hoboken, NJ, 2006, xvii + 499 pages, ISBN 0-471-74867-6.

The ground state of information theory is communication theory, the characterization of channels and codes for compression, transmission, and error correction. A secondary, classical application is characterizing statistical inference problems. Historically, information theory has been thought of as a fellow traveler with statistical mechanics due to the similarity of the underlying mathematics. This has led to a tertiary and currently popular use of information theory to characterize the behavior of systems in both analysis and design applications. Examples of this range from Conant’s Extended Dependency Analysis and Klir’s Reconstructability Analysis and General Systems Problem Solver to Fraser’s work on mutual information on strange attractors, the development of Independent Component Analysis for blind source separation, the use of information maximization criteria in a host of artificial neural network applications and a variety of hermetic attempts to describe life as a statistical epiphenomena. It is the methodological development of this tertiary field that is addressed by George Klir’s new book Uncertainty and Information: Foundations of Generalized Information Theory.

More than just an update of earlier work, this new volume is a manifesto for Klir’s Generalized Information Theory (GIT) research program. Written as a textbook for a one semester graduate level class, this book conceptualizes the connections between non-additive measures, possibility theory, Dempster–Shafer evidence theory, imprecise probabilities, and fuzzy logic in the context of a grand morphology of uncertainty characterizations. The text itself is a well written mix of mathematics, historical commentary, and illustrative examples.

The guiding principle for the book is that we should use honest representations of the uncertainty we face as analysts, problem solvers, and designers. To this end we need a variety of theories that can capture the different senses or contexts in which we deal with uncertainty, without having to make extra assumptions or adopt arbitrary priors. This volume provides a basic toolbox to satisfy this need. Klir’s research program is based on a two dimensional morphological analysis of information theory based on classical probability. The additive measure and classical set theoretic language of probability theory are replaced with candidate monotone measures and non-standard sets in this program. This basic program first surfaced in the author’s Uncertainty Based
Information: Elements of Generalized Information Theory, but finds much greater expression in the current volume.

The two independent dimensions of generalization do not really get equal emphasis. The generalization of additive measures to sub and supra additive measures, Sugeno measures, and Choquet capacities constitute at least two thirds of the material. The material on fuzzification and non-standard logic feels somewhat out of place. The single chapter on fuzzification of theories of uncertainty is but a brief sketch of a much larger endeavor, deserving of level of treatment equal to that accorded generalizations of measures.

The first chapter offers a brief introductory discussion of uncertainty, discusses four levels of development for a theory of uncertainty, and then introduces the two dimensional morphological framework of GIT. The dimensions are choice of monotone measure and formal language. The remainder of the chapter reviews classical set theory and notation. The exposition then takes off with a review of the “classical” theories of possibility and probability in chapters two and three. These introductions are focused on the Hartley and Shannon measures of uncertainty and their generalizations from finite sets to bounded, continuous sets. These discussions set the stage for what to expect from a theory of uncertainty, i.e. how to calculate joint and marginal uncertainties, transmissions, etc. Both chapters use an axiomatic approach to motivate and justify the respective measures, and are peppered throughout with examples.

Chapter four introduces the classical additive measure from an axiomatic point of view, and then discusses monotone measures based on the relaxation of the additivity axiom. Choquet capacities are then introduced as a special family of monotone measures. Möbius representations are also introduced at this point. The rest of the chapter discusses imprecise probabilities, upper and lower probabilities, marginal, joint and conditional imprecise probabilities, and Choquet integrals for working with imprecise probabilities. Chapter five looks at several well developed uncertainty theories based on monotone measures including graded (non-crisp) possibility theory, Dempster–Shafer Evidence Theory, Sugeno λ–measures, and interval valued probabilities. This discussion of established theories takes place in the broader context of monotone measures.

Chapter six considers applying generalizations of Hartley-like and Shannon-like measures in a variety of settings drawn from the previous discussions, and includes recent work on aggregate measures of uncertainty and their decomposition. The two varieties of uncertainty, non-specificity and conflict are introduced and ten axioms that uncertainty measures should satisfy are laid out. Based on these axioms a variety of proposed measures are reviewed for graded possibilities, Dempster–Shafer theory, and convex set of probability distributions. The punch line to this endeavor is that Hartley-like measures can be found for nonspecificity in Dempster–Shafer theory but not for convex sets of probability distributions. Worse, axiomatically satisfactory measures of conflict cannot be found even for Dempster–Shafer theory. This leads to the introduction of aggregate uncertainty, which is justified axiomatically, and the suggestion that this aggregate measure may be decomposed to give generalized Hartley-like and generalized Shannon-like measures.

Chapter seven is an overview of Fuzzy sets, logic, and systems. Chapter eight is a cursory sketch of fuzzifications of uncertainty theories. There is a brief discussion of measures of “fuzziness”, and then a very worthwhile section on the fuzzy-set interpretation of possibility theory. The last two sections of the chapter look at probabilities of fuzzy events, and the fuzzification of interval valued probability distributions.
Chapter nine is focused on four methodological principles for working with uncertainties. Of these, three are well known holdovers from *Uncertainty Based Information*: the Principle of Minimum Uncertainty, the Principle of Maximum Uncertainty, and the Principle of Uncertainty Invariance. The new principle is the Principle of Requisite Generalization, which is essentially the premise upon which the whole Generalized Information Theory research program is based. The final chapter contains a review of the current development of GIT and the author’s personal perspective on future research. The appendices contain the proofs of the central theorems covered in the text, as well as a helpful glossary of terms. The bibliography is quite extensive, and together with the chapter end notes provides a thorough road map to the research field.

While suitable as a course text, the great contribution of this volume is its high level perspective on how to handle uncertainty in various contexts. The notion that one should look outside of probability theory when confronted with incomplete, imprecise, or independent datasets instead of pounding the analysis into “classical” form via censoring, resampling, and the application of the maximum entropy principle should not seem radical—yet it contravenes standard scientific practice in most quantitative fields. GIT is not yet sufficiently developed to readily answer all those who insist that the world may be made flat with the hammer of Bayesian Probability Theory. But it is an internally consistent mathematical theory and as such can not be dismissed anymore than previous dubious notions such as extension fields, associative algebras, and Boltzmann’s mechanics. Given this state of development, *Uncertainty and Information* will be of greatest interest to those with a mathematician’s temperament. I would recommend it for anyone interested in soft computing techniques, fuzzy systems, non-standard logic, and abstract discrete mathematics.

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**TWO MINDS: Intuition and Analysis in the History of Economic Thought**, by Roger Frantz. Springer, New York, 2005, x + 178 pages, ISBN 0-387-24069-1.

The following is a quote from the back cover of this book: “This book provides a rich complement and alternative perspective to some of the theoretical and mathematical models that have dominated the dismal science since the late 1940s”. This is not the first time I came across this characterization of economics as “the dismal science”. It seems to be based on the fundamental dilemma of economics. While the mainstream economics has produced a sequence of impressive axiomatic systems that have increasingly been more precise and mathematically sophisticated, predictions from these ever more sophisticated systems have persistently been at odds with economic reality.

In some sense, this book is devoted to a thorough discussion of this dilemma. In the opinion of the author, Roger Frantz, economics needs two ways of thinking, reasoning and processing of information: (i) qualitative or intuitive way; and (ii) quantitative or analytic way. He refers to these two ways, which are complementary, as two minds, and connects them with the right and left hemispheres of the brain. He argues that intuition has played an important role in the work of most great economists, but it has been taken out of mainstream
economics, or, as the author expresses it more eloquently, it “went ‘underground’ as economics became more quantitative, especially after WWII”.

After discussing principal characteristics of intuition and its role in science, mathematics, philosophy, and various other areas of human affairs, the author focuses on documenting how intuition was viewed and employed by some key figures in the history of economics: Adam Smith, John Stuart Mill, Alfred Marshall, John Maynard Keynes, Frank Knight, Herbert Simon, Harvey Leibenstein, George Shackle, and Frederick von Hayek. The book concludes with a chapter entitled “Intuition in Current Economic Literature”, in which the author argues that intuition has lately made a comeback in economics. It is apparently based on his extensive review of current articles in economic journals. Views of a few additional economists, most notably Milton Friedman, are discussed in this chapter.

One economist whom I expected to have a prominent place in this book, but who is unfortunately completely ignored, is Kenneth Boulding. Although he was certainly not a mainstream economist, he was an important advocate of the role of intuition and common sense in economics. In addition, he argued that good economics should not operate as a closed system within narrow boundaries, in which only “pure” economic phenomena are considered, but should rather operate as an open system in which social, political, ecological, legal and other relevant phenomena are considered as well (Boulding 1968). Similar arguments were expressed by George Shackle and are well captured by the following quote (Shackle, 1955, pp. 228–229):

> Economics is a field of study enclosed within arbitrary boundaries. These boundaries are not part of the nature of things. No doubt after 175 years we can say that they correspond to a consensus of powerful minds. Nevertheless, these boundaries are artificial. Is there any natural discontinuity between a man’s economic motives and conduct on the one hand and, on the other, his conduct as politician, scientist, philosopher, lover, or mystic?…Economics is not a natural science, but a human science. Why then does the economist say that psychology in none of his business? Do we say that economics is concerned with men’s preferences, choices, hopes, fears, beliefs, decisions, imaginings, motives, behaviour, and yet say that the inner truths about the working of their minds are none of our business?

Although George Shackle is covered in Two Minds, the space allocated to discussing his work is not, in my opinion, proportional to the relevance and significance of his work on the issues discussed in the book. One of his greatest contributions to economics is perhaps his recognition that the proper way of formalizing uncertainty in economics is not via the notion of probability, but rather via the notion of possibility. In the 1940’s, he developed, completely on his own, a coherent formal theory for this purpose (Shackle 1949). It is now well established (Klir 2002) that this theory is equivalent to the theory of graded possibilities that emerged in the literature some thirty years later (Zadeh 1978). Moreover, Shackle’s ideas regarding uncertainty in economics can now be further developed via recent results in the area of generalized information theory (Klir 2006).

Although Two Minds can be greatly expanded, at least in the two directions suggested, it is a wonderful book in its current form, and I wholeheartedly recommend it to all readers of this journal who are interested, at least to some degree, in economics.

References

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G.L.S. Shackle, Uncertainty in Economics and Other Reflections, Cambridge, UK: Cambridge University Press, 1955.
DISCOVERING KNOWLEDGE IN DATA: An Introduction to Data Mining, by Daniel T. Larose, Wiley-Interscience, Hoboken, NJ, 2005, 222 pages, 11 chapters, companion website, ISBN 0-471-66657-2.

Data mining is commonly defined as the automated discovery of novel, interesting, non-obvious information from large databases. Data mining originated as an outflow of expert systems, where the idea is to let the data rather than the experts speak. The domain expert still has a crucial role in data mining: (i) to provide and explain data; (ii) as an interactive participant during the data mining cycle; and (iii) as customer for evaluating rule systems. The ultimate goal of data mining is to provide transparency to the user and valuable feedback to the domain expert, ideally in the form of simple and easy to understand rules. A data mining process can be considered successful when the expert gained valuable new insights. These insights can be obvious ones, but only obvious after the completion of the process.

Data mining is an interdisciplinary field that combines elements of statistics, pattern recognition, machine learning, database management, and specific domain knowledge. Data mining also involves variable and feature selection, data fusion, and rule formulation. Data mining is also a vast area with several often domain-oriented sub-disciplines such as text mining, web mining, bio-informatics, (multivariate) time series analysis, database marketing. Common elements of a data mining process involve data preprocessing, data cleansing, outlier and novelty detection, data visualization, association rules, predictive modelling for regression and classification, unsupervised and semi-supervised learning, etc.

What sets data mining apart from statistics is that data mining is a process, and that by applying this process to a large complex set of data in a systematic way often entirely new domain insights can be gained. The precise goals of a data mining procedure are not necessarily completely defined up front. Data mining often deals with: (i) a vast amounts of real-world data; (ii) that can have faulty and missing data; and (iii) that can come from multiple data sources or databases. Data mining deals with large datasets where there are either a large number of features, a large number of data records, or both. A single data set can also be so large that it cannot be contained in the computer’s processing memory.

The book Discovering Knowledge in Data does an excellent job in introducing data mining to the novice as well as serving as a reference giving hints and pointers to the more seasoned practitioner. The book explains the most common views and practices of data mining and sets itself immediately apart from most other data mining books by emphasizing and reinforcing that data mining is a process that consists of: (i) a business understanding phase; (ii) a data understanding phase; (iii) a data preparation phase; (iv) a modeling phase; (v) an evaluation phase; and (vi) a deployment phase. The book contains several case studies.
illustrating this process and gives a good detailed enough explanation of classification, regression, clustering, association and outlier detection. The book also comes with a companion website providing further pointers and access to companion material. The strength of the book is the rigid integration of statistics and the emphasis on the use of several possible methods and metrics for evaluating regression and classification models. The book gives a practical description of different tools commonly used in data mining such as statistics (chapter 4), k-nearest neighbors (chapter 5), decision trees (chapter 6), neural networks (chapter 7), hierarchical and k-means clustering (chapter 8), Kohonen networks (chapter 9), association rules (chapter 10).

The last chapter in the book explains model evaluation techniques (chapter 11). While this chapter is very useful, and actually covers a topic that is too often left out in other data mining books, this chapter could be vastly expanded. A follow up book “Data Mining Methods and Models” that recently appeared is actually an ideal follow-up companion to this book.

Daniel T. Larose, the author of the book is a professor of Mathematical Sciences at Central Connecticut State University. The didactical experience of Professor Larose clear shows throughout this book and I most highly recommend this book for use as a text book in an introductory data mining course. Besides the companion website to the book is a useful link for the reader interested in data mining is the KDNuggets newsletter (www.kdnuggets.com).

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ADAPTIVE APPROXIMATION BASED CONTROL: Unifying Neural, Fuzzy and Traditional Adaptive Approximation Approaches, by Jay A. Farrell and Marios M. Polycarpou. Wiley-Interscience, Hoboken, NJ, 2006, xvi + 420 pages, ISBN 0-471-72788-1.

The book has been written as a text for a first-year graduate course on control theory offered in virtually any area of engineering. For this purpose, it is very well done. The material is presented in a rigorous way, but with superb clarity. Introduced concepts and ideas are consistently illustrated by well-chosen examples, and each chapter contains a section devoted to exercises and design problems. Most chapters contain also concise summaries of the covered material, conclusions, or suggestions for further reading from a list of 309 references. Readers are required to have a good background in mathematical analysis and linear algebra. Otherwise, the book is self-contained and suitable not only as a text for appropriate courses, but for an independent study as well.

The focus on adaptive approximation-based control and the attempt, in my opinion very successful, to present this subject in a unified way, makes this book quite different from other books on control theory that are currently on market. It is worth to describe briefly the content of the book.

The main purpose of Chapter 1 is to introduce basic ideas regarding adaptive approximation based control in the context of various other approaches to control. Chapter 2 is devoted to the examination of various general issues of function approximation that are
relevant for adaptive approximation applications. In particular, various properties of function approximators and their roles in designing adaptive approximation-based control systems are discussed. The aim of Chapter 3 is to introduce and compare, within a unified framework, several types of approximators that have been found suitable for adaptive approximation-based controllers. These types of approximators include polynomial functions, fuzzy systems, multilayer neural networks, splines, radial basis functions, and wavelets. Chapter 4 focuses on three principal issues regarding parameter estimation methods: (i) the formulation of parametrized classes of approximators; (ii) the design of online learning schemes; and (iii) the derivation of parameter estimation algorithms with desirable stability and robustness properties. Chapter 5 is an overview of various nonlinear control systems design methodologies. In the context of this overview, basic issues of design of control systems using adaptive approximation for compensating poorly modeled nonlinear effects are discussed in detail in Chapters 6 and 7. The book is concluded by presenting in Chapter 8 a detailed design and analysis of adaptive approximation-based controllers applied to fixed-wing aircraft. To make the book self-contained, a short tutorial on systems and stability concepts is presented as an appendix.

In summary, this is a well-conceived and well-written book. I am pleased to highly recommend it not only as a text for graduate courses on control theory in engineering, but also as an excellent book for self-study by practicing engineers.

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**HANDBOOK OF INDUSTRIAL AND SYSTEMS ENGINEERING**, edited by Adedeji B. Badiru, CRC Press–Taylor & Francis Group, Boca Raton, FL, 2006, 768 Pages, ISBN 0-8493-2719-9.

Having in the past pored through other handbooks aimed at capturing the essence of an entire discipline, I will admit to being skeptical when seeing the size of this volume. This handbook aims at a very lofty, yet necessary goal of being a reference for the field of Industrial and Systems Engineering; it attempts to do so in only 768 pages. While this handbook is by no means a quick read (and of course should not be), handbooks familiar to most Industrial Engineers eclipse this total by as many as 2000 + more pages (Handbook of Industrial Engineering: Technology and Operations Management, 3rd Edition (Salvendy 2001) is almost 2800 pages, and Maynard’s Industrial Engineering Handbook, 5th Edition (Zandin 2001) is over 2000 pages). While this reviewer has not reviewed the 1236 page Handbook of Systems Engineering and Management (Sage and Rouse 1999), needless to say, I was a bit skeptical in thinking that this comparatively small new reference book would be able to hit its mark. By one count, the Handbook of Industrial and Systems Engineering is trying to pack into 768 pages topics found in two handbooks totaling over 4000 pages. Since this reviewer would do it an injustice by judging this handbook solely by its cover (or rather, the distance between its covers), a deliberate review of all six major parts (Part I through Part VI, comprised of 36 chapters) of the handbook’s contents was undertaken.

It quickly became clear that the goal was not necessarily to provide a treatise on all things Industrial Engineering (IE) and Systems Engineering (SE), but to provide coverage, with
contributions from over 40 “rising stars”, of the systems engineering aspects of industrial engineering that some may argue are lacking in the aforementioned IE handbooks. The editor established eight goals for this handbook. These eight goals are now listed and comments are provided on each of them.

**Goal 1. To provide a one-stop reference for industrial and systems engineering**

While no handbook is able to provide treatments on every topic in an entire academic discipline, this reference does provide the reader a quick guide to the important topics—with no dearth of references—of industrial and systems engineering.

**Goal 2. To use a comprehensive yet concise format**

The editor certainly achieved his goal of having a concise format. This reviewer does feel that the overall treatment, however, is too concise. To be both comprehensive (which this is, in the sense that many topics are covered) and concise (which this most certainly is, refer to the page count discussion above) this is, in the parlance of our field, a multi-objective problem with conflicting objectives. As such, there is no solution that will optimise both objectives. This problem’s solution has been *satisficed* by sacrificing some of the depth of coverage of topics. This reviewer was left with wanting more in certain sections. To the credit of the contributors, whenever sections seemed to be too brief, important and recent references were at the ready.

**Goal 3. To include an up-to-date treatment of topics**

As earlier stated, any handbook of this type may suffer from omissions or, on the other hand, contain material that is “old hat”; this reference is successful in providing a current view of the material. This is facilitated because of the many up-and-coming contributors that bring a refreshing perspective to the topics in this volume.

**Goal 4. To introduce new technologies for industrial and systems engineering**

Several chapters are presented that introduce new technologies in industrial and systems engineering—from metaheuristics, to fuzzy set theory, and others. It was in this group of chapters (Part V of the handbook) that I was particularly left with the feeling of wanting more depth; I had to remind myself that the purpose of Part V was primarily to introduce new technologies/topics.

**Goal 5. To use the systems integration approach**

The contributors did a resounding job in keeping with a systems perspective in their treatments of the topics.

**Goal 6. To provide coverage of information engineering**

This goal was certainly achieved: information engineering is covered in one chapter. The chapter covering information engineering is certainly useful and describes how information
systems relate to industrial engineering in a general sense, but this reviewer is unsure why this topic was singled out as a major goal for the handbook.

**Goal 7. To offer a diversity of contributions from industry as well as academia**

There is certainly a diversity of contributors, but there seems to be one or two (out of the 40+) contributors that have non-academic affiliations. It does seem to be the case that many of the academic contributors do consult with industry on a regular basis and bring insight from these relationships into their treatments.

**Goal 8. To provide up-to-date material for teaching, research, and practice**

This reference will certainly assist the teacher, researcher, and practitioner. In fact in regards to a personal experience, our undergraduates (BS Industrial and Systems Engineering) recently defended their senior design projects. In their senior design course, they are charged with applying all of their undergraduate experience and coursework towards solving a very complicated, and intentionally ill-defined manufacturing systems problem. I am witness, as many academicians most likely are, to a large percentage of students, and in particular undergraduates, selling back their textbooks at the end of each semester. A handbook such as this would definitely have benefited our ISE students in their senior design projects, especially in the absence of many texts that he or she would have sold back over their academic career.

In summary, I would recommend this handbook because many of the editor's goals, that I generally agree with, are met. I would like to see certain treatments covered more in-depth; but that's the challenge of trying to have a “concise” handbook that intends to cover an entire discipline. While this is a first edition and I do recommend it, I would like to see the editor relax, somewhat, the goal of being concise in a future edition. My copy of Salvendy’s handbook will not be departing me any time soon.

**References**

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**EVOLUTIONARY COMPUTATION: Toward a New Philosophy of Machine Intelligence (Third Edition)**, by David B. Fogel. Wiley-Interscience/IEEE Press, Hoboken, NJ, 2006.

The fact that the third edition of this book appears just ten years after the publication of its first edition indicates the popularity of this classic book on evolutionary computation. This
popularity is well deserved. The book, written by one of the key contributors to the field of evolutionary computation, is truly excellent by all measures.

The basic structure of the book remains in the third edition basically the same as in the previous editions. However, some material has been revised, especially in Chapter 2, which is an overview of natural evolution, and some material has been added, especially in Chapter 5, in which features of intelligent behavior are examined. A notable addition to this chapter is a review of recent research on using evolutionary computation to allow a computer to teach itself to play checkers or chess. These revisions and additions highlight results emerging from recent research and are also reflected in expanded reference lists accompanying the individual chapters. A major addition, especially significant from the pedagogical point of view, are collections of questions or programming activities related to the material covered in each individual chapter.

Let me quote a short paragraph from Preface to the Third Edition, in which the author describes the unfortunate relationship between the area of computational intelligence and the area of traditional artificial intelligence:

Five years ago, I published the second edition of this book, and noted that evolutionary computation and artificial intelligence (AI) remained mostly disparate activities. Unfortunately, that is still true. Furthermore, other alternative approaches to machine intelligence, such as neural networks and fuzzy systems, still remains outside the mainstream of AI. Surely, these areas are well known, but not as well practiced within what constitutes “traditional” AI. In contrast, within engineering organizations such methods are being embraced. This is particularly true within the Institute of Electrical and Electronics Engineers (IEEE), the world’s largest professional organization. The IEEE recently approved a request by the IEEE Neural Networks Society to change its name to the IEEE Computational Intelligence Society, emphasizing the important contributions from fuzzy logic, evolutionary computation, and other branches of machine intelligence based on inspiration from nature.

Even though his specialty is evolutionary computation, Davis Fogel has always been a proponent of the broader field of computational intelligence. It can be clearly seen from this excellent book, in which evolutionary computation is not viewed in isolation, but in the broader context of computational intelligence.

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Oxford Dictionary of Scientific Quotations, edited by W.F. Bynum and Roy Porter. Oxford University Press, New York, 2005, 712 pages, ISBN 0-19-858409-1.

1. Introduction

Busy readers of our Journal of General Systems may well wish to have an occasional leisurely moment and peruse this long list of science-related quotations. Here is the Dictionary after 15 years of preparation (with occasional hiatuses of putting the manuscript “several years on the back burner”)—but it was definitely worth waiting for. I was informed several years ago, in response to my question as to the Dictionary’s progress, that it was still “in preparation”! My own interest stems from the fact that even as an undergraduate student in Canada I had began to collect science/technology-related quotations (Wolf 1970a,b,c).

Although the editors-in-chief had two assistant editors and the input of 11 editorial board members (2 American, 6 British and 1 Australian), as well as many “intellectuals”,
researchers, and academics from many countries supplying quotations and references, it
nevertheless is a tremendous job well done. Although the Information Age’s computer
technology may have helped, there was no doubt a lot of “hackwork” involved in creating
such a fine publication. Most are quotations in prose, but numerous rhymes/poems are also
welcome. Congratulations!

From personal experience I know how many headaches are suffered in attempts to find the
references—and get them right. What is a sad academic situation is the fact, borne out by
numerous examples, that the compilation of Dictionaries, Handbooks, Reference Works, and
other summary-type publications are not considered “real” research! Not for getting tenure,
that’s certain! Yet, every knowledge domain absolutely requires periodically these labor-
-intensive compilations, as clearly emphasized by Szostak (2004, see book review in 35(4) of this
journal). Does not each member of any fraternity occasionally need to be reminded that If we
have seen further it is by standing on the shoulders of Giants—referred to four times in the
Dictionary!

Aside from that, in particular complete comparative/contrastive overviews will tell us
which errors were made, where problems need to be tackled, and so forth. I would like to
hear/read any objections. Remember that for decades attempts have been made (often
unsuccessfully) to bridge the separation of The Two Cultures (see Snow’s pronounce-
ments)—this Dictionary is a contribution towards building a bridge. And perhaps one can
consider future cooperative developments based on the Philosophy of Synthesis and Analysis
of Quotations exemplified by the present Oxford University Press book?

2. Appeal

Most individuals are interested in what people have opined in speeches and publications.
Who is not engaged in occasional historical contemplation? Professionals spice their oral and
written communications with quotes—mostly to support their ideas, but often also in
counter-arguments to demonstrate that the opposition is wrong.

Who are the prospective-cum-potential readers? Well, certainly anyone involved in
science and several other disciplines with their margins overlapping science. Researchers
will find references to many important and extraordinary publications from which the wise
statements have been borrowed. Why, even teachers and students ought to consider using this
Dictionary as a source for inspiration in their oral and written communications. Certainly,
some professors could use the Dictionary as a prize for a student’s best essay, or as part of an
award for a job well done, or for completing a fine thesis/dissertation. A parent ought to think
of the Dictionary as a present for their son or daughter.

3. Coverage

The title of the book of course suggests correctly that mainly science-related quotations are
offered. However, we all know that science impinges just about all other disciplines, directly
or obliquely, implicitly or explicitly. Consequently, neighbouring disciplines will also find
occasional quotations of interest. The broader coverage is illustrated by the types of
profession or expert who have been quoted.

You will also find philosophers, mathematicians, engineers, meteorologists, astronauts,
 inventors, explorers, physicians, pathologists, surgeons, psychologists, museum directors,
essayists, authors/writers, educators, historians, moralists, social critics, poets, theologians, even a monk and prime minister and journalist, who have given their wisdom to posterity. Inasmuch as we owe much to the ancients (e.g. Plato, Aristotle, Archimedes, Hippocrates, Leonardo da Vinci, Copernicus, Luther, ...), even emperors-cum-dictators, alchemists, and mystics are quoted aside from the very early philosophers. At least one historically unsavory character’s (to be mildly euphemistically) deleterious uttering (fortunately only one!) has been quoted, namely by Hitler. One group of Anonymous quotations (some well-known ones) is also present. The sayings of the Bible (authorless) are not forgotten. Only very rare diagrams, tables, and formulae have been chosen—more, no doubt, would have been advantageous.

Many nationalities are represented among the wise quotees. Numerous quotes are translations. However, the attempt to collect in the first instance quotations is indeed an overwhelming job—and, consequently, has certain unavoidable limitations. The historical development of a specific country’s or culture’s science/technology is one factor that determines the richness or poverty of available publications from which one can extract wisdom. We cannot go into details in a short review about this phenomenon, let it suffice that overwhelmingly American, British, German, French, and a few other modern countries/cultures are presented.

I could find only eight Australians quoted—yet I think our contributions to science and related knowledge domains would have deserved a few more quotations. Just a minor thought—because the Dictionary indeed unequivocally proffers a good coverage. But in regard to the availability of quotations, there certainly must be an information overload or glut—and decisions must be made. Does the examination-of-the-elephant phenomenon apply here too (see figure and fable) when preferentially selecting from a vast literary warehouse?

The length of individual quotations and the number of them taken from one particular quotee’s source ranges from one-liners and one quotation to several pages and nearly 30 quotes. A skewed selection is definitely unavoidable; however, a few more quotes could have been provided, as for instance from the numerous publications by John Ziman. But there you are: “How long is a string?”

As to coverage: it may be important to realize, as explained in the Introduction, that because numerous books of quotations exist an attempt has been made to avoid duplication. Even several articles have been published in the past dealing with scientific quotations/sayings, exemplified by those by Mackay (1977, copy sent to me in 1988) and Wolf (1970). Consequently, anyone searching for quotations must peruse quite a few books and some articles.

The Keyword Index deals very thoroughly with the specific subject matters, concepts, or ideas in the “innumerable” quotations. Very well done! As to errors, I could only find one (not that it was my aim to hunt for them!): Wegener was neither a geophysicist nor a geologist (as another publication stated).

4. Last thoughts

I thought my favorite, all-discipline-transcending (including science, naturally), fable of The Blind Men and the Elephant accompanied by the sketch subtitled A group of blind men investigating an elephant would have been a valuable wise addition. See figure and poem.
in Wolf (1988, original source provided therein). A practical application of both this philosophy and scientific quotations has been exemplified in the article on mining/metal exploration by Woodall (1985).

I almost wished that the preparation of books of scientific and technological quotations would continue. I was fully convinced that quotations have a great appeal when I received over 345 reprint-request cards/letters during 1970/71 (matched only by one more-esoteric scientific publication)! Why not another volume in, say, 10 years’ time? A recent media discussion was concerned with what people do after retirement. Well, many “unemployed” intellectuals (using this as a generic term) could be involved in this venture-cum-adventure.

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Appendix

The Blind Men and the Elephant*

It was six men of Indostan
To learning much inclined,
Who went to see the elephant
(Though all of them were blind),
That each by observation
Might satisfy his mind.

The first approached the elephant,
And happening to fall
Against his broad and sturdy side,
At once began to bawl:
God bless me! But the elephant
Is very like a wall!!

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The second feeling of the tusk,  
Cried: Ho! What have we here  
So very round and smooth and sharp!  
To me tis mighty clear  
This wonder of an elephant  
It very like a spear!

The third approached the animal,  
And happening to take  
The squirming trunk within his hands,  
Thus boldly up and spake:  
I see, quoth he, the elephant  
Is very like a snake!

The fourth reached out his eager hand,  
And felt about the knee,  
What most this wondrous beast is like  
Tis clear enough the elephant  
Is very like a tree!

The fifth, who chanced to touch the ear  
Said: Even the blinded man  
Can tell what this resembles most,  
Deny the fact who can,  
This marvel of an elephant  
Is very like a fan.

The sixth no sooner had begun  
About the beast to grope,  
Than seizing on the swinging tail  
That fell within his scope,  
I see, quoth he, the elephant  
Is very like a rope!

And so these men of Indostan  
Disputed loud and long,  
Each in his own opinion  
Exceeding stiff and strong,  
Though each was partly in the right,  
And all were in the wrong.

Moral:  
So, oft in theologic wars  
The disputants, I ween  
Rail on in utter ignorance  
Of what each other mean  
And prate about an elephant  
Not one of them has seen.

*A famous Hindu fable by J.G. Saxe
CRYPTOGRAPHY, INFORMATION THEORY, AND ERROR-CORRECTION: A Handbook for the 21st century, by Aiden A. Bruen and Mario A. Forcinito. Wiley-Interscience, Hoboken, NJ, 2005, xxiii + 468 pages, ISBN 0-471-65317-9.

The book covers in a comprehensive way the three areas captured by its title—cryptography, information theory, and error correction. These areas are presented as a whole, emphasizing the inherent relationship among them. As the authors emphasize in Preface, they intended to write the book in such a way that it could be used at many different levels. They accomplished this intent admirably by employing the motto: “Never use a symbol if you can get away with a word”. As a result, mathematical prerequisites are rather modest: basic ideas of calculus, linear algebra, and probability and statistics, and some elementary algebra for certain topics of error-correcting codes (basic concepts of groups, finite fields, and modular arithmetic). An important feature of the book is that it contains over 300 worked examples and problems with given solutions.

The book is divided into three parts, each devoted to one of the three areas covered, but connections among the areas are utilized and stressed throughout the whole book. Contrary to most other books that deal with the same areas, a lot of attention is given to historical development of key ideas.

The first part of the book deals with cryptography, which is viewed as one of two parts of cryptology—“the science concerned with communication in secure and usually secret form”. Cryptography deals with principles of hiding information in communicated messages. The second part is cryptoanalysis, which is viewed as “the art of extracting the meaning of a cryptogram”. It deals with the issues of how to determine the hidden information. After describing the long and very interesting history of cryptography, the authors introduce the reader to basic principles of cryptography and, then, describe the many approaches to cryptography, increasingly more sophisticated, as well as the increasingly more sophisticated approaches to cryptoanalysis. They also describe the importance of cryptography in our society, often referred to as information society, and the various practical (not necessarily mathematical) problems associated with its use.

The second part of the book deals with the classical, probability-based, information theory. All basic ideas of classical information theory (on finite sets) are introduced and their relevance to cryptography is discussed. This part of the book can be viewed as a gentle introduction to properties of the Shannon entropy and its applicability to cryptography and coding.

The third part of the book is devoted to error-correction via appropriate codes. After introducing basic ideas of error detection and error correction, various codes with these capabilities are examined, together with the computational complexity involved. Some of the material in this part requires knowledge from some algebraic structures, but the writing is still so human-friendly that these requirements are minimal.

The subtitle of this book—“A Handbook for the 21st Century”—is quite appropriate. The book can be used as a valuable handbook for someone who works in the areas covered in the book. However, as is mentioned earlier, it is also valuable for multiple other uses. A few important topics are not discussed in the books, such as information hiding in images and generalized information theory that deals with imprecise probabilities. These omissions do not detract in any way from the quality and importance of this book. They are simply topics that are outside of its scope.

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If, as Ralph Waldo Emerson said, “a foolish consistency is the hobgoblin of little minds, adored by little statesmen and philosophers and divines” [Essays. First Series. Self-Reliance], what should be said about unrecognized inconsistencies in large system models that are responsible for the waste of millions or even billions of dollars, the loss of countless working days, and production delays of sorely needed products?

It is rare, essentially unique, that a high-level technical leader not only has made a unique contribution to a major theory related to consistency in modeling of systems, but also has had an opportunity both to observe the potential utility of that theory in a variety of large systems programs, and to teach the aggregated findings to students who work with him on the pedagogy of the material.

Yet this is what we find displayed in the book Constraint Theory by George J. Friedman. Developed in his doctoral work in the late 1960’s, tested as he rose through the ranks at Northrop Corporation in ensuing decades to become Corporate Vice President of Engineering and Technology, and finally, in retirement from Northrop, taught as an Adjunct Professor in systems engineering at the University of Southern California; this book reflects insights that he gained over more than three decades of attention to his pioneering development that he christened “constraint theory” [not to be confused with other work having somewhat similar names, but very different content].

This period of activity has not been completely free of frustration for George Friedman. High-level fiscal managers are seldom receptive to what they are likely to perceive as thorny—something that is seen as a nuisance to attempt to apply: theory that may demand significant, not-ready obtained cooperation and integration of work carried out among diverse groups or even diverse organizations; theory that is best served with software which may not even be available. Yet to the extent that this frustration has had a telling impact on George Friedman, it may have been a very positive factor in the development of his book.

The book has emerged partly from his mind as a written dialog between a technical manager and an analyst: something he is well-qualified to do since he has lived in both roles (though probably not with the managerial attitudes portrayed, at least concerning constraint theory). The potential conflicts surface. The potential benefits and contributions of constraint theory, and the negatives that are almost certain to accrue by ignoring it, are highlighted in this dialog.

In the outer world of engineering, this type of presentation may forearm both the analyst who wishes to influence the technical manager in the direction of applying constraint theory, and the marginally-receptive technical manager who must get a budget line item to support the application of constraint theory, and who needs to have enough insight to argue knowledgeably for that line item.

In the process of presenting constraint theory, George Friedman offers major contributions and several supporting contributions that are essential. The major contributions are the ones that motivated the development of constraint theory in the first place:

- **Model consistency analysis and inconsistency location.** To demonstrate how to determine, from the bipartite structure of a model, whether the model is consistent and, if
it is not, to determine from the structure of the model, where the inconsistency(ies) is (are) found; so that timely corrective measures can be taken, before large amounts of time and money (and ultimately, perhaps, even lives) are wasted.

- **Computation allowability analysis.** To determine whether a computation that is requested from the model is allowable (in order to avoid brute force attempts to squeeze a computed result out of a model that is beyond the capacity of the model to produce); once again so that timely corrective measures can be taken, before large amounts of time and money (and ultimately, perhaps, even lives) are wasted.

George Friedman recognizes that no matter how well constraint theory is formulated, in application it is subject to the foibles of initial human construction. Misuse of language, failure to define terms adequately, omission of important variables, built-in insensitivity to cognitive limitations—all of these (and other) behavioral factors have the potential to foil any analytical or synthetic system. So it is that the combination of sound management, sound analysis, sound theory, and the kind of excellent algorithms based in constraint theory can come together to enable effective systems engineering to occur. These recognitions lend importance to the key supporting contributions, which include the following:

- **Founder of discursivity.** Even before model consistency and computational allowability can be assessed, it is necessary to construct a discursive language in which the necessary concepts can be embedded, and the necessary conversations can be enabled. George Friedman has provided this discursivity, and a herculean task it has surely been to do so; requiring extensive and highly-correlated inter-definition, with the precision available only through carefully-crafted mathematical language.

- **Graphical interpretability.** With the extensive nature of the discursive algebraic language, a tremendous cognitive burden lurks. George Friedman has circumvented this burden significantly by tying the algebraic language to the graphical language through the isomorphism of the bipartite graph. (A. Kaplan, *The Conduct of Inquiry*, San Francisco: Chandler, 1964).

- **Computer implementation.** The ultimate relief from the cognitive burden of the constraint theory (which is inherent in the nature of the material, and not a consequence of its formation) lies in transferring as much of it as possible to computer software. This is not a task that George Friedman could have expected to accomplish in his employment. He has, however, started to program the main principles of constraint theory, and it remains for his successors, on the one hand to understand the importance of this task and, on the other hand, to take the necessary steps to get it done; not only for use in the system design community, but also in the higher education community.

It is clear that Friedman has a strong bent toward systems engineering, where most of his professional life has been focused, but he is not blind to other issues in society, as he makes clear in Chapter 1 of the book. Still if the engineering community cannot be awakened to the merits in this work, one wonders where the leadership is going to come from in other social arenas.
Modeling. While the title “Constraint Theory” is the most appropriate one for the book, it could also be described by a longer title: “how to avoid wasting huge amounts of time with defective mathematical models”. In order to illuminate this longer phrase, it is necessary, as mentioned earlier, to introduce some more detailed language than has been commonplace in the modeling community, and Friedman has contributed to such a language.

Earlier Kaplan (1964), in considering the general nature of inquiry, had distinguished two types of theory on the basis of internal primacy. He defined a field theory as one which takes as fundamental “a system of relations among certain elements, explaining the elements by reference to these relations”. He defined a monadic theory as one that gave primacy to the relations and illuminated the relations by reference to the attributes of what they relate. Friedman is not quite satisfied with this categorization of models. Instead he requires a triadic structure.

He prefers a three-level categorization, starting with protomodel in which a set of elements is recognized but the relations among the elements are too dimly noticed to be articulated; then a model, in which the detailed elements and relations are collectively and mathematically articulated and, finally, a metamodel in which those details are subsumed (though in a recoverable way, if necessary) in larger objects such as graphs or matrices upon which operations can be carried out and which have properties not observable by visual inspection of a model alone.

If one imagines Homo sapiens observing a complicated situation and, at first, sensing it only through a protomodel in which a few elements are given names; later, while aging rapidly into the computer age, developing a model in which those and other elements are related and feeling quite competent but being unable to determine the consistency or allowability of computations involving those elements and their relations; and finally constructing metamodels from the models using constraint theory, whereupon it becomes feasible to determine the consistency and allowability of computations; we have traveled with this creature who has passed through the various stages in the evolution of the theory and utility of modeling, using the language of George Friedman. And a worthy language it is, too.

The complexity threshold and “Friedman’s Conjecture”. One of the most troublesome aspects of systems engineering, and of the world of analysis and synthesis as a whole, is the virtually universal state of denial that persists concerning the prevalence of a complexity threshold beyond which conventional assumptions about how analysis, modeling, and synthesis no longer apply. Your reviewer has dealt with this extensively in his own publications, and will not dwell on the subject here. But I bring it out to emphasize a special contribution of George Friedman in this book which he calls “Friedman’s conjecture”. Appendix A discusses this topic in more detail than I will treat it here, but I mention it here because if the existence of a complexity threshold cannot be established in the organizational power structure, the future of constraint theory or any other powerful methods of working with complexity will always be shaky. Friedman’s conjecture, which he illustrates with a straightforward example is:

“that as the dimensionality, \( K \), of the model increases, the number of allowable computational requests also increases, perhaps as fast as the square of the model’s dimensions or \( K^2 \). However the number of possible computational requests increases far faster: \( 2^K \)”.

He goes on to illustrate that for a model of dimension 100, only \( 10^{26} \) of all possible computational requests will be allowable! Since models of thousands of dimensions have
been built and are planned, the ratio of allowable to possible computational requests is enormously worse than even this incredibly low number.

In effect what Friedman is showing is roughly that, if his conjecture is correct, the likelihood that one will be able to compute anything from a model of high dimensionality is nil, unless one has applied constraint theory to assure the consistency of the model, and to determine what computations are allowable.

[I was advised some years ago that no one strives to find solutions from the high-dimensional econometric models, but rather one assumes solutions based on economic beliefs, and then adjusts parameters to see what they would have to be yield the assumed solutions. Did someone say something about model strategy?]

In light of the fundamental role of the concept of dimension in constraint theory, one must note that the term retains a highly intuitive flavor colored by the long-standing electrical network concept—a flavor that is not readily transferred to fields outside of engineering. On page 78, Friedman says that his Theorem 14 “essentially tells us the dimensionality of the circuit vector space”. His second index reference to dimensionality occurs on page 129 where the term is used as “dimensionality limitations of the human mind”. There may be a significant conceptual gap between these two views of dimensionality which can benefit by further reflection and research—research that is not likely to affect constraint theory in any negative way, but which may go a long way toward circumscribing its connection to model-building as opposed to model analysis.

**Organization of the book.** After the helpful front material, which should be read to gain perspective, the book is organized into eight chapters, four appendices, a set of references, and an index. I will discuss now the specific content of these parts of the book, in the light of what has gone before in this review. In much of this discussion, I draw freely on the author’s preface, editing as I see fit, to keep the review to a reasonable length. With apologies to the author, I note that the reader can always refer to the book for the full description.

**Chapter One** provides an example of low dimension, showing how problems of consistency and computational allowability can arise in even simple situations. The reader is introduced to the two main characters of the book—an experienced manager and an analyst—whose dialogue will hopefully illuminate the book’s many concepts.

**Chapter Two** begins to establish [a] rigorous foundation. Four views are introduced: (1) set theoretic model, (2) sub-model family, (3) bipartite graph metamodel, and (4) constraint matrix metamodel. . . .rigorous definitions of consistency and computational allowability are made in the context of these views.

**Chapter Three** discusses the similarities between language and mathematics and provides some general consistency and computability results with respect to any class of relation. . . .three classes of exhaustive and mutually exclusive relations are defined: discrete, continuum, and interval.

**Chapter Four** emphasizes the constraint theoretic properties of regular relations, the most important type in the continuum class, and the most often used in developing multidimensional math models. The topological properties of the bipartite graph are analyzed to provide key conclusions of the model’s consistency and computational properties.

A specific type of subgraph within the bipartite graph, called the Basic Nodal Square (BNS) is identified as the “kernel of intrinsic constraint” and is accused of being the culprit in model inconsistency and unallowable computability. Trivially easy computations on the
bipartite graph—such as circuit rank and constraint potential—are shown to have enormous utility in locating the BNSs which hide in tangled circuit clusters. A constraint theory toolkit is provided to help you use the rules and theorems in an orderly manner. It can help locate BNSs trillions of times faster than brute force approaches. This chapter is the longest in the book and represents the core of constraint theory in its present stage.

Chapter Five emphasizes constraint properties of discrete and interval functions such as those from Boolean algebra, logic and inequalities. Interval relations require the greatest interaction between models and metamodels. The concept of constraint potential is less useful than for regular relations.

Chapter Six provides a compact structure of constraint theory. All postulates, definitions and theorems are listed and their logical interrelationships are displayed in the form of quasi-bipartite graphs.

Chapter Seven presents detailed examples of the application of constraint theory to areas of operations analysis, kinematics of free-fall weapon delivery systems and the dynamics of deflecting asteroids with mass drivers.

Chapter Eight summarizes the book and provides the manager and analyst a final opportunity to dialogue and discuss their common background.

Problems for the interested student are presented at the end of most chapters, so this book could be used as a text for a graduate course—or senior level undergraduate course—in systems engineering or mathematical modeling.

A list of references is provided, as well as an index.

Several appendices treat detailed material to a depth that would slow down the natural rhythm of the exposition if they were included in the chapters themselves.

Appendix A is noteworthy in that it summarizes the research projects on “computational request disappointments.” On models approximately the size of Chapter 1’s “simple example”—eight variables—the percentage of allowable computational requests based on the total number of possible computational requests is only on the order of 10%. [As mentioned earlier in this review] it is presently “Friedman’s conjecture” that as the dimensionality, \( K \), of the model increases, the number of allowable computational requests also increases, perhaps as fast as the square of the model’s dimension or \( K^2 \). However, the number of possible computational requests increases far faster: \( 2^K \). Thus, for a 100-dimensional model, only \( 10^{26} \) of all possible computational requests will be allowable! Models of thousands of dimensions have been built and are planned; so the ratio of allowable to possible computational requests is enormously worse that even this incredibly low number. The technologist who wishes to gain maximum benefit from asking his model to perform any computation his imagination conjures up will certainly be disappointed! A tool such as constraint theory which will lead him to the 10,000 computational requests (\( K = 100 \)) or 1000,000 requests (\( K = 1000 \)) which are allowable should be valuable.

Appendix B provides a very brief overview of graph theory with the objective of justifying why the bipartite graph was chosen as the primary metamodel for constraint theory.

Appendix C describes the rigorous logic of the difference between “if and only if” and “if” types of theorems. Most of the theorems of constraint theory are of the latter category—a source of confusion to many students.

Appendix D develops the similarity between vector spaces and graph theory circuits. The concept of independence in both worlds is strikingly similar and the ability to analyze circuits in graph theory has powerful ramifications in constraint theory because basic nodal...
Concluding remarks. There is ample evidence in the literature that three threads of research can be consulted to gain significant perspective on working effectively with complexity in modeling. One is microbiology, whose fruits remain to be harvested, and whose future is the subject of intense speculation. Another whose contributions are already in view is cognitive psychology and what has been learned about behavioral pathologies, as well as what has already been proved about means of overcoming or circumventing these limitations. The third is graph theory and the various ways in which the scanning powers of the human eye can be put to work in conjunction with the brain to develop pictures that are superior to what the ear can aggregate. There are at least three developments in applying graph theory that have demonstrated prowess in different ways. George Friedman’s constraint theory has offered some unique and highly valuable ways to gain insights into already-constructed mathematical models. The other two developments have to do with the development of models. One of these is the work of J. Hartmanis and R. E. Stearns (Algebraic Theory of Sequential Machines, Englewood Cliffs, NJ: Prentice Hall, 1966) which uses the lattice structure (another metamodel, in Friedman’s language) as the primary graphic device, through which they were able to solve a logic design component-minimization problem that had challenged designers for decades. The other is my work which uses the digraph structure (still another metamodel, in Friedman’s language) as the basis for structuring designs, and pointing the way to what kind of mathematical models or metamodels may be needed to analyze a design whose logic structure has been proposed.

These three developments are, at best, loosely coupled. It is my conjecture that a particularly worthwhile effort could be to link Friedman’s constraint theory with the lattice structure. I believe that most of the action in Friedman’s work lies at low levels in the lattice, and that a few of the concepts would be cognitively easier to bear if envisaged as lattice components; but this remains to be seen. Further effort in all of these domains cries out for the use of creative and flexible wall displays, electronically controlled, for the purpose of supporting human insight.

In a sense the developments in graph theory can be compared to drilling in adjacent, prospective gold-mining areas. Several hardy prospectors have uncovered bonanza-type veins that are open both laterally and at depth. Mining has already begun in these different areas, but the different mines have yet to share much infrastructure (only a few roads, and a few power sources) even though it appears that the various veins are likely to be connectible, and to lead to even richer lodes as the drilling continues. Ironically, it is only when sufficient detail concerning the individual veins has been developed to produce a metamodel that gives adequate insight into the economics of the underground structure that a basis for financing the mining venture proves suitable to the banking community!!

The 1847 De Morgan theory of relations slept for more than a century until far-sighted people like George Friedman and Frank Harary placed it in a practical context (F. Harary et al., Structural Models: An Introduction to the Theory of Directed Graphs, New York: John Wiley, 1965). Let us hope that others will pick up this torch and carry it forward—a challenge made easier to accept by the availability of George Friedman’s book.

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KNOWLEDGE-BASED CLUSTERING: From Data to Information Granules, by Witold Pedrycz. Wiley-Interscience, Hoboken, NJ, 2005, xvii + 316 pages, ISBN 0-471-46966-1.

This book is an important new contribution to the area of knowledge-representation and knowledge discovery via fuzzy systems by one of the key contributors to fuzzy set theory and fuzzy logic.

The fifteen chapters of the book can be roughly divided into three parts. The aim of the first part, consisting of Chapters 1–3, is to provide the reader with relevant background knowledge in three areas that are needed for understanding the principal subject of the book—knowledge-based clustering. These areas are: fuzzy clustering, granular computing, and neurocomputing.

The second part, consisting of Chapters 4–10, may be viewed as the core of the book. It covers in great detail the various aspects of knowledge-based clustering. The simplest way of describing the content of these seven chapters is to list their titles: Conditional Fuzzy Clustering; Clustering with Partial Supervision; Principles of Knowledge-Based Guidance in Fuzzy Clustering; Collaborative Clustering; Directional Clustering; Fuzzy Relational Clustering; and Fuzzy Clustering of Heterogeneous Patterns.

The third part of the book, covered in Chapters 11–15, is devoted to a description of several models for data mining based on the paradigm of knowledge-based clustering. They include hyperbox models, granular mappings, and linguistic models.

By and large, basic ideas as well as specific algorithms are presented in the book in their generic forms, not oriented to specific application areas. The presentation is characterized by a generous use of examples and illustrations, which makes it easy to read. In summary, this is a well-written and highly original book. It introduces fundamentals of an important new area, in which knowledge-based clustering is connected with granular computing.

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Abstracts

NONLINEARITY, CHAOS & COMPLEXITY: The Dynamics of Natural and Social Sciences, by Cristoforo S. Bertuglia and Franco Vaio. Oxford University Press, New York, 2005, xi + 387 pages, ISBN 0-19-856791-X.

The book covers a broad range of topics regarding systems. It is divided into three parts. The first part focuses on mathematical modeling as an instrument of investigation in natural and social sciences. Discussed are both linear and nonlinear models. The second part deals with the emergence of chaos in nonlinear systems. The third part is devoted to a discussion of the many facets of systems complexity.

CITIES AND COMPLEXITY: Understanding Cities with Cellular Automata, Agent-Based Models, and Fractals, by Michael Batty. MIT Press, Cambridge, MA, 2006, 648 pages, ISBN 0-262-02583.

As urban planning moves from a centralized, top-down approach to a decentralized, bottom-up perspective, our conception of urban systems is changing. This book offers a comprehensive
view of urban dynamics in the context of complexity theory, presenting models that
demonstrate how complexity theory can embrace a myriad of processes and elements that
combine into organic wholes. It is argued that bottom-up processes—in which the outcomes
are always uncertain—can combine with new forms of geometry associated with fractal
patterns and chaotic dynamics to provide theories that are applicable to highly complex
systems such as cities.

MODERN INFORMATION PROCESSING: From Theory to Applications, edited by
Bernadette Bouchon-Meunier, Giulianella Colletti and Ronald R. Yager. Elsevier, Amsterdam,
2006, x + 468 Pages, ISBN 0-444-52075-9.

The book is collection of papers that were selected from those presented at the well-known
IPMU (Information Processing and Management of Uncertainty in Knowledge-Based
Systems) Conference, which in 2004 was held in Perugia, Italy. It deals with broad range of
problems involving uncertainty and uncertainty-based information, which are divided into
six sections: (1) uncertainty theories; (2) preferences; (3) classification and data mining; (4)
aggregation and multi-criteria decision making; (5) knowledge representation; and (6)
applied domains

INTRODUCTION TO NONPARAMETRIC REGRESSION, by Kunio Takezawa.
Wiley-Interscience, Hoboken, NJ, 2006, xviii + 538 pages, ISBN 0-471-74583-9.

Using a learning-by-doing approach, the book is a comprehensive introduction to
nonparametric regression. Its principal features are: (i) all methods are thoroughly explained
to help readers intuitively grasp the value of nonparametric regression; (ii) each method is
illustrated by clear numerical examples; and (iii) mathematical equations are accompanied
by clear explanations of how they were derived.

FEEDBACK CONTROL OF COMPUTING SYSTEMS, by Joseph L Hellerstein, Yixin
Diao, Sujay Parekh and Dawn M Tilbury. Wiley-Interscience, Hoboken, NJ, 2004, xx + 429
pages, ISBN 0-471-26637-X.

The book is intended to introduce systems scientists and engineers to basic ideas of systems
modelling and feedback control and show how they can be employed in designing and
analyzing complex computing systems.

MULTIVARIABLE FEEDBACK CONTROL: Analysis and Design (Second Edition),
by Sigurd Skogestad and Ian Postlethwaite. John Wiley, Chichester, UK, 2005, xiv + 574
pages, ISBN 0-470-01168-8.

The book is a rigorous introduction to the analysis and design of robust multivariable control
systems. It is written as a text advanced undergraduate and graduate courses on multivariable
control, but it is also suitable for practicing engineers since its emphasis is on practical
applications.
**SYSTEM ANALYSIS, DESIGN, AND DEVELOPMENT: Concepts, Principles, and Practices**, by Charles S. Wasson. Wiley-Interscience, Hoboken, NJ, 2006, xii + 818 pages, ISBN 0-471-39333-9.

The book presents systems concepts, principles, and practices that can be applied to small, medium, and large organizations in business sectors such as medicine, transportation, construction, finance, education, government, defense, and utilities. Equipped with this knowledge, developers are better prepared to minimize the pitfalls of shortcut approaches that attempt a quantum leap to a single-point solution, which more often leads to technical, cost, and schedule performance problems.

**ERROR CORRECTION CODING: Mathematical Methods and Algorithms**, by Todd K. Moon. Wiley-Interscience, Hoboken, NJ, 2005, xiv + 756 pages, ISBN 0-471-64800-0.

The book provides a comprehensive introduction to both theoretical and practical aspects of error correction coding. The presentation is suitable for a wide spectrum of readers, including graduate students in mathematics, systems and information sciences, electrical and computer engineering, and computer science. The book contains many examples and exercises.

**STATISTICAL AND INDUCTIVE INFERENCE BY MINIMUM MESSAGE LENGTH**, by Christopher S. Wallace. Springer, New York, 2005, xv + 429 pages, ISBN 0-387-23795-X.

The principle of Minimum Message Length (MML) is an information-theoretic approach to induction, hypothesis testing, model selection, and statistical inference. MML, which provides a formal specification for the implementation of Occam’s Razor, asserts that the “best” explanation of observed data is the shortest. Further, an explanation is acceptable (i.e. induction is justified) only if the explanation is shorter than the original data. The book gives a sound introduction to the MML principle and its applications, provides theoretical arguments for its adoption, and shows the development of certain approximations that assist its practical applicability. The principle appears also to provide both a normative and a descriptive basis for inductive reasoning in general, and for scientific induction in particular.