An Annotated Bibliography of the Writings of Richard L. Epstein

My good friend and colleague Fred Kroon asked me to write a summary of my work, an overview of what I have done over the many years we've known each other and worked together.

So I set out descriptions of my works as I would to a friend or student, telling not only what is in each but also the genesis and background. It's only because a few friends and colleagues have said they liked such descriptions and have not found them too much the ramblings of a garrulous old man that I have decided to write this.

So many people have helped me, have taught me, have given me direction in my studies and research. Here I mention only those I recall as having helped in a substantial way the development of some particular part of my work, though I am sure I have missed some. But in all my books I have an acknowledgements section at the beginning in which I thank the people who helped me, and at the beginning of each of my published papers I have tried to note who influenced the work.

June, 2025

An account of my views and methods for all but the most technical of my work in recursive function theory can be found in the chapter "Concluding Philosophical Remarks" of *Propositional Logics* (annotated on p. 15 here), "Truth and Reasoning" (annotated on p. 47 here), and "*Postscript*: Logic as the Art of Reasoning Well" (annotated on p. 50 here).

Early years as an undergraduate

Anthropology

In my second year of the honors program at the University of Pennsylvania we had a course on anthropology taught by Igor Kopytoff. We read about strange people from around the world, tribes and all that. It didn't interest me much. For the final exam one question was to make some comment about anthropology. I said that it seemed to progress by someone making a theory or claims that were way too much, and then others corrected that going too far in the other direction, back and forth, but never leading to a middle ground. Kopytoff didn't like that. For the second semester we were to do a project, but I didn't want to work in the library. So Kopytoff arranged for me to visit regularly the Episcopalian church on Rittenhouse Square downtown in Philadelphia. It was definitely a foreign world to me, upper-class, Christian. I wrote up a report, I still have it, where I described the church as trying to continue without a congregation—an expensive area downtown, few people lived there. They were trying to attract young people. We had the choice with our project to take a safe B or try for an A+. I handed in my report and asked for the safe B. Later Kopytoff called me in—I was worried that it wasn't even good enough for a B. He said I would get an A+. It was the first write-up he'd seen of an how an institution copes with dying. He said that I should make it into a paper for a journal. I had no idea how to do that, so we agreed he would write it up. That was the end of spring, my second year at Penn. I would return at the beginning of the fall term to work with him for a few days before I went to London for the year. But when I returned he was away on a honeymoon, and I never saw him again. So nothing was ever done with it. But it convinced me that if anthropology was that easy, it wasn't for me. (I still have that report.)

Literature

That second year in the honors program there was a course on Elizabethan theater, taught by Peter Murray. It was great. I understood and learned a lot, in part because I had been a student actor in the Utah Shakespearean Festival the summer before. I wrote up a good term project. Then I asked Murray if I could do an independent study with him for the next semester. I had organized three other students to do a "reader's theater" with me where we did readings of plays and poetry in the campus coffee house. I said I would report to him on what we were reading and then do a term paper. He agreed. I did a show of readings of the poetry of Walt Whitman, acting as if I were Whitman. I read and studied Whitman's work and also essays about Whitman's poetry. Then I wrote a paper about how his work could be understood well only if spoken out loud, how so much of it depended on the rhythms and sounds of speech. Murray liked it and gave it to a Whitman scholar in the English department at Penn. That man read it and told me that it was a good start for a thesis. I told him I was just a second-year student. I thought that if it was this easy to do good work in English literature, it wasn't what I'd pursue. But reading Whitman shaped my attitude to poetry and life, and my writing style. (I still have that report.)

Recursive Function Theory

Review of "Degrees of unsolvability complementary between r.e. degrees" by S. B. Cooper

Journal of Symbolic Logic, vol. 40, no. 1, 1975, p. 86.

The first year that I was a graduate student in the Mathematics Department at the University of California, Berkeley I took the introductory graduate-level course on mathematical logic. It was taught by Julia Robinson. She was not a faculty member because of the anti-nepotism rules at the university: her husband, Raphael Robinson, was a professor in the department. This was despite her excellent work in recursive function theory and mathematical logic. She taught from Joseph Shoenfield's *Mathematical Logic*, which had just been published. The book and entire course was incomprehensible, not only to me but to all the first-year graduate students, except for a presentation she gave on recursive function theory. With a fellow graduate student in the course, Ion Filotti, I worked on a question that she posed about computable groups, he trying to show a positive result, I the negative. We worked for a couple months, and then he called, we met in the evening in an empty classroom in the department, and he showed his analysis that he thought would solve the problem but he couldn't see how to use it. I immediately saw how to get the result. He said he would write up our work. He went with his advisor for a semester to the Massachusetts Institute of Technology. When he returned he presented me with a paper he wrote with the proof—but only his name was on the paper. I was shocked. He wanted to claim full credit so he could use it for his Ph.D. thesis. I was so disgusted that I didn't object. Fortunately, it didn't turn me off from collaborating with others through the years.

Over the summer, preparing for my qualifying exams, I read *Theory of Recursive Functions and Effective Computability* by Hartley Rogers, Jr. With that as background I was assigned to be the graduate assistant for a course on recursive function theory given by Robert W. Robinson (no relation to Julia or Raphael Robinson). I convinced him to teach the course not with lectures, since he was going to use that book, but to spend class time going over the exercises. It worked great. I was to grade the exercises, which wasn't hard since we went over them in class. One time one of the students complained that he didn't get a gold star, which I pasted on the homework papers for getting all the exercises correct. It had fallen off—look you can see where it was! This class is where I met Ivan da Costa Marques , and we've been colleagues and friends now for more than 50 years, talking, encouraging, stimulating.

Robert W. Robinson and I became friends—he was only five years older than I. We often went out in my double kayak in the San Francisco Bay. I wanted to work on recursive function theory and asked Julia Robinson to be my thesis advisor. She demurred, saying that in all her years (she was elderly) she had never had a student and didn't feel right to start now. So I asked Robert W. Robinson to be my thesis advisor. He said no since he had too much other work as advisor for masters' degrees. I persisted, saying that I would just give him a thesis and he would only have to say it was O.K. He finally agreed, and we never talked much about work except that he gave me a paper that Stuart Barry Cooper had submitted

to a journal for refereeing. So I read it and learned about Cooper's method of full approximation constructions: it was very difficult, very complex. I gave a report to Robert W. Robinson, and he submitted it to the journal. Then I wrote this review.

The next year Cooper was a visitor to the mathematics department and I worked with him. Robert W. Robinson did not get tenure and left the department, distinguishing himself later with work on graph theory.

Minimal Degrees of Unsolvability and the Full Approximation Construction
Memoirs of the American Mathematical Society, no. 162, 1975.

From 1970 to 1973 I worked on recursive function theory as a Ph.D. student in the mathematics department of the University of California, Berkeley, enlisting Robert W. Robinson as my thesis supervisor and later with help from S. B. Cooper, who was visiting there. Both thought that it should be possible to use the full approximation method of constructing degrees of unsolvability below the degree of the halting problem (**0´**), which Cooper had devised, to show that the theory of the degrees of unsolvability less than **0´** is undecidable. For over a year, I worked on constructions of degrees that seemed to be needed to show that, but could not do the crucial construction. But I was able to show that a modification of a construction due to Robert W. Robinson could be combined with Cooper's full approximation construction to prove a theorem about joins in the degrees less than **0´**. For my Ph. D. thesis in mathematics I gave a textbook presentation of full approximation constructions, culminating in that new theorem, which I submitted in November, 1973. This monograph is an exact reproduction of that.

It was very difficult for me to write up that material for the thesis. What I wrote sounded stilted or forced. A fellow student in the mathematics department a year behind me , David Posner, was interested in Cooper's construction and wanted to use it in his work. But both Bob Robinson and Barry Cooper were no longer at Berkeley. So I began to write my thesis as if explaining it to Dave, stopping my writing at my desk and turning as if to talk to him before I set down more. Thus began my conversational style of writing, which I have used in all my work.

It was with this work that I first followed the genetic approach to presenting mathematics which I had learned at King's College, London, setting out what problem was meant to be resolved by the mathematics and showing how other more or less obvious routes would not work, building to the final solution. I had learned this approach from two teachers whose courses I attended at the University of London in the school year 1967–1968. Abraham Lue taught Galois theory by tracing the development of the mathematics as attempts to show or disprove that any angle could be trisected using only a straightedge and compass. H. Kestelman taught the theory of the real numbers and integration as attempts to solve problems of how to measure areas. It is what is called the *genetic method* of teaching (the genesis of the subject) and is what I have tried to do in all my subsequent writing.

It was because of what I learned in London that I was able to pass my preliminary exams (oral) for the Ph.D., receiving an A in algebra, a B+ in analysis, and a gift C in logic: I only knew recursive function theory and hadn't understood any

thing from Joseph Shoenfield's book, which was used to teach the course, and which I still think is incomprehensible, just a compendium of "results". I remember in the logic exam being asked to state the incompleteness theorem for arithmetic, so I quoted it by memory from Shoenfield's book; then Jack Silver, one of the examiners, said, "Look what they're calling the incompleteness theorem now."

Initial Segments of the Degrees Below **0´**

Memoirs of the American Mathematical Society, no. 241 (2), 1981.

Degrees of Unsolvability: Structure and Theory

Lecture Notes in Mathematics, 759, Springer-Verlag, 1979.

After four years of working irregularly on the problem of how to construct degrees of unsolvability that would lead to a proof that the theory of degrees of unsolvability less than that of the halting problem is undecidable, I was finally able to do it by relaxing the conditions in the full approximation construction I was using. That was in late 1977 when I was a post-doctoral fellow at Victoria University of Wellington, New Zealand. I then visited Robert W. Robinson who was in Australia at that time, and we went over the construction before I announced it in the *Bulletin of the American Mathematical Society*. The construction of those degrees constitute the first publication listed above.

At that same time I was working on a textbook presentation of the subject of degrees of unsolvability, which I finished when I was teaching at Iowa State University and published as the second book above.

To show that the construction in the first of these led to the undecidability result presented in the second book, I needed to use a result about the theory of degrees of unsolvability that Richard Shore at Cornell University had proved, or at least I had heard from colleagues that he had proved. I wrote to him for a description of that work, enough for me to use in the textbook. He demurred; recursive function theorists on the East Coast of the United States were intensely competitive and fearful that others would steal their work. So I had to reassure him and finally got the material; I did not include his proofs but only referred to the theorems. But their worry about others taking their material was not idle. I had been corresponding from New Zealand with Manuel Lerman on the East Coast about my work on constructions that would establish that the degrees below that of the halting problem were undecidable, time and again explaining to him why the methods he was using wouldn't work and the problems I was having. What a surprise when I returned from Australia to New Zealand to find that Lerman had announced in a journal that he had solved the problem, though he had not told me.

Before I published *Degrees of Unsolvability*, Piergiorgio Odifreddi from Italy was visiting at the University of Illinois and saw a draft. He asked to visit me at Iowa State University. He joked with me as if he knew me already. I was surprised, and he said that it was because he felt he knew me from reading the draft. Thus began many years of correspondence, discussions, and visits, which helped us in our work, though we never wrote anything together.

Diagonalization in Degree Constructions (with David Posner)
Journal of Symbolic Logic, 43, no. 2, 1978, pp. 280–283.

When visiting Berkeley while I was based in New Zealand, I found that David Posner had devised a way to show that some constructions in the theory of degrees of unsolvability could eliminate certain steps for diagonalizing. He showed it to me. I saw how it could be applied broadly, So he and I wrote this paper.

A Hierarchy of Sets and Degrees Below $\mathbf{0}'$
with Richard Kramer and Richard Haas, in *Logic Year 1979–1980*, eds. Lerman, Schmerl, and Soare, *Lecture Notes in Mathematics*, no. 859, Springer–Verlag, 1981, pp. 32–48.

A Mistaken Theorem on Degrees of *f*-r.e. Sets
Abstracts of the American Mathematical Society, 3, no. 1, 1982, p. 129.

I was a friend of Richard Haas when we were in the doctoral program in mathematics at the University of California, Berkeley. He had written notes towards a thesis, but then he was in a bad motorcycle accident that left him severely physically impaired and only barely able to do mathematics. I retained those notes and thought they were very good, yet I never found anyone else who developed similar ideas. When I was teaching in the Mathematics Department at Iowa State University I took them up with a student, Richard Kramer. He and I developed the ideas and methods. I then went back to Richard Haas, and he agreed that we would all three publish the work.

The second announcement is a correction to *Degrees of Unsolvability* on that topic from the first paper. The theorem was right; the proof was wrong.

Complementing Below Recursively Enumerable Degrees
with S. B. Cooper, *Annals of Pure and Applied Logic*, 34, no. 1, 1987, pp. 15–34.

The last summer I was in Iowa , 1982, Barry Cooper visited me . He had made a construction in degrees of unsolvability which he showed me. I was able to connect it to some of my work, and we proved some new theorems. He wanted to write it up. His write up, which is the published version, was incomprehensible to me.

Recursive Function Theory and Philosophy

Computability: Computable Functions, Logic, and the Foundations of Mathematics
with Walter Carnielli, Wadsworth & Brooks/Cole, 1989. 2nd edition, Wadsworth, 1999. 3rd edition, Advanced Reasoning Forum, 2008.

Computability & Undecidability: The story of the development of the theory of computable functions and the undecidability of arithmetic to 1970.
TImeline. Wadsworth, 1999.

Computabilidade: Funções Computáveis, Lógica, e os Fundamentos da Matemática
Walter Carnielli and Richard L. Epstein, Editora UNESP, 2005.

In 1977 in the second year of my post-doctoral fellowship at Victoria University of Wellington, New Zealand, I was asked to teach the undergraduate course on computability in the philosophy department. I was surprised because recursive function theory was pure mathematics to me. So I began reading. I found out about Alan Turing, Turing machines, Emil Post, Hilbert, Ackermann, and others. I began to see the development of the subject as an attempt to draw a line between the finite and the infinite. I made up a lot of notes, copying readings for the students, and I gave a genetic presentation of the subject in the course.

I had a notebook with the notes and articles and mathematics for the course, but I never did anything with it, though I thought it was good. Then when I was teaching mathematics at Berkeley in 1983 I met Walter Carnielli who was visiting there for the year on a post-doctoral fellowship from Brazil. We worked together, and I gave him the notebook on the computability course. He liked it a lot and asked me why I didn't publish it. I told him I was too busy working on my book on propositional logics, but if he wanted to take the project and start on making it into a book, I'd work with him. In 1987 I went to the University of Campinas in Brazil as a Fulbright Fellow to work with him. As part of our proposal we said we'd develop the computability notes into a book. I gave a course on the material, attended by a few students and faculty. Carnielli and I worked together on it. I remember we got to a place where I needed to present Gödel's incompleteness theorem, but I had no notes on that. So Walter and I got together and discussed how we would do it. Three days later I went to his home and showed him my write-up. (I had with me my Apple Macintosh, one of the first, with a mathematics font I had developed, so I could print it.) He was surprised and said to his wife excitedly, "Look what he's done in just three days!" To which I replied, "Yes, and ten years experience."

Back in the U.S. I found a good editor John Kimmel at a science publisher, Wadsworth & Brooks/Cole. I showed him a draft of the book, and he liked it but said we should change the title from "Computability: Computable Functions, Logic, and the Philosophy of Mathematics", as he thought mathematicians wouldn't buy a book with "Philosophy" in the title.

Then I read through it with Karl Henderscheid, who had been my student at California State Hayward. His questions and comments helped me improve the exposition, and he contributed the wonderful illustration of Achilles and the tortoise for the cover.

Walter and I published the book in 1989, printed from the camera-ready copy that I provided to Brooks/Cole. This was the first book I did that way, *Propositional Logics* being the second. For all my subsequent books I have done camera-ready copy. That helped the publishers because they didn't have to set it in type, and they gave me money for doing it. Moreover, making the final copy helped me because I didn't have to find mistakes in what the publisher did setting it in type. That was the first time anyone had used the word-processing program WriteNow to do camera-ready copy, at least in math or science, and I sent a copy to the company. The developers of that program were glad to see it. I have done camera-ready copy for all my subsequent books this way, always using the technical fonts I developed and the program WriteNow, which is extinct as Microsoft bought up the company so it could close down competition with Microsoft Word.

The book begins with readings on the liar paradox and Zeno's paradox of Achilles and the turtle, then turns to readings by Turing, Post, and Hilbert, before beginning the mathematical presentation. Some teachers told me that students couldn't get through all that in one semester. I told them that if the students knew what problems the theory was meant to resolve, they'd be able to understand the mathematics much more easily, seeing why the formal work was needed and how it was meant to resolve the issues—as I knew from teaching it. This led to including some mathematics that hadn't been presented in other textbooks, particularly transfinite induction as an attempt to make a complete collection of computable functions that couldn't be diagonalized out of.

As a final chapter we included readings on constructivist views of mathematics as a counterpoint to the platonist, abstract approach to mathematics. We included David Isles' "Remarks on the notion of standard non-isomorphic natural number series" which gives a serious challenge to the received views. I can't remember when I first encountered Isles' finitistic analyses of arithmetic, but over the years that followed until his recent death, David and I corresponded, read each other's work, and enjoyed each other's insights about our work, including *The BARK of DOG* (p. 59 below) We had a deep connection that affected much of what I wrote.

Walter and I also provided a full instructor's manual, explaining how to teach the material, with answers to the exercises, some of which were very hard.

In 1999 after I published *Critical Thinking* (annotated below, p. 27) with Wadsworth, my editor there, Peter Adams, agreed to publish a second edition of *Computability* with Wadsworth. Walter Carnielli and I made corrections, but the book remained essentially the same. In going over the history of the subject, trying to sort out who did what and when, I made up a timeline about computability and undecidability. With the encouragement of Peter Adams, I prepared it and we included it in the new edition. I also made a poster of that which included photographs of many of the people mentioned in it. It was meant to be distributed free to faculty and students with the purchase of the book.

Later, when I got the rights back to the book and began publishing as the Advanced Reasoning Forum, I prepared a third edition. The only difference was to include a final paper , "Mathematics as the Art of Abstraction" (annotated

below p. 36), which I had previously published. It presents a major challenge to the other readings and to the philosophy of mathematics: whatever foundations mathematics needed and used were in methods of reasoning applied to mathematics, not in resolving what mathematical objects exist.

Walter Carnielli wrote an adaptation of the book for Brazil in which he revised the material and translated all the articles in the book into Portuguese. That was published in 2005. It was a big job because so little of that kind of material was available in Portuguese and he had to develop a vocabulary, checking with other academics about his choices. I did almost nothing in preparing this—it was Walter's project. It won third place of the Jabuti prize in Brazil in the category "Ciências Exatas, Tecnologia e Informática" .

A Note on Countability

Think, vol. 17, no. 50, 2018, pp. 57–59.

Here is how the paper begins.

> My student Esperanza Buitrago-Díaz and I went to the airport to pick up Henrique Antunes Almeida for his first visit to my research institute, the Advanced Reasoning Forum. On the way home I mentioned that we have sheep there.
>
> > Henrique: How many sheep do you have?
> > Me: I don't know. Every time I try to count them I fall asleep.
> > . . . They're uncountable.
> > Esperanza: That's ridiculous. That's not uncountable. I can count them.

I then compare different standards for how to understand "countable" and conclude:

> Only two definitions of "countable" are clear, entirely precise, and require no supplementation: the fully subjective and the fully abstract. The former is clear but does not lead to a shareable standard. The latter is clear but removes the notion from our lives, except to the extent that we can actually construct an enumerating function.

Propositional Logics

Relatedness and Implication
Relatedness and Interpretability (with L. Szczerba)

Philosophical Studies, 36, no. 2, 1979, pp. 137—175, pp. 225—231.

From late 1975 to early 1978 I was a post-doctoral fellow in mathematics at Victoria University of Wellington, New Zealand. There was a good logic group there, including Max Cresswell, George Hughes, and Rob Goldblatt, that met weekly for a seminar or discussion. Attending those meetings I was first exposed to philosophy, but only as connected with logic and language.

In my second year there, Douglas Walton visited for an extended period. Here is what he and I wrote as a preface to this issue of *Philosophical Studies*

> The work in this issue was done almost entirely in May, June and July of 1976 with the logic seminar at Victoria University of Wellington, New Zealand. It is a group effort. Much of the credit is hard to assign; the material covered in the various papers by the authors reflects their interests and major contributions.
>
> What's been exciting about this project is the exchange of ideas between mathematicians and philosophers. Doug Walton gave a talk on action theory. Throughout, the nature of material implication kept him from expressing formally the ideas he had. Dick Epstein suggested thinking of relatedness as a binary notion. Several people in the group urged us to follow up on this. In the next weeks we tried out several ideas, and eventually went to Rob Goldblatt for help on the technical side of things. His important suggestions determined much of the development, and finally we were able to produce a completeness theorem. Success! We took it back to Doug Walton to see if it made sense: whether it was useful for the problems he was studying. We adjusted, we tinkered. Finally we presented it to a meeting of the seminar where a rich discussion ensued. This prompted David Lewis to present, the following week, his ideas about joining the 'subject matter' approach to ours. Much of the issue was shaped in those talks. The task of writing up the 'philosophical' sections was assigned to Doug Walton, and the 'mathematical' sections to Dick Epstein. The completeness proof section is the joint work of Dick Epstein and Rob Goldblatt.

The contents of the issue comprise the two papers above and two papers by Doug Walton: "Philosophical Basis of Relatedness Logic" and "Relatedness in Intensional Action Chains".

I called the paper "Relatedness and Implication" because I was under the influence of the people in the seminar who worked on modal logic and who did not distinguish between conditionals and implications. Now I know better. It's about relatedness and conditionals.

I wrote the second paper with Ladislaw Szczerba when he visited the logic group for an extended stay in 1977. It shows that you can formulate set theory and other mathematical theories as propositional logics by adding quantifiers over propositions to the language: the indices of the propositional variables act as variables.

Relatedness and Dependence in Propositional Logics, ed. Richard L. Epstein, Research Report of the Iowa State University Logic Group, 1981.

Most of the formal work that appeared in *Propositional Logics* (annotated below, p. 13) was presented in this when I was at Iowa State University. Though complete in itself, it was meant as a draft of a book, listing two papers in preparation.

It's possible that I have the only copy of it now. Roger Maddux can't find one; the Mathematics Department and the Library at Iowa State University can't find one; and Douglas Walton as well as Gary Iseminger, who is listed as having a paper in preparation, have passed.

- Relatedness and Dependence in Propositional Logics

This gives an outline of the unified presentation of propositional logics that later appeared in *Propositional Logics* (annotated below, p. 13).

- Dependence Logic

From *Propositional Logics*:

> I first presented dependence logic in a preprint in 1978. I was stimulated to do so by conversations I'd had with Niels Egmont Christensen the previous summer. We had been discussing relatedness logic, and I found that I could model his notion of entailment by changing the relationship governing the truth-table for the conditional from nonempty intersection to containment.

I called this new system "dependence logic", and more history about it can be found in *Propositional Logics*. This is the first full presentation of it.

I wrote to Max Cresswell in Wellington and asked him how to decide which was right: relatedness logic or dependence logic. He wrote back and asked me why one had to be right and the other wrong. That was the start of my project of viewing propositional logics as apt or inapt for capturing some notion of what we consider significant in our reasoning beyond truth-values of propositions. And that led, over many years, to my view of logic, reasoning, and language as how we cope with the world, not (necessarily) right or wrong, nor even judged by the standard of usefulness.

- The Algebraic Nature of Set Assignments with Roger Maddux

This paper has an outline of a remarkably complicated construction of set-assignment semantics given a dependence relation, that is, a relation that is reflexive, transitive, and D(A,B) iff D(A, p) for every p in B.

> Abstract:
>
> A set assignment is s: FM $\rightarrow$ Sb S [FM = formulas of language of propositional logic, Sb s = subsets of S]. We say that a binary relation *D* on FM arises from a set assignment s if *D*(A, B) iff s(A) * s(B) for some given set theoretic relation *. For example, * could be $\supseteq$ or non-empty intersection.
>
> In this paper we show that for various classes of set assignments characterized by first-order set conditions we can characterize by first-order conditions on *D* the class of relations arising from such set assignments when * is $\supseteq$ or $\subseteq$. The purpose of this is to facilitate completeness proofs for propositional logics as in the paper "Dependence Logic" above.

- S4 and Dependence Semantics with Roger Maddux

  Abstract:
  We present dependence-style semantics for the modal logic S4.

- Dependence Semantics for Modal Logics

  Abstract:
  In this paper we will set out [set-assignment] semantics for most of the well known modal logics which have possible world semantics.
  [S4, S5, S4Grz, T, B, K, K4, G, G*]

- Intuitionism and Dependence Semantics

  Abstract:
  In this paper we set out dependence semantics for Heyting's Intuitionistic Propositional Calculus, and for Johansson's weaker system of Minimal Calculus. In developing semantics for the former we note that a 3-valued version of the same naturally shows up.

- Dependence Semantics for Some Many-Valued Logics

  Abstract:
  In this paper we show that several many-valued logics can be given dependence semantics. In most cases these semantics appear to be a specialized form of algebraic semantics, using sets and $\subseteq$ rather than arbitrary elements and a $\leq$ operation: for any $\langle v,s\rangle$, {s(A) : A is a wff} creates the algebra and v picks out the set (usually S) which will correspond to the designated truth-value. [$G_{\aleph}$, G_3, K_3, $L_{\aleph}$, l_3]

- Dependence Semantics for Quantum Logics

  Abstract:
  We give dependence style semantics for Orthologic and Quantum Logic as presented by R. Goldblatt in "Semantic Analysis of Orthologic".

Also included were "Ignoratio Elenchi" and "Conditionals in Act Sequences" by Douglas Walton.

Relatedness and Dependence in Propositional Logics

Abstract of talk to the Association of Symbolic Logic, 1979 Spring Meeting, *Journal of Symbolic Logic*, 6, no. 1, 1981, p. 202.

In this twenty-minute talk I sketched how propositional logics could be viewed as a spectrum.

I thought it would be hailed as a major breakthrough. It wasn't.

A General Framework for Semantics for Propositional Logics

Text of invited address to the VII Latin American Symposium on Mathematical Logic. In *Methods and Applications of Mathematical Logic*, eds. W. Carnielli and L. P. de Alcantara, *Contemporary Mathematics*, no. 69, 1988, pp. 149–168.

Walter Carnielli arranged for me to be invited to give a keynote address to this conference in which I set out more fully the conception of how a choice of what to pay attention to in our reasoning leads to what propositional logic we will use.

This was, in outline, the conception and formalism of *Propositional Logics*, annotated directly below. Walter helped me a lot with it.

The talk was enthusiastically received. One person later told me that he was sure this had been done before and gave me a reference. I was anxious to see what it was. When I looked it up later, I found it had nothing really to do with what I was working on. This was the first of many times that I would present a new conception, an overview or way of conceiving of some subject, and people were sure it had been done before. I used to be frustrated because I wasn't being given credit for what I had done. Later I came to see this as a compliment: the conception seemed so natural that people couldn't imagine that it wasn't already part of our heritage.

The Algebra of Dependence Logic

Reports on Mathematical Logic, 21, 1987, 19–34.

In the mathematics department at Iowa State University Donald Pigozzi and Roger Maddux led a year-long seminar on the algebras of propositional logics which I attended. I began reading for my project of giving uniform semantics for propositional logics and began to understand the various logics better. I think I still have the notebook(s) I made from that. Under their influence I set out in this paper how to have algebras for dependence logic.

A Paraconsistent Many-Valued Logic: J_3

with Itala M. L. D'Ottaviano, *Reports on Mathematical Logic*, 22, 1988, pp. 89–103.

Itala D'Ottaviano and Newton da Costa had devised a 3-valued logic that was paraconsistent: not every proposition could be derived from a contradiction. When I was at the University of Campinas in Brazil as a Fulbright Fellow I worked with Itala D'Ottaviano to show that this logic could be given set-assignment semantics, which we presented in this paper. It occurred to me then that paraconsistent logics could be viewed as taking "true" rather than "false" as the default truth-value in the table for set-assignment semantics: if the relationship does not hold, the conditional is taken to be true. This paper is the first time that was suggested. This work is more fully presented in *Propositional Logics*, annotated next.

Propositional Logics

with the assistance and collaboration of Walter A. Carnielli, Itala M. L. D'Ottaviano, Stanislaw Krajewski, and Roger D. Maddux. Kluwer, 1990.

In this book I set out systems of propositional logic: classical logic, relatedness logics, dependence logics, modal logics, intuitionist logics, many-valued logics, and the paraconsistent logic J_3. Each is presented in the terms of their originators, with examples and discussion, followed by showing how each can be given relatedness or set-assignment semantics compatible with those motivations. In this way these logics comprise a spectrum: each arises by ascribing some semantic value other than truth-value to atomic propositions and then to compound

propositions. As we vary the semantic value, we vary the logic, all within a single framework for the semantics.

The history of the development of this work is given in the Acknowledgements section at its beginning. Here I'll point out the main contributions of the people listed as collaborators.

Walter Carnielli pointed out to me that I had neglected to give a mathematical overview of the system of semantics. Working with him we did that here.

The work with Itala D'Ottaviano discussed in the annotation to the previous entry above formed a chapter here.

Though only one section of the book (Chapter VI.F) is ascribed to Roger Maddux , many discussions I had with him in the years I was at Iowa State University were important for the development of the material, and he is mentioned in the book several times in regard to that. See the annotation to the collection *Relatedness and Dependence in Propositional Logics* (p. 11 above).

In previous work in logic, a translation was understood to be a syntactic mapping that preserved theorems. For each translation that was proposed, an *ad hoc* proof was given to show that the mapping preserved correctly theorems and non-theorems of the logic translated into the second logic. With this new general framework for semantics, and under the influence of Leszek Szczerba's work on interpretations of theories (presented in *Classical Mathematical Logic*, annotated below p. 23), Stanislaw Krajewski and I developed a general theory of translations between propositional logics which we presented in this volume. A translation between logics is taken to be a syntactic mapping from the formal language of the first into the formal language of the other along with a semantic mapping in the other direction which could establish that the mapping preserves consequences. This is what we called a "meaning-preserving" translation. (Preserving theorems is not enough, since that could be trivial without some constraint on the mapping, and even then would not guarantee that consequences would be preserved.) After I gave a talk on the general framework to the mathematics department at the University of Hawaii in the hopes of obtaining a position there, I ran into the chairman of that department when he was on a short visit to the Mathematics Department at Berkeley. I asked him about the position. He said that though the material was interesting, I hadn't proved anything, so they wouldn't consider me. "Prove? Prove?" I said. "I spent years trying to figure out the right questions." But with his comments as stimulus, I worked with Staszek to show that the intuitionists' claim that the theorem-preserving mapping of classical logic to intuitionist logic didn't make sense was apt: we showed that there could be no meaning-preserving translation of classical logic into intuitionist logic.

This was part of the view I had that formal syntactical systems without a semantic basis—axioms and methods of deriving consequences from them—were not worth our attention: see, here is the logic, endow it with any meaning you like. That is, if you can find any meaning that will fit. (That was particularly the case with paraconsistent logics which had been devised by varying the axioms of classical logic to avoid deriving any proposition whatever from a contradiction A-and-not-A.) Abstracting from our ordinary reasoning to develop a logic,

meaning is always a constraint, perhaps implicit. That's why I called this series *The Semantic Foundations of Logic*.

The final chapter, "Concluding Philosophical Remarks", was stimulated by conversations I had with students, visitors, and faculty in the philosophy department at Berkeley in the 1980s. In it I set out a view of logic and language that I followed in all my subsequent work:

> We can understand all logics in the same way. We start with our everyday language and abstract away certain aspects of linguistic units and take into account certain others by making idealizations of them. The aspects we pay attention to determine our notion of truth.
>
> There does not seem to be a difference between logical and pragmatic aspects of what we call propositions, or at least any difference we can justify. What direct access have we to the world but our uncertain perceptions? And how can two of us share exactly the same perceptions or thoughts? Vagueness seems essential to communication. So to call a sentence true seems at best a hypothesis we hope to share with others. This sharing, which in a sense amounts to objectivity, is brought about by common understandings, which I call agreements. But this notion of truth is so basic to our experience and the fit of thought to the world that we can no longer allow ourselves to see that truth is in how we abstract, perceive, and agree, lest we have no language to talk.

I gave a draft of the book to a philosopher who was visiting Berkeley for a year. He said he liked it a lot except that it sounded just like me talking. He, as so many others, thought that work in logic needed to be written in an impersonal, technical style. I took what he said as a compliment.

More summary or outline of the contents of this volume would be too much here, besides being too technical. There is much history of the project in the volume, and there are several reviews of the book (in *Mathematical Reviews*, by Charles Kielkopf, in *SIGART Bulletin*, vol. 2, no. 2 by Walt Truszkowski, in *Zentralblatt für Mathematik und ihre Grenzgebiete*, by S Gotwald, in *The Journal of Symbolic Logic*, vol. 56, no. 4 by Graeme Forbes, in *Choice*by R. Puglianda, and in *Journal of Logic and Computation* by Arnon Avron).

During my years at Iowa State and in Berkeley from 1978 to 1989 I worked on this book, which was published in 1989. Though most of it was done by 1987, I couldn't find a publisher. Twelve academic publishers rejected it on the basis that it was really philosophy and they published mathematics, or that it was really mathematics and they published philosophy. It got to the point that I followed the advice someone gave me that as soon as I sent the book to a publisher I prepared a submission to be sent to the next publisher in case it was rejected to avoid abandoning the project in despair. Finally I submitted it to Stan Surma, whom I'd met when he was visiting Berkeley, when I found that he was on the editorial board of the Nijhoff International Philosophy series. He got it accepted. Nijhoff offered me a contract with no royalties on the first 500 copies sold, then 5% on the rest. I said I should get 5% on the first 500 copies, too. They agreed. But they wouldn't tell me how many copies they planned to print. It turned out to be 500. At a high price. Most were sold for standing orders for the series by libraries.

After the printing was sold out, Kluwer had no more interest in the book. So I was able to get the rights back.

2nd edition, Oxford University Press, 1995.

When I submitted *Predicate Logic* (annotated below, p. 20) to OUP, Dov Gabbay accepted it, and I asked OUP to also publish this new edition. They did, but in their advertising they confused it with *Predicate Logic*. From the Preface:

> In 1992 I was asked to publish the second volume of my series *The Semantic Foundations of Logic*, and I suggested doing a second edition of *Propositional Logics*. There were a few corrections that colleagues had pointed out, and I thought I could clean up the text a bit.
>
> It turned out that a lot of corrections were needed, both to the technical work and the exposition. For the second edition I revised the entire text, with more changes than I could easily list here. Among the most significant are the correction or simplification of many axiomatizations, the addition of examples of formalization of ordinary reasoning, and the addition of exercises to make the text more suitable for individual or classroom use.

3rd edition Advanced Reasoning Forum, 2012.

From the Preface:

> In 2011 Esperanza Buitrago-Díaz came to the Advanced Reasoning Forum at Dogshine as an ARF Student Fellow to work through the second edition of this text with me. Her questions and comments, difficulties and insights led me to prepare this new edition. The most notable differences from the second edition are:
>
> - The chapter on the general framework now follows the development of the examples of logics rather than preceding them.
> - In the chapter on modal logics the logic of logical necessity is developed before accessibility relations are introduced.
> - In the chapter on paraconsistent logics a new approach to paraconsistency is introduced by modifying the notion of semantic consequence.
>
> In my recent studies I have tried to place formal logic in the larger context of a general theory of inference. The first presentation of that was in my *Five Ways of Saying "Therefore"*. The mature version can be found in my series of books *Essays on Logic as the Art of Reasoning Well*. It would have been too large a project to modify this text to fully take account of that work, although I have made some changes in Chapters I and II to reflect those ideas.

In preparing this edition I was very uneasy about classical modal logic. There are two deduction theorems and two completeness theorems for every modal logic in the text: one relative to a model, one relative to all models of the logic. I sensed but could not say why this mixing of semantics into the syntax was wrong. I could not give good examples of how to use the logics, for in ordinary reasoning we never iterate modal operators as in the formalism: I can make no sense of "It is possible that it is necessary that it is possible that Ralph is a dog". That is why I first present the logic S5 in the chapter on modal logics, for in it all iterations of modal operators collapse into a single use of the necessity operator or the possibility operator, perhaps with negation. It was only in "Reflections on Temporal and Modal Logic" (annotated below p. 17) that I was able to identify the problem.

In Chapter VIII.C I present the work from "Paraconsistent Logics with Simple Semantics" (annotated directly below). I point out that this semantic consequence for relatedness logic is not transitive, for overlap of subject matter is not transitive. This gives an example of a logic for which consequence does not satisfy the conditions that Tarski set out for characterizing consequence operators.

In this third edition I also introduced criteria of formalization for propositional logics based on the criteria of formalization for predicate logic given in *Predicate Logic* (annotated below p. 20).

Valid Deductions for Falsity-Default Logics

Reports on Mathematical Logic, 26, 1992, pp. 89–95.

I present a formal system from which all and only inferences valid for all logics based on the general framework can be derived. This was incorporated in Chapter IV.G.8 of the second edition of *Propositional Logics* and in the 3rd edition.

Paraconsistent Logics with Simple Semantics

Logique et Analyse, vol. 189–192, 2005, pp. 189–207.

Relatedness logic and dependence logic do not satisfy the Deduction Theorem: B is a semantic consequence of A iff $A \rightarrow B$ is valid. That is because the contents of A and B are not taken into account in evaluating consequence: B is defined as a semantic consequence of A iff in every model in which A is true, B is true. Newton da Costa suggested that we should take account of content in defining semantic consequence, which is what I do here. The result of doing so is that the logics are paraconsistent, for B is not a semantic consequence of $A \wedge \neg A$ unless the contents of A and B are correctly related.

Originally paraconsistent logics were presented entirely syntactically, modifying axiomatizations of classical propositional logic to avoid deriving any proposition from a contradiction. Only much later were semantics offered for some of those and other systems of paraconsistent logic. I never could understand those semantics, and I offered this paper as providing a clear semantic basis for reasoning that does not allow that from a contradiction we can conclude any proposition whatsoever.

Reflections on Temporal and Modal Logic

Logic and Logical Philosophy,vol. 24, no. 1, pp. 111–139, 2014.
DOI 10.12775/LLP.2014.015.

A Propositional Logic of Temporal Connectives with Esperanza Buitrago-Díaz

Logic and Logical Philosophy, vol. 24, no. 2, pp. 155–200, 2014.
DOI 10.12775/LLP.2014.015

When Esperanza Buitrago-Diáz was at Dogshine in 2011–2012 with an ARF Student Fellowship, she read through *Propositional Logics* and helped me prepare the third edition. I wanted to add a chapter on temporal propositional logic. But I didn't understand that subject enough to write a chapter. So I tried to figure out on my own how to formalize reasoning taking account of time. Most basic, it seemed to me, is the idea of before and after, which I now suspect is part

if not all of talk that takes account of time in any language-culture. I struggled with how to formalize reasoning that takes account of before and after, comparing intervals, until Esperanza hit on the solution: focus on endpoints of intervals of time, using those to define precedence (before) and overlap. The result of our collaboration is the second paper, which was finished after the third edition of *Propositional Logics* was published.

Later, after Esperanza left, I began to read about temporal logic in the tradition of Arthur Prior, wanting to explain that to her. That approach uses temporal operators "Past" and "Future", as in "Past (Ralph is a dog)". It hit me that there was a great mistake in that work. In a model, "Ralph is a dog" is taken to be a proposition, true or false. But in "Past (Ralph is a dog)" the phrase "Ralph is a dog" is taken to be a scheme of propositions, for the compound proposition is true in the model iff at some time in the past "Ralph is a dog" is true. So "Past" does not operate on propositions but on schemes of propositions, and any justification for iterating the operators is gone. Then I noted that exactly the same occurs with using the modal operators "Necessary" and "Possibly" in classical modal logic, which was the guide that Prior used in creating his temporal logic. Modal logic, too, was based on a confusion of proposition and scheme of propositions that could not be rectified. My misgivings about modal logic in writing the third edition of *Propositional Logic* were justified. And the incredible complexity of modal predicate logic can be seen to be an effect of this confusion.

I gave a talk on this to a seminar at the University of Minas Gerais in Belo Horizonte in 2014 which Walter Carnielli attended. It took only a few minutes to show why temporal logics based on temporal operators was nonsense. He and the others agreed. But when I showed that the same analysis applies to propositional modal logics, he was sure I was wrong. He had written extensively on those. He would find a fix, he said. But no one has found a fix, though almost no one has noticed the paper.

A deeper problem with propositional modal logic with operators shows up when we try to make sense of "Possibly Necessarily Possibly Ralph is a dog". We have to rely on the formal semantics, for we never (rarely?) iterate modal operators in our speech. We have no intuition to guide us. And that is because "possibly" and "necessarily" are not operators on propositions or sentences. We don't say "Necessarily Ralph is a dog". We might say "Possibly Ralph is a dog", but that should be understood as elliptical for " "Ralph is a dog" is possibly true". Then it is clear exactly why propositional modal logic is a mess, for it is an attempt to import the meta-logic into the logic without taking account of the predicate "is true". I have sketched how we might develop modal logic by incorporating "is true" into the formal language, as in my "A Theory of Truth Based on a Medieval Solution to the Liar Paradox" (annotated below p. 19), taking "necessarily" as a predicate modifier that can be applied to that predicate, using the analyses of predicate modifiers from *The Internal Structure of Predicates and Names* (annotated below p. 24ff). But there are difficulties in following that through. And I am old.

The Liar Paradox and Truth

Truth is Beauty

Essay review of *John Buridan on Self-reference* by G. Hughes, *History and Philosophy of Logic*, vol. 6, no. 1, 1985, 117–125.

Charles Chihara taught a graduate seminar in the Philosophy Department at Berkeley on theories of truth which I attended. Several new theories of truth were being propounded by Kripke, Gupta, and others. These were meant to supplement or replace Tarski's theory, which avoided the liar paradox by not allowing the predicate "is true" in the language. I described Buridan's work in this review, which fit in well with my developing ideas on logic and logics. Buridan showed that we could, and I now think, should base logic not on types of propositions or formulas but on tokens, particular instances, giving conditions for when we're justified in identifying two tokens of the same type as the same proposition. I posed the problem to give a modern formal theory of truth based on Buridan's work.

It was in reading this that I saw how important it is to give examples for analysis, as Buridan did. Examples motivate and explain the general ideas that are meant to comprise an approach, though not in Buridan's time what we would call a theory .

A Theory of Truth Based on a Medieval Solution to the Liar Paradox

History and Philosophy of Logic, 13, 1992, pp. 149-177.

In this paper I presented a formal system of predicate logic in which the predicate "is true" is allowed in the formal language along with names for formulas of the formal language, based on the work of Buridan. I developed that system without as much commentary in *Classical Mathematical Logic* (annotated below p. 23).

I first submitted this to the *Notre Dame Journal of Formal Logic*. The review came back saying that it isn't possible to do formal logic taking tokens as true or false rather than types or abstract objects. I wrote to the editor and said that it is possible, and that this paper shows how, and cited other work on mathematics of tokens. I pointed out that an example the reviewer said couldn't be handled by this theory was one of the worked examples later in the paper, so he or she hadn't read that far. The editor was not interested. Nominalism wasn't even considered in formal logic.

See also "The Twenty-First or "Lost" Sophism on Self-Reference of John Buridan" annotated below (p. 61).

Predicate Logic

Predicate Logic (*The Semantic Foundations of Logic*)
Oxford University Press, 1994. Reprinted, Advanced Reasoning Forum, 2012.

I wanted to extend the general framework for semantics for propositional logics to predicate logics. To do that, I had to understand the semantic basis of predicate logic. It seemed that only in the early years when predicate logic was being formulated by Peano, Frege, Russell, and others was close attention paid to issues of what is a predicate, what is a name, what it means to say that a predicate is true of an object. Those analyses were tied to a project of using predicate logic to analyze and formalize reasoning in mathematics, or as it was said then, giving a foundation for mathematics. For that, the originators and those that followed mathematized the formal language and semantics, relying almost always on a platonist conception of the world. Those explanations could not serve as a basis for a general framework for logics other than classical logic, certainly not for intuitionist logics or relatedness logics.

So I set out to give a semantic basis of predicate logic that would depend on only metaphysical assumptions that could serve for many views of logic. I met with Benson Mates regularly, going to his office where I would try out my attempts. Benson never criticized, never disagreed; he listened, but I could tell that I wasn't making good sense when he would lift an eyebrow quizzically. Then we would discuss.

After leaving Berkeley and settling in Cedar City, Utah I got a grant from the Brazilian research funding organization CNPq (Centro Nacional de Pesquisas) to visit and teach at the Philosophy Department of the University of Paraíba in João Pessoa, Brazil. Teaching a class on predicate logic, I tried to give ordinary language examples—in Portuguese—that would make clear(er) the issues involved. This was the start of my using the example-analysis format for testing a general theory, which I've used in all my subsequent books in logic and reasoning. The use of examples and format was motivated by my reading George Hughes' translation of Buridan's sophismata (see the annotation for "Truth is Beauty" above, p. 19).

Working on the project back in Cedar City, I was uneasy about some of my analyses. I called Mates from time to time, and I sent drafts to George Hughes who was willing to read them and comment on them regularly via long-distance telephone calls to him in New Zealand, which helped enormously. George Hughes and Benson Mates set for me a standard for what it means to be a scholar and philosopher. They came from a time when (some) philosophy professors were philosophers, and their historical studies were for not just history but to better understand problems that philosophers, above all, worry about.

Here I will list the main points in the book which, along with the overview in the final chapter of *Propositional Logics,* I've used in my all my later work in logic and reasoning.

• Predicates, names, and propositions are taken to be linguistic. For platonists those can be understood as representatives of "real" abstract predicates and propositions, though abstract counterparts of names are not posited by them.

• The division of atomic propositions and the use of quantification are based on the assumption *Things, the World, and Propositions*: the world is made up of individual things, and the propositions in which we interested are about individual things. This does not assume that the world is made up only of things.

• The idea of a semi-formal language is introduced: a realization of the formal language by assigning ordinary language predicates to predicate symbols and ordinary language names to name symbols.

• The inductive definition of truth in a model begins with assigning or recognizing the truth-values of atomic propositions in a semi-formal language, including those such as "*x* is a dog" when a reference is supplied for *x*. No particular choice of what is meant by an atomic proposition being true or a predicate being true of an object is taken as fundamental.

• An investigation is made of what is meant by "supplying a reference for a variable", which is essential for formulating semantics for quantification. For each kind of object in the universe of a model a criterion of identity is needed. Since there is no criterion of identity that is applicable to all objects whatsoever, the universe of a model cannot be taken to be all objects.

In discussing the evaluation of the equality predicate I raise the issue of how we consider a thing to persist in time through its changes.

• I point out that instead of evaluating the equality predicate using the informal criteria of identity for objects in the universe, an evaluation could be made according to the principle of the identity of indiscernibles: if two things are distinct, then there must be some property that one has which the other does not. This can be formalized in predicate logic by evaluating the equality predicate to hold of what we recognize as two distinct objects of the universe if there is no open formula of the language that is true of one and not the other. I called this the "predicate logic criterion of identity".

• The theory is tested with examples of formalization of ordinary language propositions or apparent propositions into a semi-formal language. Some criteria for judging the aptness of formalizations is needed, which I formulated. This was new, for I could find none in the literature or textbooks on predicate logic: examples of formalization or exercises in formalizing that are given suppose that what is a good formalization is obvious.

• Three assumptions are made to begin the analysis of truth in a model.

Form and Meaningfulness

What is grammatical and meaningful is determined solely by form and what primitive parts of speech are taken as meaningful. In particular, given a semi-formal propositional language, every well-formed formula will be taken to be a proposition. And given a semi-formal language for predicate logic, every well-formed formula will be taken as meaningful, and every closed formula will be taken as a proposition.

The Fregean Assumption

The truth-value of a complex proposition is determined by its form and the properties of its constituents.

(This was used in the first edition of *Propositional Logics*.)

The Division of Form and Content

If two propositions have the same semantic properties, then they are indistinguishable in any semantic analysis, regardless of their form.

(This was added to the second edition of *Propositional Logics*.)

• An important supplement to *Form and Meaningfulness* is the rule, made more or less explicit here and used in all my subsequent work, that we can assimilate what is nonsense to what is false. This allows "7 is green" to be in a semi-formal language by classifying it as false.

• I show with examples that predicate logic cannot be used to formalize or judge reasoning about masses and processes, or rather using mass terms or process words, for reasoning about those is not compatible with the assumption that we are concerned with propositions only to the extent that they can be construed as about individual things.

• I give an analysis and development of how to extend predicate logic to allow for quantifying over predicates in what is called "second-order logic". Though taking predicates viewed as linguistic can serve for many different notions of predicates in the semantics and syntax of predicate logic, whether we understand predicates as linguistic or abstract leads to different second-order logics.

• In an appendix , "The Notion of *Thing* in Predicate Logic", I collect the assumptions about the nature of things made in the text and suggest that predicate logic as a whole serves to formalize our notion of thing. I raise the question of whether there are languages for which the assumption that the world is made up of things is not apt and hence for which predicate logic could not serve to formalize or analyze reasoning in that language.

The Metaphysical Basis of Logic

Manuscrito, vol. 22, no. 2, 1999, pp. 133–148.

I was stimulated to write this by hearing of the title of a paper by Michael Dummett, "The Logical Basis of Metaphysics". Here is the abstract.

> Logic is not metaphysically neutral. For propositional logics assumptions are needed about the nature of the world as it relates to propositions. Predicate logic assumes a metaphysics of individual things. The metaphysics is reflected in the linguistic forms chosen for investigation. The scope and limitations of the logic are determined in large part by the metaphysics.
>
> The metaphysics of predicate logic precludes analysis of inferences that depend on aspects of verbs. A logic of processes is needed, for there is much more in the world than everything.

Classical Predicate Logic with Non-Referring Names

Logique et Analyse, vol. 189–192, 2005, pp. 71–86.

Here I show how we can reason with non-referring names in classical predicate logic. This is developed more in *The Internal Structure of Predicates and Names* (annotated below p. 24ff).

Relatedness Predicate Logic with Stanislaw Krajewski

Bulletin of Advanced Reasoning and Knowledge, vol. 2, 2004, pp. 19–38. Available at www.AdvancedReasoningForum.org/bark-volume.

In this paper Stanislaw Krajewski and I present predicate logics based on taking account of subject matter of predicates and names. This was meant to be part of a volume of *The Semantic Foundations of Logic* giving a general framework for semantics for the predicate logics of the propositional logics presented in *Propositional Logics*. I never wrote that, discouraged by the complexity (Walter Carnielli had classified ways to add quantifiers to many-valued logic) and lack of clarity (modal logic) of presentations of non-classical predicate logics.

The history of this paper is given on the first page of it, dating it back to work that Krajewski and I began in 1981.

Classical Mathematical Logic (*The Semantic Foundations of Logic*)

Princeton University Press, 2006.

My first year as a graduate student in mathematics at the University of California, Berkeley I was eager to learn logic. That year Joseph Shoenfield's textbook on mathematical logic came out and was used for the first-year graduate course. Something better was needed (see my comments on that book on p. 3 and p. 4 above.)

Later Ladislaw Szczerba visited the department and gave lectures on axiomatic Euclidean geometry. I attended those, for I had really liked the plane geometry course in high school, learning how to use axioms to prove. In Szczerba's lectures I saw for the first time the use of some of the big theorems of mathematical logic, such as Craig's Interpolation Theorem. Later I attended a course on that subject that Szczerba gave at Victoria University of Wellington, New Zealand and again at Iowa State University, where I had invited him. I also learned from him his theory of translations of mathematical theories which are formulated in classical predicate logic, particularly the example of translating the theory of Euclidean geometry into the theory of real numbers and vice-versa.

In 1981 I went to Warsaw on a National Academy of Sciences Exchange to the Polish Academy of Sciences and studied with Szczerba my notes on his courses, trying to set out clearly the development of axiomatic Euclidean geometry, which is what I include in this volume. It is, I believe, the first presentation that clearly distinguishes the syntactical theories from the semantics, the "geometry" that was used to justify that certain formal propositions were consequences. By the time this work was ready to be checked by Szczerba he had died, so I had to rely on my own limited skills as a geometer for the presentation.

From the Introduction:

> The word 'mathematical' in 'classical mathematical logic', then, had two meanings: the mathematization of models of reasoning, and the use of formal logic to formalize reasoning in mathematics.
>
> The 'classical' in 'classical mathematical logic' in the 20th century came to mean the extreme restriction of the notion of predicate to just its extension: those objects of which it is true. This was in opposition to other views of reasoning in which epistemological, psychological, modal, or other aspects of propositions and predicates were taken into account.

> In this text we we will see the basic outlines of the limitations as well as the scope of modeling reasoning within classical mathematical logic.

The view of mathematical logic as illuminating our mathematics is what I took as direction when I set out to give a presentation of classical mathematical logic. I did not intend to innovate in the presentation, only provide a careful exposition that would pay attention to the semantic assumptions which were made in the development.

From the Preface:

> In this volume I carefully trace the path of abstraction that leads to classical mathematical logic in order to clarify the scope and limitations of the subject.
>
> In Chapters I–XI I set out the language and semantics of the logic and give an axiomatization of the semantics.
>
> Chapters XII–XIX then present applications of the resulting logic to mathematics, beginning with the most abstract part of mathematics, group theory, and progressing through the study of linear orderings, the natural numbers, the integers, the rationals, and finally the real numbers. The real numbers can be understood axiomatically (Chapter XVII), as constructed from the rationals (Appendix to Chapter XVII), or geometrically (Chapters XVIII and XIX). The formalization of axiomatic geometry and its relationship to the formalization of the real numbers is important because it requires a careful analysis of how theories of different subjects in different languages are related, which is the subject of Chapter XX on translations between theories.
>
> By paying attention to differences that were previously ignored along the path of abstraction, we can devise models of reasoning that take into account more of the world and our reasoning, for, above all, logic is the art of reasoning well. Though much of that project is for a subsequent volume, in this volume I present two examples. In Chapter XXI I show how we can relax the assumption that every name must refer to an object. In Chapter XXII I show how by no longer considering propositions to be types we can lift the restriction that we cannot reason about our logic within our logic.

The Internal Structure of Predicates and Names
(Volume 1 of *Logic, Language, and the World*)
Advanced Reasoning Forum, 2016.

Time and Space in Formal Logic
(Volume 2 of *Logic, Language, and the World*)
Advanced Reasoning Forum, 2022.

> In "The Metaphysical Basis of Logic" (annotated above, p. 22), I gave an example of a valid inference that could not be formalized in predicate logic: "Juney is barking loudly, therefore Juney is barking". I said that was because the focus of the inference is on the process of barking, not on the object Juney.
>
> Sometime about 2006 when I was out for a walk with my dogs it hit me that we could formalize that inference in predicate logic if we take "loudly" to be a modifier of the predicate "is barking". I set out to show how to do that. I found that some other people had talked about predicate modifiers and predicate restrictors, but none had developed that beyond observations and classifications.

Then the project grew to try to figure out how to reason in predicate logic taking account of time and space. But in that as well as the work on predicate modifiers I kept coming up with examples about mass or process that could not be formalized in predicate logic reasoning, according to the criteria of formalization I had given in *Predicate Logic*. So I set out to develop a logic that could be used for reasoning about mass and process.

By 2009 I had developed the analyses and made a draft of a book *The Internal Structure of Predicates and Names with an Analysis of Reasoning about Process*. It was over 600 pages. I announced it on the FOM (First-Order Mathematics) electronic bulletin, but got few responses and I distributed it to only a few people.

Eventually I divided up that book into three parts, the first two parts of which are the books annotated here. The third part, only now just published, is *Reasoning about the World as the Flow of All* (annotated below p. 57).

From the Introduction to *The Internal Structure of Predicates and Names*:

> I show how we can extend classical predicate logic to formalize reasoning that involves adverbs and relative adjectives by viewing those as modifiers of simpler predicates. What we previously took to be atomic predicates, such as "barking loudly", can then have internal structure. Reasoning that involves conjunctions of terms, as in "Tom and Dick lifted the table", conjunctions of modifiers, conjunctions of predicates, and disjunctions of predicates can also be formalized by viewing them as part of the internal structure of atomic predicates.
>
> The internal structure of names is the topic of the third and last section. Names for functions are used in classical predicate logic to form complex names, such as "sin (x^2)", which is what I present first. In our ordinary reasoning we use descriptions to form functions, such as "the wife of", and we use descriptions to form names, such as "the cat that scratched Zoe". To reason with those we need to take account of their internal structure, which we can do if we drop the assumption, basic to classical predicate logic, that every name must refer to a specific thing. Then we can devise formal logics as a guide to reasoning with simple, atomic names that do not refer.
>
> Others use logic as a bulwark against the mysteries. They build a wall within which reason reigns and live within the cities built of logic. I use logic as a way into the mysteries, using reason where I can to lead me to the boundary beyond which reason has no sway if we are to enter. Logic is the path, not the end. There is no end but only a continual beginning.

From the Preface to *Time and Space in Formal Logic*:

> What is time? What is space? I will not try to answer these questions, if they even make sense. What we can do is try to understand what role time and space play in our talk and reasoning, hoping to come to some better understanding of what it is we believe and perhaps a better understanding of the world.
>
> In this book I present two very different approaches. . . .
>
> *Relative Times*
>
> We say "Spot barked before Dick yelled"; we say "Tom met Suzy after Tom broke his foot". We talk of before and after. But before and after what? We

can and do pick out times with true descriptions: "Spot barked", "Dick yelled", "Tom met Suzy", "Tom broke his foot". We order them as describing before and after. This is all we need to take account of time in our reasoning: a minimal metaphysics of before and after, codified with temporal propositional connectives.

Times and Locations as Things
A different approach assumes instead that we can talk about times and locations as if they were things, quantifying over them in an extension of classical predicate logic. This is closer to what has been done by others in trying to include talk of time and space in the scope of formal logic and mathematics. But paying attention to the metaphysical assumptions on which to proceed, there are many hard questions to investigate, even before setting up formal systems, and then many more arise in formalizing ordinary language propositions and inferences.

An Introduction to Formal Logic

Advanced Reasoning Forum, 2016. Second edition, 2020.

I wanted to make a text for a first course on formal logic. I culled from my books on formal logics this introduction. Only in 2019 did I have an opportunity to teach an introductory course on formal logic, in the Philosophy Department at the University of New Mexico . I found that my text was too difficult, and I revised it considerably for a second edition. I later called this "Volume 0" of the series *Logic, Language, and the World*.

Critical Thinking textbooks

Critical Thinking

Wadsworth, 1998.
5th edition with Michael Rooney, Advanced Reasoning Forum, 2017.

While living in Cedar City, Utah I was asked to fill in to teach critical thinking in the Philosophy Department at the University of Nevada, Las Vegas. I had very little notice and looked at the books that were being used for that course. I chose one by Brooke Noel Moore and Richard Parker because it had lots of examples. I commuted and taught four sections, meeting Tuesday and Thursday. After teaching that book one semester I had to prepare notes for students to explain what was wrong with it, from incoherent or wrong theory to bad analyses of examples. The other texts that were available were just as bad, only with fewer examples.

So I set out to write a textbook. Here is what I wrote in 2005 in response to a questionnaire, the origin of which I can't remember but seems to be from a student who was making a survey.

> *CT was "founded" by John Dewey around 1915. Since then, has there ever been a period in which Critical Thinking has received more attention than in other periods?*

NO, NO, NO. Critical Thinking was not founded by John Dewey in 1915. That is a common misconception of people who do not know the history of rhetoric and logic.

Perhaps you mean by "critical thinking" just thinking hard. The definition I give in my books is:

> " Critical thinking is evaluating whether we should be convinced that some claim is true or some argument is good, as well as formulating good arguments."

The first to teach critical thinking in earnest whose work we have was Socrates. Aristotle was the first theorist in his *Organon*.

As time went on, logic began to have two sides: the formal, basically Aristotelian logic, which became quite sophisticated in the 14th Century, and the "informal", which amounted to studying cause and effect, induction, etc.

Formal logic began a long decline after the 14th Century. Arnauld and Nicole in the late 1600s wrote their 'Logique ou l'art de penser'. In it only a small fragment of Aristotelian logic--the syllogisms--was formal logic, and the rest was informal logic mixed with what we now would consider psychology. By the standards of what most critical thinking texts contain now, this was the first critical thinking text.

The degenerate tradition of logic codified in Arnauld and Nicole's work dominated all study of reasoning until the early 1800s. At that time a renewed interest in Aristotelian logic began, slowly discovering the great work of the medievals. At the same time problems in the foundations of mathematics engendered a new kind of formal logic. That was taught in both mathematics and philosophy departments, while at the same time the degenerate tradition of teaching fragments of Aristotelian logic and fallacies continued to be taught as "rhetoric". However, the new formal logic became so potent and intellectually deep that from at least the 1950s it began to dominate all teaching of reasoning, with rhetoric courses concerned more with, well, "rhetoric" and not reasoning.

This was unsatisfactory for students because they were not being taught how to reason, for formal logic is not much use in everyday life . . . . Max Black in the 1950s

(I don't have the exact date) wrote what could be considered the first modern "critical thinking" text.

The problem with critical thinking as it was developed from 1950 onwards, then, is that it was a reaction to the dominance of mathematical logic and to formal logic methods in philosophy. But the people who took that view and wrote critical thinking textbooks were not well trained in formal logic methods. So they were stumbling around as if nothing serious new had been developed of any interest since about the early 19th Century. They began with the work of John Stuart Mill, if they even knew that. Or they tried to re-invent everything. There was a huge emphasis on fallacies and their classification; see for example the work of Douglas Walton. Almost all of that is worthless--see the discussion of fallacies in my book *Five Ways of Saying "Therefore"* . . . Fallacies are just labels that shortcut the process of argument analysis—they have no deeper meaning.

So the work in this area and the research was almost completely uninformed about research in formal logic, and only slightly better informed about research in philosophy (scientific methods analysis, induction, cause and effect, Aristotle, . . .). They called the new subject "Informal Logic", but really it was just logic. For some of it, formal methods are useful; for others no formal methods have been developed, yet it seems formal methods might be useful and a challenge is to find some; and for other parts it seems on the face of it that no formal methods will ever be useful (e.g., induction, cause and effect). The informal logicians are a bunch of "know-nothings", defensive about their ignorance by claiming formal logic and the history of philosophy isn't really useful anyway. A perfect example of this is an appendix in the textbook *Practical Reasoning in Natural Language* by Stephen Naylor Thomas on how worthless formal logic is. Any of those big researchers whose work you're interested in you can probably find in my index to my Five Ways and then read an analysis there by me of their most important work.

The result was that a lot of absolute rubbish was written. Almost everything in that subject. The journals are awful—I remember reading one of them, I think the Journal of Informal Logic, where someone about 1980 discovered that the conditional of classical propositional logic doesn't require that the antecedent and consequent be related. That is, he or she just rediscovered the "paradoxes of material implication", a subject that has been very extensively studied and discussed since about 1930. And he presented it as basic new research. The Association for Informal Logic and Critical Thinking, and the International Society for the Study of Argumentation—and any journals they publish—are hopelessly bad intellectually.

When I was asked to teach critical thinking in 1995 I almost said no, because the course had such a bad reputation and was so worthless. But I took on the job and quickly learned how important it was to my students to be able to reason well and how little formal logic had to offer. I used a textbook and surveyed others; I found them so hopelessly bad—everything was either too incomplete to use in actual reasoning or was actually wrong—that I began to write my textbook. I published *Critical Thinking* in 1998. It has been considered by philosophers to be the first intellectually respectable textbook on the subject. At the same time I was writing my *Five Ways* and finished that in 2001. It is the first book to cover all of logic since John Stuart Mills'. At the very least, you should understand the material in that book before attempting to analyse the critical thinking movement. . . .

To write intelligently and *critically* about critical thinking, you need to read and understand Aristotle's organon, have some background in medieval logic

(Duns Scotus, Buridan, etc.), understand how Arnauld and Nicole's work relates to earlier work and how it dominated the teaching of reasoning for 150 years (compare Kant's work on logic), read well John Stuart Mill's *System of Logic* and understand how it was a revolutionary work and how profoundly it affected teaching and research in non-formal areas of logic for 150 years, be conversant with modern formal logic, and with the history of the teaching of rhetoric and Aristotelian logic from the late 1800s to about 1950, and then, only then, can you appreciate what was going on in critical thinking from about 1950. You will then see that the work on critical thinking from that time forward was for the most part stuck directly in the degenerate tradition of Arnauld and Nicole, and how absolutely dreadful the books on the subject have been. Critical thinking was to the art of reasoning well as, in literature, Donald Duck comic books are to Dante's *Inferno*.

By the beginning of my second year I had written a few chapters, which I sent to the critical thinking editor at Wadsworth, Peter Adams. A few days later I found a phone message saying that he had received the material and would get back to me. And then another phone message saying how he loved the material and was quite enthusiastic. Thus began a long collaboration with him that has led to improving this book through several editions and to other work (see p. 35, p. 36, and p. 8 here). Starting with the second edition I dedicated the book to him.

The most important innovation in *Critical Thinking* was the method for evaluating arguments. It begins with the following.

The Principle of Rational Discussion We assume that the person with whom we are deliberating or whose argument we are reading:

a. Knows about the subject under discussion.
b. Is able and willing to reason well.
c. Is not lying.

This replaces the unusable and unmotivated principle of charity invoked by others as a rule for argument analysis. If it is not satisfied, then there's no point in reasoning with that person. It is used as justification for the following.

The Guide to Repairing Arguments

Given an apparent argument that seems defective, we are justified in *adding* a premise or conclusion if all three of the following hold:

1. The argument becomes stronger or valid.
2. The premise is plausible and would seem plausible to the other person.
3. The premise is more plausible than the conclusion.

If an argument is valid or strong, we may *delete* a premise if doing so does not make the argument weaker.

Unrepairable Arguments

We don't repair an attempt to convince if:

There's no argument.
The argument is so lacking in coherence there's nothing obvious to add.
The obvious premise to add would still leave the argument weak.
The obvious premise to add to make the argument valid or strong is false.
A premise is false, dubious, or question-begging and cannot be deleted.
Two of its premises are contradictory, and neither can be deleted.
The conclusion is clearly false.

There is more in it that is new, much of which I discuss below in the annotations to the books in the series *Essays on Logic as the Art of Reasoning Well*. Here I'll comment on what I brought new to teaching the subject.

I thought that adding some diagrams, as I used in class, would be useful. I saw some good cartoons in the student newspaper done by Alex Raffi, and I contacted him. We worked together and from just a few diagrams we began to add cartoon characters who could illustrate the ideas with examples of good and bad reasoning. Alex Raffi is a genius, imaginative and clear at creating what I often only saw dimly. In working together I drew on my experience writing dialogue in plays (see p. 60 below). In the end he drew more than 100 cartoons, which Peter Adams was able to get Wadsworth to pay for. A great collaboration; we still talk and encourage each other.

Critical thinking should be active, not just passive mastery of some theory and examples. So I added writing lessons. When I first assigned them, students said they didn't know how to begin. So for each lesson I included one or two sample essays on a slightly different topic as written by the cartoon characters. Some were meant to be bad, others good, but with corrections by the cartoon instructor Dr. E they allowed students to begin their work. The first day of classes I gave this one:

> Write an argument either for or against the following:
>
> "Student athletes should be given special leniency when the instructor assigns course marks."
>
> *Your argument should be at most one page long*.

One woman in the class came up to me and said she was a senior in English and knew how to write essays, but she was forced to take this course because it was a requirement. She knew how to write. The second day of class I collected the arguments and graded them. They were all awful. No one thought to say who counted as a student athlete or what "special leniency" should amount to. The students had learned to fill up a page with waffling. That woman came back and said that now she saw that she needed the course.

I also added cartoon writing lessons. A picture or sequence of two or three pictures was presented and a claim made about that for which the student was meant to give an argument establishing the claim or show that no good argument could be given from what they saw there. This was a way to get them to see that they could use critical thinking in their daily lives.

Because instructors expected material on diagramming arguments and truth-tables, I included those as appendices, and later I added an appendix on Aristotelian logic. Diagramming arguments is worthless and in later editions I deleted that, while the other two appendices were made available as online supplements.

I knew that an instructor's manual would be important. I remembered teaching Euclidean geometry at Hayward State University on short notice; there was a good textbook for the course, but what saved me was the instructor's manual for it. The instructor's manual I wrote for *Critical Thinking* had teaching suggestions and answers to exercises as well as explanations of what was new and explanations of why that mattered. For example, I explained why the terms "inductive" and "deductive" did not appear in the text (see the annotation on the essay

"Induction and Deduction" in *The Fundamentals of Argument Analysis*, p. 42 below). From the Introduction to the Instructor's Manual:

> TRUST THE TEXT. The text is supposed to take the place of lectures. You can expect your students to read the text, so you can use class time to explain what they find confusing, for discussion, and for exercises.
>
> Students will feel uneasy at the start of the course that there is often no absolute right answer to an exercise. Explain that we are teaching them judgment. We could give easy, simple examples, so that each exercise would have a clearly correct answer. But then they wouldn't be able to use this material outside the textbook. Right from the start in the Exercises for Chapter 1 there are messy examples.

I wrote *Five Ways of Saying "Therefore"* to set out, explain, and defend what is new in this book. See the annotation of that (p. 35 below) and of the books *Essays on Logic as the Art of Reasoning Well* (p. 37ff).

But who needs to read a critical thinking textbook?

> To the editors BMC Medicine
>
> I have just read an article published in vol. 5, no. 14, 2007 of BMC Medicine "Do citizens have a minimum medical knowledge? A survey" by L. Bachmann, F. Gutzwiller, M. Puhan, J. Steurer, C. Steurer-Stey, and G. Gigerenzer.
>
> In describing their methodology for surveying and generalizing about minimum medical knowledge of citizens the authors say:
>
> > "We recruited participants at six busy locations in Zurich, Switzerland. Eligible participants were randomly approached and asked whether they would agree to take part in the study. We approached 272 pedestrians, and 185 (68%) were willing to take part."
>
> The word "randomly" is used incorrectly here. What the authors describe is a haphazardly chosen sample. It is almost exactly the example I give in my textbook *Critical Thinking* where I illustrate the well-known observation that there is no good reason to believe that a haphazardly chosen sample is representative of any group other than itself. Hence, there is no reason to think that the facts about their surveyed sample generalize to even all citizens of Zurich. Certainly there is nothing in the authors' work that suggests the conclusion:
>
> > "In this sample, Swiss citizens did not know more than a third of MMK [minimum medical knowledge]. We found little improvement from this low level within groups with medical experience (personal or professional), suggesting that there is a consistent and dramatic lack of knowledge in the general public about the typical signs of and risk factors for important clinical conditions."
>
> The survey methodology is so flawed that the entire paper has no worth.

Here is the response:
Dear Dr Epstein,

> Many thanks for your email and expressing your concern with an article we published in BMC Medicine last year (http://www.biomedcentral.com/1741-7015/5/14).
>
> Manuscripts submitted to the BMC-series journals do go through rigorous review and we rely on our reviewers to let us know if a manuscript is scientifically sound. One advantage of publishing in the BMC-Series is that the pre-publication history is available from the manuscript page, in this you can look at what the reviewers said

in their reports and how the authors responded.

We do currently operate a comments system, again it can be accessed from the manuscript page (http://www.biomedcentral.com/1741-7015/5/14/postcomment) from the menu to the left, if you'd like to post a comment please do. The comments are moderated by the editor following a set of guidelines. I'd encourage you to keep your comments constructive should you wish to post a comment. Once accepted by the editor a copy is sent to the authors allowing them the opportunity to respond.

Many thanks for your interest in this article.

With best wishes,

Annabel Phillips PhD, Senior Assistant Editor
BMC-series Journals, BioMed Central

2nd edition, 2001

Notably new to this edition:

Many new cartoons are added with now 38 cartoon writing lessons along with hundreds of new exercises and examples.

A new *Science Workbook* was written to accompany the text.

Examples and Exercises from the Law were added to the Workbook.

There is a new appendix on Aristotelian logic.

3rd edition, 2006

with Carolyn Kernberger

Notably new to this edition :

A new section on prescriptive and descriptive claims has been added.

A new section on graphs has been added to the chapter on numerical claims.

A new section on advertising and the Internet has been added to the chapter on evaluating unsupported claims.

This was the first edition published by the Advanced Reasoning Forum.

4th edition, 2013

with Michael Rooney

Notably new to this edition :

A new section on analyzing arguments that involve prescriptive claims has been added.

A new chapter on evaluating risk has been added.

Supplements for legal reasoning, science reasoning, truth-tables, Aristotelian logic, and diagramming arguments are now available as free downloads from the ARF website.

5th edition, 2017

with Michael Rooney

There is a new section of seven chapters on reasoning in the sciences.

Material on explanations has been added. The basics are in Chapter 16 and the use of explanations in science is covered in the science section.

There is now enough material in the text for a two-semester course.

Pensamento Crítico: O poder da lógica e da argumentação

Walter Carnielli and Richard L. Epstein with the assistance and collaboration of Desidério Murcho. Editora Rideel, 2009. 5th edition, 2024.

Walter Carnielli adapted *Critical Thinking* for Brazilian students, with only a little input from me.

The Pocket Guide to Critical Thinking

Wadsworth, 1999. 5th edition Advanced Reasoning Forum, 2016.

This started as just a summary of the main points in the book and then grew to a short presentation of the essential material from *Critical Thinking*. It is meant to be read by students without the help of a teacher. It became popular especially after a good review of it by Timothy Murphy in *Teaching Philosophy* (vol 40, no. 1, pp. 119–122).

The Pocket Guide to Critical Thinking

Arabic edition, 2014.

I was contacted by a Saudi Arabian publisher Jarir Bookstore about publishing an Arabic edition of *The Pocket Guide*. I sold them the translation rights. The Arabic edition was made with no input from me, though I offered to help.

Pensamiento Crítico

Spanish edition of *The Pocket Guide to Critical Thinking*
Manuel Dahlquist and Juan F. Rizzo. July, 2018.

Answering questions and working with Manuel Dahlquist and Juan Fransisco "Pancho" Rizzo we began a long-lasting collaboration. We found that many examples had to be changed because what was an obviously plausible claim as a premise or needed as a premise was not obvious without an American background.

The Guide to Critical Thinking in Economics

with Carolyn Kernberger, South-Western, 2004.

Reasoning in the Sciences: A Self-Study Guide

Advanced Reasoning Forum, 2008.

These two books were adapted from *Critical Thinking* for specific audiences.

The Guide to Writing Introductory College Textbooks

Advanced Reasoning Forum, 2013.

I wrote this thinking it would be a big help to authors and would be bought by a publisher to give them an edge on the competition. No publisher was interested. Very few copies were sold. The material on how to write a textbook is still good, but the discussion of how to find a publisher, work with an editor, and see the book through to publication are outdated since e-books are now common and textbook publishing companies have been bought by large companies that have no experience in publishing textbooks. Publishers kept raising the prices of their books with the result that in the 2010s the textbook market collapsed as teachers would no longer tell their students to buy a very expensive textbook.

How to Reason: A Practical Guide
Advanced Reasoning Forum, 2019.

How to Reason + Reasoning in the Sciences
Advanced Reasoning Forum, 2019.

In 2016–2017 I taught critical thinking and read stories from *The BARK of DOG* at the local jail in Socorro, New Mexico. You can read about that in "Teaching Addicts" in *Last Writings* noted below (p. 59). I started by handing out copies of *Critical Thinking*, but that was too hard and required too much work from the students. So we switched to *The Pocket Guide to Critical Thinking*, which I supplemented with lots of examples in class.

Later I rewrote the *Pocket Guide* to make it a self-help book, one which someone could learn from and master with no teacher, with many worked exercises. That's the first book above. The second book extended that to show how to reason in the sciences.

I had thought that these would be perfect for new students to read in the summer before they started at a college or university. Just tell students they have to read it, perhaps even sending it to them, figuring that the cost of doing so would be recouped by fewer students dropping out. No luck.

Reasoning and Inference

Five Ways of Saying "Therefore"

Wadsworth, 2001.

I wrote this book to set out, explain, and defend the views I developed in writing *Critical Thinking*. I saw that inferences are used in five different ways according to the goal of the reasoning.

An *argument* is an attempt to convince, and for it to be good, the inference has to be valid or strong and the premises more plausible than the conclusion.

For an *explanation* to be good, the claim being explained—the conclusion of the inference—has to be more plausible than the premises, and the inference has to be valid or strong.

Counterfactuals and certain other kinds of conditionals can be read as inferences with a false or dubious premise.

A *cause and effect claim* can be understood as a condensed inference: for the claim to be true, the inference from a claim describing the cause to a claim describing the effect must be valid or strong, and those claims have to be clearly true.

Inferences as used in mathematical proofs are evaluated differently, though in this volume I was still searching for what criteria should govern the evaluation of those, which I later gave in "Mathematics as the Art of Abstraction" (annotated below p. 36).

One colleague said he thought the title of the book was worth the price of it. No one, as far as he or I knew, had seen the uses of inferences in reasoning as tailored to different goals.

I was encouraged to write this by Peter Adams, my editor for *Critical Thinking* at Wadsworth. It was to be distributed free to faculty who adopted that textbook. It did not get wide distribution. And since the work was big, too big to be assimilated by a reader without a lot of patience and work, when I got the rights back to it from Wadsworth I decided to break it into separate volumes, which I included in the series of books *Logic as the Art of Reasoning Well* (annotated below p. 37ff). Those, I hoped, would be more accessible.

The appendix on rationality here is expanded in an essay"Rationality" in *The Fundamentals of Argument Analysis* annotated below (p. 44).

Arguments and Explanations

Bulletin of Advanced Reasoning and Knowledge, *1*: *Proceedings of the Advanced Reasoning Forum meeting in Bucharest*, *Romania*, *2000*, New Europe College, 2001.

Available at <www.AdvancedReasoningForum.org>.

This is where I first set out in an academic setting the relation of arguments and explanations as being inverse to each other, which I developed further in *Cause and Effect, Conditionals, Explanations* annotated below (p. 37).

On Models and Theories, with Applications to Economics

Bulletin of Advanced Reasoning and Knowledge, vol. 2, 2004, pp. 77–98. Available at <www.AdvancedReasoningForum.org>.

I was asked by Peter Adams to review a textbook on economics by Mankiw in preparation for a new edition of that. Carolyn Kernberger and I worked on it, and we saw that it was very badly written. My nephew was studying economics then and he had questions, and I came to see that the problem was that the economists had no criteria by which to judge a theory. Completely unrealistic assumptions, indeed ones that could not be true, were justified because they gave "the right results". But that gave no justification for why to accept the results.

In this paper I set out how models and theories in science and economics can be seen as arising from a path of abstraction from experience, which gives standards for when to accept the claims that can be deduced them: when we can be justified in ignoring some of our experience. With many examples I show that correct predictions and incorrect predictions serve to establish the scope of application of the theory, for the assumptions of the theory are abstractions which are clearly not true. New theories replace old ones when the scope of application is found by experiment to be too limited.

I expand on this analysis and include reasoning with prescriptive claims in "Prescriptive Theories?" annotated below (p. 47).

Mathematics as the Art of Abstraction

In *The Argument of Mathematics*, eds. Andrew Aberdein and Ian Dove, Springer-Verlag, 2013, pp. 257–289.

Back in 1981 or 1982 I gave a talk to the mathematics club at Iowa State University where I suggested that mathematics arises by analogy. I wasn't very clear about that, but it was accepted well.

I gave a talk on mathematics as the art of analogy at the University of Nevada, Las Vegas in about 1997, which was clearer and accepted well, but I still felt was lacking some fundamental insight.

Finally, I was able to see a pattern of how mathematical theories arise from abstraction from experience, including experience with other mathematical theories, in the same way as models and theories in science arise from abstraction.

Though it is usual to say that the theorems of a mathematical theory are necessary consequences of the assumptions of the theory (axioms), valid proofs are rarely given by mathematicians. Rather, a mathematical proof of a proposition is a strong argument that there is a valid inference from the assumptions of the theory to that proposition. This can be seen in many examples in *Classical Mathematical Logic* (annotated above p. 23).

I originally titled the paper "Mathematics as the Art of Analogy", but the editors convinced me that it was better to see the method as the art of abstraction. This was the first presentation of this view, which I later included in *Reasoning in Science and Mathematics* (annotated below p. 39).

Valid Inferences

In *Logic without Frontiers:Festschrift for Walter Alexandre Carnielli on the Occasion of his 60th Birthday*, eds. J.-Y. Béziau and M. E. Coniglio, College Publications, pp. 105–12, 2012.

> What is logically possible depends on how we understand valid inferences. But what is a valid inference depends on how we understand possibilities, particularly logical possibilities. We seem to be in a circle with no way out.

I show how we can extricate ourselves from this circle by adopting a formal logic, which constrains what counts as a possibility.

Essays on Logic as the Art of Reasoning Well

A series of books.

I proposed to an editor at the Massachusetts Institute of Technology Press that I could edit a series of books on logic as the art of reasoning well. He replied that no one would be interested. So I set out to write the series myself. The essays in the first three books are derived from *Five Ways of Saying* "*Therefore*" with almost no alteration, except for the essay "Mathematics as the Art of Abstraction".

I wrote these books so anyone with an interest in the subject could understand them with little or no background. In each the subject is developed slowly, making clear the path of abstraction using (what I considered) minimal metaphysical assumptions. That some reviewers saw these books as introductions for students I took as a compliment, for that meant that what was new seemed so apt it was considered standard. And by making it simple, I hoped that even professors could understand it. I included, mostly in footnotes and appendices, discussions of other views, the history of each subject, and ways that other, "more ample", metaphysics could be adopted in the development.

Cause and Effect, Conditionals, Explanations

Advanced Reasoning Forum, 2011.

Here are the essays in it.

- Background: Claims, Inferences, Arguments

This essay presents the basics of inference analysis as first presented in *Critical Thinking* and developed in *The Fundamentals of Argument Analysis* (annotated below p. 40). Necessary conditions for an argument to be good are given:

> The premises are plausible.
>
> Each of the premises is more plausible than the conclusion.
>
> The argument is valid or strong.

- Reasoning about Cause and Effect

> *Conclusion*
>
> The mystery of cause and effect can be circumvented if not eliminated in our reasoning by using claims to describe purported causes and purported effects and understanding a causal claim as true if and only if the relation between those claims satisfies the conditions for a good causal inference. Different notions of

> cause and effect correspond to placing different conditions on what counts as a good causal inference. This provides a method of reasoning about cause and effect that is clear and useful in both our ordinary lives and science.
>
> *Necessary conditions for cause and effect*
> For a particular causal claim to be true, describing the purported cause with a claim A and purported effect with a claim B, the following must hold:
>
> 1. Both A and B are true.
> 2. Given the normal conditions, the inference from A to B is clearly valid or strong.
> 3. Given the normal conditions and perhaps other plausible claims, the inference from B to A is clearly valid or strong.
> 4. A is true of an earlier time than B, and both are true of particular places.
> 5. There is no common cause of both A and B.
>
> Claims offered as normal conditions must be plausible and make the inference valid or strong.
>
> Colloquially, conditions (1–5) are:
>
> - A and B both happened.
> - It's (nearly) impossible for A to have happened and B not to happen.
> - If A hadn't happened, B wouldn't have happened (the cause makes a difference).
> - A happened before B happened.
> - There is no common cause.
>
> The rubric "The cause is close in space and time to the effect" is just the "clearly" in (2) and (3) as well as a reminder that we have adopted a metaphysics of space and time in our understanding of cause and effect.
>
> I wish we could resolve here what we should take as normal conditions and establish sufficient conditions for a causal inference to be good. But at best we can look at many examples and try to refine these conditions. Understanding the causal relation as a special kind of inference does not resolve all problems in our understanding of cause and effect. But it does give us a framework in which to discuss more clearly various points of view about the nature of cause and effect.

Unlike for a good argument, for a causal inference to be good both the premise and the conclusion must be plausible.

Many examples are analyzed in a format for checking each of the conditions above. In some of those a causal claim is made about what is seen in a cartoon, where it is clear that we take the causal claim to be true yet can provide only a strong causal inference with no universal claim among the premises.

- The Directedness of Emotions

Here is the abstract.

> Is every emotion we feel directed at something? Examples from ordinary life suggest not. We can better understand emotions and why we sometimes do and sometimes do not feel justified in calling them directed by using the methods of analysis for reasoning about cause and effect.

The motivation for writing this is presented in the essay.

- Conditionals

In this essay I survey ways we can analyze conditionals and present a new standard for evaluating counterfactual conditionals, that is ones in which it is obvious that the premise is false .

> Sentences of the form "if . . . then . . ." play a major role in our reasoning. Some conditionals, as they are called, are claims, and for those we have criteria for when they are true. Some conditionals are intended to be understood as inferences: were this to be true, this would follow. If meant to be judged solely as valid or not, those can sometimes be evaluated by the methods of modal logics. However, we often use conditionals that we deem good that are only strong or moderately strong inferences, and here I present a theory for how to reason with those.

- Explanations

Here is the abstract.

> Explanations are answers to questions. Verbal answers to a question why a claim is true can be evaluated as inferences that satisfy conditions peculiar to explanations. Some minimal conditions are typically taken as necessary, though not sufficient. Other conditions have been proposed, but they are either difficult to formulate clearly or have not been widely accepted. An important tool in evaluating inferential explanations is to recognize that the direction of inference of such an explanation is the reverse of that for an argument with the very same claims.
>
> Answers to a question about the function or goal of someone or something are teleological. They depend on clarity about the nature of functions and goals, and there is little agreement about criteria for those to be good beyond the necessity of avoiding the fallacy of assuming that because something occurs in nature it must have a purpose or goal.
>
> *Necessary conditions for an inferential explanation to be good*
> For the inferential explanation "E because of A, B, C, . . ." to be good, all the following must hold:
>
> E is plausible.
>
> A, B, C, . . . answer the right question.
>
> Each of A, B, C, . . . is plausible, but at least one of them is not more plausible than E.
>
> The inference "A, B, C, . . . therefore E" is valid or strong, possibly with respect to some other plausible claims.
>
> The explanation is not circular.

A causal explanation is one in which the inference is meant to establish a causal claim. With examples I motivate the following.

> The *fallacy of inference to the best explanation* is to argue that because some claims constitute the best explanation we have, they're therefore true.

Reasoning in Science and Mathematics

Advanced Reasoning Forum, 2012..

- Background: Claims, Inferences, Arguments, Explanations
- Models and Theories

This is the essay "On Models and Theories, with Applications to Economics" annotated above (p. 36).

- Experiments

In this essay I present seven examples of experiments to make clearer what we mean by "observational claim", "evidence", "replication of an experiment", and how experiments can be used as the basis of correlations for causal reasoning.

- Mathematics as the Art of Abstraction

This is the essay "Mathematics as the Art of Abstraction" annotated above (p. 36). By placing it in this volume I hoped to make clearer how reasoning in mathematics is similar to reasoning in the sciences.

The Fundamentals of Argument Analysis
Advanced Reasoning Forum, 2013.

From the Preface:

> I hope in this book to give a clearer idea of how to reason well, setting out methods of evaluation that are motivated in terms of our abilities and interests. At the ground of our reasoning, though, are metaphysical assumptions, too basic and too much needed in our reasoning for us to try to reason to them. But we can try to uncover those metaphysical assumptions to see how they are important and what depends on them, as I do throughout this volume.

- Arguments

Here I present a full discussion of the definitions and methods of *Critical Thinking*, comparing, in endnotes, my analyses with those of others, from Aristotle to recent authors on critical thinking. In doing so, I take reasoning (and hence logic) as a human activity. Here is the summary at the end.

> We first considered what things are true or false: claims. Then we looked at the idea of one claim following from one or more other claims: inferences. Then we focused on inferences that are intended to show that the conclusion is true: arguments.
>
> A good argument is one that gives good reason to believe that its conclusion is true. So a good argument must have plausible premises that are more plausible than the conclusion. And the conclusion must follow from the premises.
>
> Two standards are proposed for when the conclusion of an argument follows from the premises. If an argument is valid then the conclusion definitely follows. If an argument is strong, it would seem that the conclusion follows, too, but some dispute that. Considering examples and the reasons for doubt, we saw that if strong arguments are not good by some impersonal standard, then they are nonetheless good enough for reasoning in our ordinary lives.
>
> Many arguments we encounter are not good as stated but can be repaired to be good. Clear standards can be given for how to repair an argument and when we can classify an argument as unrepairable, based on assumptions we make about those with whom we reason.
>
> Good reason to believe, not "knowledge," is the issue. Perhaps we have knowledge but are not ever certain that we do. But we can determine whether we have good reason to believe. Perhaps only God, or the gods, or Dog who

> smells all have knowledge, can see the true nature of things. We should strive to know as He, She, or They do. But that is a goal, and we might never even know we have attained it.
>
> We can treat all our "knowledge" as provisional. That is only to accept that we could be wrong, admitting our fallibility, our human limitations. But it would not seem to have any significance in our daily lives other than making us more willing to revise our opinions. Looking for good reason to believe rather than knowledge is acceptance of humility and our human limitations.

Perhaps noteworthy is the definition of "claim", which I adapted from Buridan:

> A claim is a written or uttered piece of language that we agree to view as being either true or false but not both.
>
> Speech can include not only regular parts of spoken languages, but also parts of signed languages, as well as gestures and interjections, though we'll focus here on ordinary language speech, and in particular sentences. It is not a sentence type or an inscription devoid of context that is a claim. A claim is a specific piece of language in a specific context.

I continue the view from *Propositional Logics* (annotated above, p. 13) that agreements are at the basis of reasoning:

> The word "agree" suggests that it is a matter of convention whether we take a sentence to be a claim. But almost all our conventions, agreements, assumptions are implicit. Our agreements may be due to many different reasons or causes, compatible with many different assumptions about the nature of the world.

Possibilities, too, are placed within human capabilities:

> Some consider possibilities to be abstract, but for our purposes it's enough to consider descriptions, for it's through those that possibilities enter into our evaluating reasoning. A description is a collection of claims: we suppose that this, and that, and this might all be true at the same time. We do not require that we give a complete description of the world, for no one is capable of presenting such a description nor understanding one if presented.

The evaluation of an argument as strong depends on what we believe:

> Typically, though, the scale from strong to weak is not so completely relative to a particular person that there is no hope we can agree on the strength of arguments. Suppose we disagree. I find a particular argument strong, and you find it weak. If we wish to reason together, you should describe to me a way the premises could be true and conclusion false that you think is not unlikely. That may depend on knowledge you have of how the premises could be true which I do not have, but once you've made that explicit we can agree or disagree that there is such a possibility. The only issue, then, would be whether we agree that the possibility is likely. Sometimes we can't agree on a determination, but further examination will leave us with a clearer understanding of what our differences in evaluation are based on more than just whim. When the beliefs involved in determining the strength of an argument are made explicit, determining the argument to be strong or weak is far more likely to be a shared judgment. This is the same observation we made about the plausibility of claims, and it must be, since the strength of an argument can be reduced to consideration of the plausibility of claims describing possible ways the world could be.

Then the Principle of Rational Discussion is introduced—replacing a principle of charity—and from that the methods Repairing Arguments and Unrepairable Arguments , as described in the annotation to *Critical Thinking* (p. 29 above).

- Fallacies

I survey definitions of "fallacy" and show that most are attempts to classify arguments as bad without giving criteria or standards for what counts as a good argument. Then I define:

> A *fallacy* is a bad argument of one of the types that have been agreed to be so bad as to be unrepairable.
>
> By "agreed" I mean that the community of reasoners has explicitly pointed out the type.
>
> A *fallacy type* is a scheme or description of potentially infinitely many arguments. Not every argument that fits into a fallacy type need be bad, as I show in the examples below. A fallacy is an argument that fits at least one fallacy type and is actually bad.
>
> Fallacies can be classified into three broad categories according to the ways in which they are bad arguments.
>
> *Structural fallacies* The argument has one of the forms of a bad argument type, relative to the (possibly implicit) logic we adopt.
>
> *Content fallacies* The argument uses or requires via the Guide to Repairing Arguments a particular kind of premise that is typically implausible.
>
> *Violations of the rules of rational discussion*

- Induction and Deduction

The abstract:

> No current definitions of the words "induction" and "deduction" divide arguments according to what we think those words should mean and how we want to use them. Those terms are poor substitutes for a theory of how to evaluate arguments, useful only as a marker for whether we should judge an argument as valid/invalid or on the scale from strong to weak.

Some instructors told me that the presentation of this analysis in the Instructor's Manual to *Critical Thinking* relieved them of trying to use a distinction that makes no sense in argument analysis.

- Base Claims

The abstract:

> Unless we accept some claims without reasoning to them, we can have no good reason to believe any claim. In this paper we'll consider what counts as good reason to believe a claim without reasoning.

- Analogies

From the conclusion:

> Analogies, though usually only sketches for arguments, are nonetheless part of the general method we use in all our reasoning: consider two or more situations, or things, or masses, or relations, or processes to be similar and generalize or draw similar conclusions. The general claims that justify such reasoning may be about

the nature of reasoning or particular to the argument at hand. In the end, we recognize, or see, or perhaps just agree on similarities, abstract from those, and justify our reasoning. Analogies are the basis, if a very informal basis, the first step in seeing how we can do that.

- Subjective Claims with Fred Kroon and William S. Robinson

In this essay we give a definition of "subjective claim" that can serve to allow for evaluating reasoning about the mental life of others. We first survey definitions of "subjective" and "subjectivity" proposed by others and find them inadequate for that purpose. Then we define:

> A claim is in *subjective form* if it is of the form:
> [Someone, or some people, or some thing(s)] thinks/believes/feels/wants . . .
>
> A *simple subjective claim* is a claim that is in subjective form or is equivalent to one in subjective form.
>
> By focusing on the linguistic standard and then on the equivalences we propose or adopt, we can arrive at some standard for classifying claims that is clear enough for many of us to use together. It will not be one that is permanently fixed but is always open to adding or deleting subjective terms. In the process of deciding what words or phrases go on our list we will have a clearer idea of our differences about what constitutes a mental life.

Through a series of examples we refine this definition, show how it can be used to classify claims, introduce the notion of intersubjective claim, compare it to the use of personal standards, and relate it to the use of judgment in evaluating claims as true or false.

We then investigate whether there can be a good argument in which all the premises are subjective and whose conclusion is objective, and whether there can be a good argument in which all the premises are objective and conclusion subjective. In doing so ,we show that much of what counts as good reasoning about the mental life of others depends on what base metaphysical claims we assume.

This work was stimulated by the difficulty of finding a definition of "subjective claim" that could be used in my *Critical Thinking*. It became a true collaboration, developed over months, with many disagreements among Fred Kroon, Bill Robinson and me, some of which we resolved together and some which we set out in the essay as suggestions for debate.

- Generalizing

The abstract:

> To generalize is to make an argument from premises about a part to a conclusion about the whole. How do we evaluate such arguments?
>
> After defining what we mean by a generalization, we'll look at the standard method for evaluating generalizations, which we'll refine through a series of examples. Then we'll turn to the question of whether a generalization can be a good argument.
>
> Though a mathematical theory is often invoked to judge the evidence for a generalization, the details of the mathematics are inessential to the basic ideas of the standard approach.

Here, as in the essay "Probabilities" in this volume, my training in mathematics allowed me not to be awed by or enmeshed in details of mathematical analyses but to look for the assumptions that govern the mathematics.

- Probabilities

I survey the three standard approaches to probability: the frequency view, the subjective degree of belief view, and the logical relation view. I then show that none of them can replace our informal analyses of plausibility of claims and the strength of an argument. In doing so I suggest a new definition of probability, similar to that of Keynes.

I wrote this a long time ago. Reading it now, it sure seems hard, trying to make clear what is concealed in mathematical theories of probabilities.

- Rationality

The abstract:

> Except for a clear minimal notion of rationality, the use of that term is too vague to be helpful and can be replaced with other common terms that are clearer. Generally, the ascription of rationality or irrationality is a value judgment and not a tool of analysis.

After an extensive survey of discussions of rationality I present what I take to be a minimal notion of rationality that can be used in argument analysis:

> *The Mark of Irrationality* If someone recognizes that an argument is good, then it is irrational for him or her not to believe the conclusion is true.

Then I introduce the Principle of Rational Discussion (see the annotation for *Critical Thinking* p. 27 above) as a way rationality can enter into the analysis of someone's reasoning.

Then I propose a standard for classifying behavior directed towards a goal as irrational;

> *The Mark of Irrationality for Prescriptions* A person is irrational to believe a prescription and to act consciously in a way that he or she knows is incompatible with it.

In an appendix I show that it is not helpful to classify people acting emotionally or having certain emotions as irrational.

The Conclusion:

> I, too, would like to label some people I know as irrational based on what they do. After all, I am quite rational, and it always surprises me how irrational others are and how often I have to say to them "Be rational: agree with me." But to differentiate that from simply labeling them "stupid" or "bad reasoner" (or just not liking what they do), I should be able to state: 1. A minimum level of knowledge, 2. The norms of reasoning that I accept, and 3. Rules for how to infer not only beliefs but what forms of reasoning a person is using based on what he or she does. That seems very hard. And in the end, the label "rational" or "irrational" seems to add no more to such an analysis than a value judgment.
>
> This is not to deny the importance of our daily attempts to understand others by ascribing beliefs to them. And often enough we have practical success. But nothing in that success requires or is made clearer by adding the label "rational."

The history of this work:

> "Rationality" was the keynote address of the first meeting of the Advanced Reasoning Forum. It was published as an appendix to my *Five Ways of Saying "Therefore."* A discussion of the notion of rationality for actions has been added, and the notes have been expanded to include comparisons to more views of rationality. It appears also in *Prescriptive Reasoning*.

Prescriptive Reasoning

Advanced Reasoning Forum, 2013.

From the Preface

> The topic of this volume is prescriptive reasoning. Descriptive claims say how the world is, was, or will be; prescriptive claims say how the world should be. We have fairly clear rules for reasoning with descriptive claims. The goal of the first essay, "Reasoning with Prescriptive Claims," is to clarify how to reason with prescriptive ones. The first step in doing so is to justify viewing prescriptions as true or false.
>
> That justification is part of a general approach to reasoning in which many kinds of evaluations are taken to be true/false divisions. That view has been implicit if not explicit in analyses of reasoning from formal logic through argument analysis. In "Truth and Reasoning" I set out reasons for adopting that methodology.
>
> Theories, too, seem to be descriptive or prescriptive. Some say how the world is, others how the world should be. Yet as I show in "Prescriptive Theories?", on close examination the distinction evaporates. Unless, that is, one says that certain theories about values use an entirely different notion of truth than is used in science and is codified in our usual methods of reasoning. Absent that, there seems to be no justification for constructing and evaluating differently what are typically thought of as prescriptive theories.
>
> Many discussions of how to evaluate prescriptive claims are given in terms of what is rational or irrational to do. In the final essay, "Rationality," I look at what we mean by the idea of someone being rational and show the limitations of that label in evaluating reasoning or actions.

The essay on prescriptive claims and on prescriptive theories are the most difficult to read of any I have written. The material is complicated, with many branchings and much commentary, and is almost entirely new, as you can see in the citations and quotations from others that are included in it.

I don't remember writing this book—it's been a long time. The motive , I suspect, was to give some way to reason with the prescriptive claims that students put into their arguments in critical thinking classes, which began with the third edition of *Critical Thinking*.

- Reasoning with Prescriptive Claims

A far as I know, this is the first attempt to give a systematic analysis of how to reason with prescriptive claims. (I discount what is called "deontic logic" which treats "it is obligatory that" in the same manner as "it is necessary that" in formal modal logic for, as I show in an appendix to this paper, that development is incoherent when it is not simply wrong.)

After justifying why we can treat prescriptions as claims, I introduce two approaches to reasoning with prescriptions.

In the standards approach we take certain general prescriptions as basic, and from those we infer particular prescriptions. Thus from "You should not torture puppies" we conclude "Zeke should stop kicking this puppy." How we judge an inference from standards to particular prescriptions is so different from how we evaluate inferences in other reasoning (argument analysis, cause and effect, conditionals, explanations, mathematical proofs), that it constitutes a sixth way of saying "therefore".

The other approach to reasoning with prescriptive claims is to understand a prescription as advice for how to achieve some aim. This is how we reason with should-claims in our ordinary lives, concerned with not just large moral judgements but whether "You should close the window" is correct/apt/true. In figuring out what counts as a good inference with this method, we have to distinguish between goals that are subjective (personal), intersubjective (social), and objective (assuming there are any), and then consider means for achieving those goals, either subjective, intersubjective, or objective. I work through a lot of examples, some of which are ethical, some practical, and some both.

> Some economists use an absolute notion of rationality that surpasses human abilities, requiring a rational person to be one who considers all aims and all ways of achieving them and all the consequences of acting according to those options with no circular preferences. Others hold a similar view but considering "satisficing," that is achieving the aim, or more or less achieving the aim, rather than maximizing, which would be achieving the aim in the best possible way.
>
> Why then bother with all this talk of the truth-conditions for "should"-claims? Why not just refer to the literature on rationality as the term is used in that sense?
>
> The analysis of prescriptive claims in terms of aims provides a framework for all those views. Each is a way to flesh out the general framework of the truth-conditions for a "should"-claim, stipulating that personal, interpersonal, or impersonal standards will be the only ones considered. The general framework allows us to make comparisons across the various viewpoints and assimilate discussions in ethics and meta-ethics to such an analysis.

There is no through-line here, no final conclusion that this or that is the right way to reason with prescriptive claims. Each of these two approaches branches according to what basic assumptions or aims we posit and whether we take into account the abilities of a person or people generally and whether they know the basic prescriptions or good aims, or whether the prescription is entirely impersonal. Moreover, there is no clear way to go from justifying a prescription according to the standards approach to justifying it on the aims approach or vice-versa.

> This concludes my attempt to understand better how to reason with prescriptive claims. We have two approaches, each sufficiently general to accommodate many different metaphysics, and we have some idea of how those approaches relate. But even if we understand these quite well, we will understand better only how to reason about what should be done. What should be done—what are our most fundamental prescriptions and aims—is a question beyond logic.

- Truth and Reasoning

The abstract

> A major goal of reasoning is to establish truths and to determine what would follow if certain assumptions are true. There are many different notions of what is true, both in what kinds of things are true or false and what makes them true or false. By looking at what is common to those, we can find an idea of truth and the things that are true that can accommodate many particular views of truth and account for the wide agreement on what counts as good reasoning.

- Prescriptive Theories?

The abstract

> What is the difference between descriptive theories and prescriptive theories? Unless we assume that prescriptive theories are about value-judgments that are not true or false, and hence adopt a new method and justification for our reasoning, there seems to be no difference that would affect how we construct and evaluate theories.

This extends the work in "Models and Theories" annotated above (pp. 39–40). I also discuss other views of models and theories and present as a case study how the idea of "reflective equilibrium" for constructing and evaluating theories is nonsense.

- Rationality

This is the same essay as in *The Fundamentals of Argument Analysis*, annotated above p. 44.

Reasoning and Formal Logic

Advanced Reasoning Forum, 2015.

When Henrique Antunes Almeida came as an ARF Student Fellow at Dogshine (see p. 68 below), I was trying to bring together papers I had already published along with some new essays to place formal logic in the view of logic as the art of reasoning well. Henrique's close reading and deep analyses contributed so much that I dedicated the book to him.

- Valid Inferences and Possibilities

This is the same essay as "Valid Inferences" annotated above (p. 37).

- A General Framework for Semantics for Propositional Logics

This is a summary of the view of logics as a spectrum as given in *Propositional Logics* annotated above (p. 13).

- Why Are There So Many Logics?

The abstract:

> If logic is the right way to reason, why are there so many logics?
>
> The choice of a logic depends on what we pay attention to in our reasoning. Calling one logic right and another wrong often arises from judging one by the background assumptions of the other. Necessity in our reasoning, if there is any, is in the common background for all logics.

This presents the view of logics as depending on what we pay attention to in our reasoning set out in Chapter XI ,"The Semantic Foundations of Logic", in *Propositional Logics*, annotated above (p. 13).

- Truth and Reasoning

This is the same essay as "Truth and Reasoning" in *Prescriptive Reasoning*, annotated above (p. 47).

- On Translations

In *Classical Mathematical Logic* (annotated above, p. 23) I presented a theory of how to translate between formal theories. Here I describe that and relate it to how we might judge translations between ordinary languages.

- Reflections on Temporal and Modal Logic

This is the same essay annotated above (p. 17).

- The Timelessness of Classical Predicate Logic

The concluding paragraph:

> Classical predicate logic is useful for formalizing reasoning about things outside of time, or about essential attributes or permanent capacities of things that are in time. We need to incorporate temporal aspects of propositions into the semantics of classical predicate logic and ways of talking about that into the syntax in order to reason about things in time.

- Events in the Metaphysics of Predicate Logic

For years I inveighed against talk by linguists and philosophers of events as things. Here I observe that if we are to think of events as things, then we should be able to reason about them in predicate logic. But we can't, and I show further that motives given by others for treating events as things are at best confused.

A more comprehensive presentation of problems with event-talk, without considering formal logic, is " Why Event-Talk Is a Problem" in *Language and the World*, annotated below (p. 56).

- Categoricity with Minimal Metaphysics

The abstract:

> Contrary to the views of many logicians, a categorical finite characterization of the natural numbers can be given in which no infinitary assumption nor assumption about the nature of collections is required beyond what are used in first-order logic. This can be accomplished with an extension of first-order logic in which quantification over names is allowed and in which a formalization can be given of "Every object has a name."

When I tried to get this paper published, I was told that I had misinterpreted the people I quoted who didn't *really* say that infinitistic assumptions were needed to give a categorical axiomatization of arithmetic as in second-order logic or a somewhat weaker formalism. (I wish I could find the referee's report.)

- Reflections on Gödel's Theorems

Again and again I heard how great a logician Gödel was. But that was by the standards of modern formal logicians, the kind I knew at Berkeley, whose work I considered and have shown is very limited. Here I set out the limitations of Gödel's work., based as it is on a platonist conception of mathematics,

> . . . the view that logic and mathematical intuition are divorced from people who reason.

I conclude:

> One final thought about Gödel's work. Krajewski says:
>
> > [Gödel is] widely seen as "the greatest logician since Aristotle."
>
> Gödel distinguished some conceptions that were confused together: provability and truth. He showed us that the ancient hope of a universal machine to calculate all truths is not attainable. But his few philosophical writings are only metaphors for his platonism, not especially distinguished in the history of analyses of platonism. He did no systematic work on the nature of logic. He never worked on the areas of logic covered in this series of books, such as reasoning about cause and effect, explanations, and generalizing, which form a major part of our understanding of how to reason well. Even as a formal logician, it is not clear his work is superior to that of John Buridan. The evaluation in the quote is a symptom of identifying logic with classical mathematical predicate logic and various offshoots of that subject, denying any conception of logic as the art of reasoning well.
>
> It is one thing to comment on Gödel's platonism. It is quite another to assume platonism as the basic understanding of mathematics and evaluate Gödel's work by that standard only.

- On the Error in Frege's Proof that Names Denote

The abstract:

> An examination of Gottlob Frege's proof that names in his system denote illuminates the nature of induction proofs in formal logic today as well as the importance of keeping the formation rules for the syntax distinct from the semantics of the formal language.

My last year in Ames, Iowa I was worried about the nature of logic, looking for a basis for all the propositional logics I was studying and devising. I read Frege, and I was under his spell. Like Freud, reading him I was carried away with his conception—of logic by Frege, of life of the mind by Freud. Only later did I outgrow them, seeing how distorted and narrow they were. But at that time I wanted to talk with someone about Frege, to study, to learn more of philosophy. I took a year off from Iowa State University and went to Berkeley, though there were other motives, too.

At Berkeley I met Hans Sluga, a Frege scholar. I had figured out what went wrong in Frege's proof that names denote, which if it had been successful would have shown that his system was consistent. In this paper I present that analysis, taking it as a cautionary tale about the importance of keeping formation rules in a formal language distinct from the semantics, whether formal or informal, which has served me well over the years in my work.

When I showed a draft of this to Sluga, he said that it was well-known where Frege went wrong in his proof that names denote. But I never saw elsewhere the analysis of the problem as the way Frege mixed syntax and semantics.

- *Postscript*: Logic as the Art of Reasoning Well

This is it. A summary. It describes how I understand and develop analyses of logic and how to reason well and my other studies and writings. I conclude:

> We can restrict ourselves to only what is (seems to be) certain, what is (seems to be) independent of our human judgments and errors, and build a bulwark against the less than perfect in our lives. Or we can embrace the imprecision, the uncertainty, and try to reach out to each other to make agreements that can guide us in our reasoning and lives.
>
> Come, let us reason together.

Compare this to the quote from *The Internal Structure of Predicates and Names* above (p. 25).

Gestures

In 1988,living in Berkeley, trying to find a publisher for *Propositional Logics*, without a job., I began teaching English to foreign students at a small language school in downtown Berkeley. I reckoned I could learn a lot about language, and it would keep me occupied. It was great fun and challenging. Later, as I describe in the Preface to *Conventional Gestures* (annotated below):

> In 1992 I was teaching English as a second language to foreign students in Cedar City, Utah. One day a Japanese student raised his hand to be called on—with his middle finger extended. I told him it wasn't a good idea to do that. He was puzzled. I explained that it was an obscenity, a direct challenge, and if he did it to someone on the street he might get his finger broken. He was glad to know. And I realized that my students couldn't recognize our most common gestures.
>
> So I set out to make a list of those. I asked the other teachers at the school for their suggestions, giving them an idea of what I was looking for with a few examples. I compiled a list of 65 gestures. I described the movement of each gesture in words, in some cases with a little diagram, and gave a short explanation of the meaning of the gesture. This was adequate for use in our classes since we all knew the gestures. The other teachers were enthusiastic about the project, but there wasn't much more I could do because I couldn't illustrate the gestures.
>
> In 1998 I was writing a textbook on critical thinking and was looking for a cartoonist to provide some illustrations. I met Alex Raffi, and we were able to develop over one hundred cartoons for that book. At the end of the project in 1999, I suggested to him that we work together to illustrate a book of common American gestures for students of English and for travelers to the United States.
>
> It took a while for us to decide how we would illustrate the gestures. We needed to show the movement, but we quickly realized that without a context, the use and meaning of the gesture are unclear. So for each gesture we provided a context cartoon as well as a close-up illustration of just the movement. To that we added a telegraphic explanation of its meaning, just enough for a classroom or a tourist.
>
> By observing and discussing with friends and colleagues, we compiled an illustrated draft with about 115 gestures. We showed that to acquaintances in the U.S. and in other countries. They suggested a few additional gestures, and the readers abroad commented on whether the illustrations were clear enough for them to recognize and duplicate. We had a couple offers at that time to publish it as a textbook for English-language classes, but we chose not to do so as no publisher was interested in marketing it to tourists as well.
>
> Over the next several years I began to read more about gestures. I discussed gestures with colleagues in linguistics and philosophy. Alex Raffi and I asked our friends about gestures. We looked at other collections of gestures. We talked with Carolyn Kernberger about how women gesture in the United States. We began to observe more carefully people in daily life and in movies and television, looking for gestures we had missed. By 2003 we had a draft of this essay and an additional 120 gestures.
>
> Still I felt there was much more to learn and puzzle out. I continued to read and to discuss the issues with Alex Raffi. I began talking with linguists at the University of New Mexico. But for the most part we put aside this project while I was writing books on logic and critical thinking and he was starting up a marketing company of which he was the artistic director.

We returned to the project in earnest in 2010. Working together, we completed a new draft of the gestuary with about 340 gestures, though less than half were illustrated. We found as we began compiling, indexing, and cross-referencing that we were less sure of what to include. Though we had begun with a clear idea of what we were trying to illustrate, namely, common American gestures, in order to decide what to include and to distinguish what we were studying from many other ideas of gesture, we had to be clearer about the criteria we had implicitly adopted. We also realized that to make general claims about gestures and to expand on the ideas we needed not just a list but illustrations for all of the gestures. We returned to our work, and now we have over 400 gestures in the gestuary.

We would like to complete that project, but we don't have the money. So we have decided to publish separately this book, which was meant as an introductory essay for the gestuary. Though Alex Raffi is an equal partner in making the gestuary, this work is principally by me, reflecting my concerns about meaning and methodology, and he should not be held responsible for any inaccuracies or mistakes here. We have made available the current draft of the gestuary on the website of the Advanced Reasoning Forum <www.AdvancedReasoningForum.org/gestures> so that the gestures whose names appear in italics in this text can be seen. We also intend to update there the Annotated Bibliography of this book as we receive information about new collections of gestures.

American Gestures

with Alex Raffi, Advanced Reasoning Forum, 2021.

This is the short collection of gestures for students and teachers described directly above.

Conventional Gestures: Meaning and Methodology

Illustrated by Alex Raffi. Advanced Reasoning Forum, 2014.

This is meant to be read in conjunction with *An American Gestuary* annotated next.

From the Preface:

> We begin here with an attempt to give explicit criteria for what we've included in the gestuary. Then we discuss how others have studied similar classes of gestures, comparing their methodology for compiling collections of gestures. With that as background we try to understand what and how a gesture means. Providing some categories of gestures leads to a better idea of the scope of our inquiry. After considering whether there are any universal gestures, and how gestures change over time, we discuss the difficulties in organizing a gestuary. We conclude with an annotated bibliography of collections of gestures that extends many of the discussions in the text.

The annotated bibliography comprises more than half of this book.

An American Gestuary with Alex Raffi

Illustrated dictionary of gestures, 2013. Draft at www.AdvancedReasoningForum.org.

This is described directly above.

The World as the Flow of All

See the annotation (p. 24 above) for *The Internal Structure of Predicates and Names* and *Time and Space in Formal Logic* for the genesis of these studies.

When I was developing this work, I came across *Language and Art in the Navajo Universe* by Gary Witherspoon. He was struggling to explain how the Diné (Navajo) had a world-view much like what I was seeing in the world as process, the flow of all. Later I read the work of Benjamin Lee Whorf in the posthumously edited *Language, Thought, Reality* and was struck to find that other language cultures had a similar view. That was a book I had read many years previously, been taken by it, then forgot about. Only in the reading now did I find that his essays has been mangled by the editor and were much better in the originals. Following up on his work, reading studies by Dorothy Demetracopoulou Lee and others, I began to have a much richer understanding of how people do view the world as the flow of all. Then I discovered in 2015 Chad Hansen's *Logic and Language in Ancient China* and found that the ancient Chinese language-culture was also based on the view of the world as the flow of all: no nouns, no verbs, no adjectives, no adverbs, only base words. The difference between Chinese and American Indian languages is that the speakers of the latter add to the base words with prefixes, infixes, and suffixes to make a single "word" that we regard as a sentence, whereas Chinese only juxtaposes the base words. Finding that Chad Hansen's book was out of print, I wrote to him and offered to reprint it in the ARF Classic Reprints series (see p. 68 below), which we did. We began a correspondence that helped me a great deal, and I hope helped him. Later, in 2019, I came across the work of Vera da Silva Sinha on native languages in the Amazon, and I wrote to her. So began a continuing exchange with her and her husband Chris Sinha, who has written about how many other languages are based on the view of the world as the flow of all. He especially encouraged me to see my studies as the natural working out of the ideas of Benjamin Lee Whorf, who died so young.

As well, I learned much from folks in the Linguistics Department at the University of New Mexico, particularly Melissa Axelrod and Sherman Wilcox, where I was allowed to give several talks on my research on the world as the flow of all and on conventional gestures (p. 51 above).

The World as Process

ETC: A Review of General Semantics, vol. 73, no. 3, pp. 213–232, 2016 (published 2018).

This is the first publication of my view of the world as the flow of all, though I talk here of the world as process. I found that this led some people to think that I was working on what was called process metaphysics of Whitehead and Russell, which I explain in this paper is quite different from what I am doing.

I chose this journal because Dorothy Lee and others who I respected and whose work was in line with this published in it. But that was long ago, and by the time I submitted to it, it was in disarray to the extent that this paper was published in the 2016 issue which came out in 2018. And few people were reading it, certainly not the audience I wanted. Since the editor liked the paper, I sent him my poems in the style of the Chinese to look at; then without my knowledge or consent he included

them in the same issue as this paper, but treating them as one long poem. I argued with the editor and the publisher and finally got a retraction for them.

Still, this is a good introduction and overview of what is in the next two books.

Language and the World: Essays New and Old

Advanced Reasoning Forum, 2021.

The Preface:

> Language and the world. A big subject. The structure of languages, metaphysics, knowing and wondering, things and mass and process, how to reason well, thought, ethics. All these and more are involved in understanding how we encounter the world with our languages.
>
> The first three essays, "The World as the Flow of All", "Language and the World", and "Language-Thought-Meaning", set out the overall perspective. The other essays extend, or contradict, or support the ideas in these first three, leading to a large view of how we talk and understand, and how that affects how we live.
>
> In part, this work is an exploration of the idea that language shapes how we encounter the world. I do not attempt to trace the history of "Whorf's Thesis", the use and misuse of that term, and the many ideas of what it's thought to be, for that is ably done by John Leavitt in *Linguistic Relativities* (Cambridge, 2011) and Penny Lee in *The Whorf Theory Complex* (John Benjamins Publishing Company, 1996).
>
> This book is an exploration in essays by me and others as we try to understand, and to understand how we understand, an exploration leading, I hope, to less certainty and more wonder.

My essays in this volume:

- The World as the Flow of All

This is the same as "The World as Process" annotated directly above (p. 53) but only up to the discussion section of that paper.

- Language and the World

The Introduction:

> There are two kinds of languages: thing languages and mass-process languages.
>
> In a thing language, the grammar leads speakers to look first for stability in the world: the world is made up of things, individual things that persist in time. Words that can be used to pick out that stability are nouns. Descriptions of the individual things in time are verbs. There may be words for mass and process in such a language, but they are secondary, and the grammar forces their use into the syntactic role of nouns and verbs, leading speakers to think of them in some way as things and as descriptions of things in time.
>
> In a mass-process language, the grammar leads speakers to encounter the world as the flow of all. There is no idea of change, for there is nothing to change, there are only differing descriptions of the flow. Every base word can serve as a description and as a modifier. Each can be marked for time, or whole assertions can be marked for time, or assertions can be compared for time as before or after. If stability can be found it is only with secondary grammatical constructions. There are no nouns and verbs, for there are no words for individual things and no descriptions of things in time.

> There is good reason for a noun-verb distinction in thing languages. There is good reason for no noun-verb distinction in mass-process languages. This is what I will show in this paper, along with how linguists and anthropologists do or do not take account of such very different grammars.

By way of summary for this long paper, I include its table of contents:

- Language-Thought-Meaning

From the Apology at the beginning:

> For many years I have been thinking about logic. I've been writing, trying to understand, and I've been putting the human back into logic. To me it's not some formal game, nor a study of abstract things, but a serious project to give us guides for how to reason well. We need to reason well in our ordinary lives. We need to reason well for our deepest worries and fears, which include our worries about the way the world is. Doing so I have had to relate language and how we mean to reasoning and how we give rules for reasoning. Now it's time for me to try to write up a summary of my ideas, ideas that I've developed here and there throughout my work, a little piece made explicit in one place, used and lightly commented on in another.

Perhaps most salient here is my view of language as embodied, language in us not apart from us, which I was encouraged to find was held by some linguists I talked

with at the University of New Mexico. particularly Sherman Wilcox and Melissa Axelrod.

- Why Event-Talk Is a Problem

I show here that event talk is an attempt to parcel out the world into parts which are taken to be things. But we have no way to distinguish events, no way even to point to an event, except with a linguistic proposition, the event being what in the world makes the proposition true. But I show that is not coherent, for it's not part of the world that makes a proposition true but the world, all the world. What event-talk is meant to clarify only obscures, and can be better investigated by talking of the linguistic propositions that the events are supposed to make true.

This is my best explanation of why event-talk is a problem and not a solution. It is entirely non-technical. (Compare the annotation of "Events in the Metaphysics of Predicate Logic" p. 48 above.)

- On the Genesis of the Concept of Object in Children

> The work of Jean Piaget in *The Construction of Reality in the Child* illustrates how the strength of the conception of the world as made up of things colors and distorts research.

- A New Turing Test

> Alan Turing in 1950 proposed a test to answer the question "Can a machine think?" Roughly: There is one person who is the tester. In separate rooms there is another person and a machine. The only links are between the tester and the other person, and between the tester and the machine, and those links are via typed responses only. The tester puts questions to the other person and the machine, not knowing which is which. If in 5 minutes the tester cannot determine which is the machine, then, it's claimed, the machine can think.

I show that if the Turing test were apt, then it could equally be used to answer "Can women think?", "Can African- Americans think?" "Can Native Americans think?" "Can Hispanics think?" . Stating these questions lets us see how we build into our test the assumptions about cognition that serve us, unable to see our own bias, testing only whether the other thinks as we do. I cite others who discuss much the same problem in collecting data about cognition of people from other cultures.

- The Thing-Basis of Western Philosophy

The Abstract:

> Almost all philosophers in the Western tradition tie their work to the thing-view of the languages they speak. By contrasting such thing-based analyses with the metaphysics of the flow of all, we can gain a better understanding of issues in Western philosophy and the solutions that have been proposed.

- The Metaphysical Basis of Logic: Things and Masses

This expands and improves the article "The Metaphysical Basis of Logic" annotated above (p. 22). I explain how our conception of mass and process is so different from our conception of individual things that reasoning about mass and process requires a different logic than reasoning about things.

- Languages and Logics

  Here I set out how I see formal logics as based on the implicit metaphysics of ordinary languages, which must be respected in using the logic to formalize ordinary propositions and inferences with criteria of formalization. I use this to justify attempting to develop methods of reasoning in *Reasoning about the World as the Flow of All*, annotated directly below.

This book includes essays by the linguists Dorothy Demetracopoulou Lee and Benjamin Lee Whorf, who came to the view that I call "the world as the flow of all" in the 1930s and 1940s, with Whorf suggesting that a development of methods of reasoning for flow-of-all languages should be made. There is also an essay by the linguist M. Dale Kinkade who shows that the noun-verb distinction cannot apply to the American Indian language Salish. That led me to take a noun-verb distinction in a language as a criterion for a language being a thing-language.

I also include the essay " 'Reason' in Philosophy' " by Friedrich Nietzsche for a discussion of Heraclitus and appearance and reality. An extended quotation from *The Philosophy of Leibniz* by Benson Mates on metaphysics and linguistic relativity is included to show how at least one philosopher took seriously the challenge that Whorf's work posed to Western philosophy. I did not become aware of or at least did not appreciate this part of Mates' book when I read it with Mates in the 1980s. But surely it must have influenced me as I so closely track his conclusions in the development of my work.

Reasoning about the World as the Flow of All

Advanced Reasoning Forum, 2024.

The Preface:

> As speakers of English, German, or Romance languages it is hard for us to conceive of the world as flux, the flow of all, with no or only a quite secondary idea of individual things that persist through their changes. In *Language and the World: Essays New and Old* I've tried to make it possible for you to enter that way of encountering the world with essays by linguists and anthropologists who have described people who talk and live with that conception. That is important and useful background, but not essential, for I have set out the basic idea of the world as flow in the first two chapters. In this volume I hope to explore more clearly that conception by asking how we can reason in accord with it.
>
> This is an attempt, a first attempt as far as I know, to give a systematic analysis of how to reason that is not tied to our European languages, to step out of our language conceptions and habits. Consider then this work as a bridge, a chance for us as speakers of languages that focus primarily on the world as made up of things to begin to see the richness and complexity of encountering the world as the flow of all, the one and not many. The contrasts, often unsettling, can lead us to understand better how we encounter and reason about the world as made up of things.

This was meant to be Volume 3 of *Logic, Language, and the World* (see pp. 24–25 above). But it was too different from the first two volumes on the internal structure of predicates and names and on time and space in predicate logic, and none of the work in those volumes was needed as background. It took a long time for me to realize that, as I tried to use methods and analyses from modern predicate logic for reasoning about the world as the flow of all, each time going down a

rabbit-hole that I had to extricate myself from. Only the connectives of classical propositional logic and the notion of inference from Western logic were useful.

There was no work on reasoning about the world as the flow of all that I could consult as a guide. I often found myself falling into a thing-conception of the world as I went along, only to have to correct myself. Though I treat syntax and semantics separately, there is no division, no keeping those distinct, except to the extent of giving formation rules for the forms of claims that are to be analyzed. Not even a formal language. And when done, this book seemed only a sketch, an attempt that might stimulate others rather than a completed vision. In particular, I could not figure out how to incorporate talk of time and space in the language of talk of the flow of all that I devised, leaving a description of the problems of doing so as an appendix to challenge others.

Newspaper column

They're Not Like You and Me

Published weekly in *El Defensor Chieftain*, Socorro, NM, from July, 2006 to May, 2008, and again irregularly from February, 2013 to June, 2013.

Available now as a file on my computer and printed out with my papers.

These newspaper columns were part of my work, so long now, for peace. Since 2001 I and others have been on the Plaza in Socorro opposite the post office to demonstrate for peace, every Friday afternoon. Sometime in 2019 Harry Richardson began coming to our peace vigil, and in our regular talks we encourage each other in the way of DOG (see below) and better understand the world as the flow of all.

Translation and editing

The BARK of DOG

The BARK of DOG Foundation, 2013.

The history and genesis of this is given in the Introduction to it.

Of all my works, this is the one I hope will continue to be read.

Final Work

Last Writings

Advanced Reasoning Forum, to appear posthumously.

To lessen the burden for whoever will be in charge of my estate, I've collected here my unpublished writings that I think might be worth preserving . The when, where, and sometimes why of writing each part is given in the front matter of the book. Though I've given copies to a few friends, it is meant to be distributed only after my death so I can avoid hearing the charge of vanity.

Addiction studies

Teaching Addicts

This describes how I taught critical thinking and read from *The BARK of DOG* to inmates at the Socorro County Detention Center, suggesting a new approach to helping addicts and others.

Evaluating Treatments for Addiction

I show that there is no good/reliable objective measure for the quality of treatment programs for addiction.

Addiction in the Body—Too Good Is Just Awful

This is a draft of a comic book explaining how addiction works in the body and why it is so hard to quit an addiction.

Poems

Stories

Jewish Dogs

In a small town in Poland a man studying the Talmud encounters two Jewish dogs.

Youth

A story of yearning and hope.

Arfito to Uschi

When I was sick from chemotherapy, my dog Arfito took it upon himself to write to my dear friend Uschi.

Devil's Dictionary—an update

Short definitions that show how acerbic I can be.

Shakespeare and dogs

A refutation of the claim that Shakespeare didn't like dogs, quoting from his writings.

Plays

The Hanging Tree

A play about people who end up meeting at a tree at which each plans to hang himself or herself.

Princess

A short play about a man who sees a sign "If you've lost your dog inquire within" and hopes to find his dog.

Ralph

A man confronts a dog that barks at him on his evening walk. Audience participation required for this one-act play.

My Fight with the Alligators

A longer one-act play about two unemployed men who are hired to kill alligators in the sewers of New York.

Spiridon

How a man loses his way trying to cope with his wife having left him. A full length play in two acts.

Essays on logic and . . .

Three Questions about Logic

The abstract:

> Logic, whether formal or informal, is meant as a guide to reasoning. From that perspective arise three questions about the foundations of logic as the art of reasoning well: On what basis should we choose which logic to employ? On what basis do we decide between the descriptive and prescriptive aspects of a logic? What heuristics do we use in evaluating the strength of an argument?

Is There a Problem with Formal Semantics for Natural Languages?

The abstract:

> A close examination of a paper by Jeffrey Pelletier in which he offers formal semantics for mass nouns and count nouns raises the question of what justification there can be for using the methods of formal logic in the study of meaning in natural languages and what is the study of meaning.

Mechanical ≠ Computable

The abstract:

> The notions of computable and mechanical are often taken to be the same, but they are not. A criterion that distinguishes them has consequences for whether a person's behavior could be modeled by a computable procedure.

Intentions

The introduction:

> We take "Spot wants to play" to be about Spot and what he wants: a desire. We understand "Flo thinks that coyotes are dogs" to be about Flo and what she is thinking: a thought she has. We take "Suzy believes that Spot will bite Puff" to be about Suzy and what she is believing: a belief she has. It seems natural to us to talk of thoughts, beliefs, and feelings as things
>
> I will present here a different way to understand such sentences that invokes no mental objects, no talk of thoughts, beliefs, feelings, but rather ways of thinking, believing, feeling.*
>
> * This project arose in my work in *Time and Space in Formal Logic*. The presentation there is embedded in formal logic analyses of reasoning taking account of time. Here I hope to present the ideas and methods without appealing to formal logic while using examples only in the present tense.

Numbers?

I give lots of examples of counting, illustrated with color photographs, to show that numerals are adjectives, not nouns.

This essay supplements and extends the view that mathematics is created and developed in the same way as scientific theories, presented in "Mathematics as the Art of Abstraction" (annotated above, p. 36).

The Twenty-First or "Lost" Sophism on Self-Reference of John Buridan

The abstract:

> The discovery of the twenty-first or "lost" sophism of John Buridan on self-reference and the nature of wishes is recounted, and the sophism is translated.

The Procrastination Paradox

Self-reference can lead to a paradox without involving talk of what is true or false.

Education

University of Pennsylvania
B. A. in Mathematics, *summa cum laude*, 1969

University of California, Berkeley
M. A. in Mathematics 1971 (by examination)
Ph. D. in Mathematics 1973

Fellowships

Postdoctoral Fellow in Mathematics and Philosophy
1975–77 Victoria University of Wellington, New Zealand

U. S. National Academy of Sciences Exchange Scholar
1981 (January to May) Polish Academy of Sciences, Warsaw

Fulbright Fellow
1987 (February to July) Center for Logic and Epistemology,
University of Campinas, Brazil

CNPq Fellow
1991 (February to June) Philosophy Department, University of Paraíba, Brazil
(CNPq is the Brazilian government's research funding foundation.)

Academic Employment

Iowa State University
1978–1979 Lecturer in Mathematics
1979– 1981 Assistant Professor of Mathematics
1981–1982 Associate Professor of Mathematics with tenure

University of California, Berkeley
1983–1985 Visiting Associate Professor of Mathematics

California State University, Hayward
1985–1987 Temporary Associate Professor in Mathematics and Computer Science

San Jose State University
1988 Visiting Associate Professor in Mathematics and Computer Science

University of Auckland, New Zealand
1993 (June) Visiting Lecturer in Philosophy

University of Nevada, Las Vegas
1995–1998 Lecturer in Philosophy

Advanced Reasoning Forum
1999– Head

Addendum: Berkeley (1982–1989)

So I left Ames, Iowa on a leave of absence from Iowa State University to go to Berkeley to learn from the philosophers at the University of California, where I had gotten my Ph.D. in mathematics. I sat in on a course on Plato by Benson Mates, which was very strange since he considered himself in the line of logical positivists and thought that Plato's work was nonsense, so he just rambled on in the class. Still, I liked him and found that I learned from him and slowly got to know him.

I also sat in on at least one class with Charles Chihara on theories of truth (see p. 19 above) and perhaps one by him on philosophy of mathematics.

I introduced myself to the logicians at the Mathematics Department and said that I could teach if they needed someone to fill in. They were glad I was there because I was known for my work in recursive function theory (see pp. 3–6 above), which was the kind of research in mathematical logic they liked and respected. But I wasn't working on that anymore. Indeed, one day one of the logicians, I can't remember who, called me at home because he was trying to master the method of full approximation constructions from my writing and he had a question about how boundary strings worked. I couldn't recall offhand, so I said I'd call him back—I had to read what I had written. He called me back an hour later to say no need, he'd figured it out. As hard as that was, I was surprised, but maybe he did figure it out.

Then I was asked to teach introductory calculus to a big lecture class. Since I was an associate professor at Iowa State, they hired me as a temporary associate professor for one semester, with a salary more than at Iowa State. I was in charge of several teaching assistants. The person in charge of teaching in the department liked what I did, and asked me to teach second-year calculus to a smaller lecture class on very short notice for the second semester. I told him I'd fill in until they found someone else because I didn't know the material well enough to teach it.

The end of my first year back in Berkeley, I resigned from my position at Iowa State University. I couldn't face the severe allergies and severe winter cold there, and Berkeley was much better for my studies. But I still had allergies, and I went to a crazy doctor in the summer who was going to cure me of them by making me take a bit of a powder on my tongue every day to kill all the mold in me. By the third day I was very weak, couldn't sleep, sensitive to not only allergens but even smells. She sent me to another crazy doctor who sent me to an endocrinologist who was ready to take out my thyroid, though my blood tests showed no problem with it. A good friend was horrified and directed me to a good friend of his who was an internal medicine doctor. He immediately diagnosed what was wrong and put me on the anti-depressant Desyrel which cured me in one night. I remember him asking me when I went back a week later for a follow-up whether I had stayed up all night to see if the medicine would work to help me sleep—and of course I had. But that next morning I was strong enough to go for a walk for the first time in four months. I've been taking that medication, now the generic Trazodone, ever since, more than 40 years, every night.

That summer, I was asked to teach set theory at Berkeley, and I had barely strength enough to do that. Then for the fall semester, now cured, I was asked to teach introduction to calculus and also an upper-division course on real analysis. I knew real analysis well from my studies in London, but it was odd teaching that as it depended on assuming completed infinities, which I thought were somehow illegitimate.

When I was a post-doc at Wellington, I was asked to fill in teaching a large lecture course of second-year calculus half-way into the term. That semester the Education Department was working with various departments to improve teaching. One of the professors came to observe me teach and gave me advice. Then he filmed a class I gave. I met with him and he made comments on what to watch for in the film. Then he started the film and left. A darkened room, listening to me drone on and on, I fell asleep in my own lecture. I learned then that if you can't keep the students awake, you can't teach them. So after the first semester at Berkeley, when I was asked to teach the basic introduction to calculus course, a large lecture of almost 500 students, I decided to start the class dramatically. I had one good teaching assistant (whose name I don't remember) from the previous course to be my head teaching assistant, with eleven other teaching assistants (each taught 2 sections of about 20 students each). My head assistant found a couple students he knew from band, and I recruited a student I'd gotten to know from the real analysis class, and the first day of classes, in a big auditorium, when all the students were seated, the teaching assistants came in and said, "All rise, all rise". I entered from the back of the auditorium in cap and gown which I'd got on loan from the university. I walked in followed by my head assistant who was weighed down carrying an armload of books. We went to the front of the auditorium, mounted the stage, and he dropped the books on a table, while the band students played a fanfare., I stood before the podium and said, "Welcome to the study of the calculus. You may be seated." I then said "Mein namen ist herren Professor Doctor Epstein. My friends call me Dr. Epstein. You may call me Dr. Epstein." Then I gave the basic information for the course. Then I said, "You may come to my office anytime. I am not intimidating. I am not intimidating." And I emphasized that by hitting the podium with a yard-long wooden ruler which I had broken before and taped together so it broke in two with one piece flying when I hit the podium with it. Then the student from my last semester's real analysis course, who was sitting near the front and was wearing a red hat, started talking to a student sitting next to him. I said, "Quiet. No talking." He continued (as we had planned), so I said, "Teaching assistants take him out." They came and took him, with him protesting "But I'm registered for this class." After that, talking in class was never a problem—every professor's dream of how to handle a disruptive student. Then every class, about mid-way in the hour, when the students were having trouble staying alert, I told a story about George the duck who lived with me. He wanted to be a BDOC (Big Duck on Campus). I gave his end of the dialogue by playing a duck call. I can't remember the stories—all impromptu—but the students liked them, and were waiting for the break, which refreshed them for the last 20 minutes. One day, with a particularly complicated piece of mathematics to explain, I kept on going, and after the point where I would

usually give a story I could see the students getting restless. Where is the story? I continued. With only about five minutes left I finally told the George-the-Duck story.

With the midterm in the course, a lot of students failed. I was warned by the teaching assistants that the students were very upset. When I walked into the auditorium, there were more than a few boos. I went to the podium and waited for quiet, and said, "That was an easy exam. The first question was exercise (number) from chapter (number) with only the numbers changed, the second one . . ." and similarly for all the questions but the last which was harder, so I'd know who to give an A to. They were stunned. I told them they could now boo me. Only one or two half-hearted boos came. Then I said I would grade to improvement, so they would get a mark no less than what they did on the final exam. The examinations along the way were important for them to learn, not to show what they had already learned.

The head of teaching called me in about the mid-term because he was getting calls from counselors of students who had failed. I told him that I was supervising the teaching assistants closely, going to their classes and giving them advice, and that the exam was not hard. I showed it to him, and he agreed. It turns out that professors who had taught the course had given exams that were way too hard, thinking that this was O.K. for serious mathematics, so they had to grade on the curve, with 20% on the exam enough to pass. So students were not at all prepared for the courses in other subjects that depended on their knowing calculus. I told him I was just bringing the standards of the department up to the level at Iowa State University. In the end most students stayed with the course, and almost all passed. And learned the subject.

In 1983, I think, I attended a graduate seminar course by Mates on logical positivism. He gave us a list of books and papers we'd discuss; and each of us would present one. I chose to be the first in order to get it out of the way; I can't remember which book or essay. In class I described logical positivism as presented in it. Then I said that the positivists were rejecting metaphysics only to set up their own metaphysics. Mates was not amused. He had been raised on logical positivism, and apparently approved of it. But unlike pyrrhonism, it was dogmatic, indeed very dogmatic. And where there is dogma, there is metaphysics.

In spring of 1983 I gave a talk to the large logic colloquium, part of the Logic and Methodology of Science program there. I thought philosophers would come. It was essentially the talk "A General Framework for Semantics for Propositional Logics" (p. 12 above) that I later gave in Brazil, but I titled it "If Logic Is the Right Way to Reason, Why Are There So Many Logics?". The logicians in the math department thought that logic was a branch of mathematics, which they saw as talk of abstract objects, having nothing to do with reasoning. They were not receptive at all.

The beginning of the second year, fall of 1983, I went to the head of teaching in the math department and said that I would be willing to teach a graduate seminar on my research on propositional logics in addition to the calculus course and the real analysis course. I hoped to find a graduate student who would be interested in the material and work with me. I put up a notice for the course in the philosophy department. Walter Carnielli and Newton da Costa saw it—they were

visiting Berkeley for the year and were very interested. They attended the classes, and thus began my collaborating with Walter, and some with Newton, which I describe in the annotations. But it didn't go well with the logicians in the mathematics department: they thought that propositional logics were not serious mathematics, almost trivial (thinking of classical propositional logic), and the material couldn't be serious because some of the secretaries in the department attended and only one graduate student, who wasn't really interested in the subject. And hence after that second year in Berkeley, though the head of teaching wanted me to continue teaching, I was told that the logicians blocked me. Not a serious logician, by their standards.

I continued to live in Berkeley, in my rent-controlled house. I was asked to teach mathematics at Mills College the next year, but I turned it down, wanting to work full time on the book I was writing on propositional logics (*Propositional Logics*, annotated on p. 13). I met Peter Eggenberger at Berkeley, who was very interested in that work. He'd had his Ph.D. in philosophy at Berkeley and was working in administration at a bank. I learned a lot of philosophy from him and dedicated *Propositional Logics* (see p. 13 above) in part to him. Our talks also helped with the development of *Computability* (see p. 7 above) and later for *Critical Thinking* (p. 27 above).

I continued as a visiting scholar in the Philosophy Department, but that ended when the administration of the university wanted to limit who could have that title, since it carried the privilege of having a library card. That was important because access to the main stacks was limited to faculty and graduate students because the building was old and there was a risk of collapse if there were too many people in the stacks

The next year Itala D'Ottaviano, a colleague from Brazil of Walter Carnielli and Newton da Costa, visited Berkeley, which led eventually to our collaborating when I visited Brazil (pp. 13–14 above).

Sometime about then, I don't remember when, I was on a bus from the campus back to my home when a young man politely asked me if I was Dr. Epstein. I said yes. He said that he was a graduate student in the math department and that he had read my book *Degrees of Unsolvability*. He said that all the graduate students who needed to know that subject read my book for their exams, though the professors didn't recommend it. I was pleased my work had helped, and I thanked the young man.

To support myself, I took a position at California State University Hayward, a half-hour drive from where I lived. I enjoyed teaching there, and was asked to be on a committee to decide what text to use for their discrete mathematics course. It also got me a library card I could use at Berkeley. But after the end of year, I had to turn it in—though I was sure to be hired again the next semester according to the university's contract with "temporary" faculty. I asked the chairman if he could arrange for me to have a library card so I could continue my research, but he didn't or couldn't. So I wasn't a continuing faculty there. So I applied for unemployment insurance for the summer. A big mistake. It involved the department, which had to say that no I wasn't continuing but would be hired in the fall. Then in the second year there (1986–1987) Walter Carnielli, back in Brazil, organized for me to get a

Fulbright Fellowship to his university, UNICAMP (Universidade de Campinas). That was a big enough deal that the newsletter put out by the Cal State administration announced it. But it meant that I couldn't teach the spring semester, and the department chair, now wary of me for claiming unemployment, wouldn't say that I would have a position the following fall. I was stupid.

The rest of my time in Berkeley I've described in the annotations to the work I did. Besides what's in those annotations, I attended a course Mates gave on Leibniz, which led to his book *The Philosophy of Leibniz*. I didn't like it. Leibniz seemed to me like so many "genius" mathematicians working in logic, going from one project to another, very clever and unfocused.

One course he gave that I attended late in the 1980s was a graduate seminar on pyrrhonism. We went through Sextus Empiricus' *Outlines of Pyrrhonism* in the Loeb Classical Library translation by Bury, with Mates commenting on it. Mates often explained why the translation was wrong, and he made good sense of what Sextus had written. I was taken with pyrrhonism as Mates explained it, for it was more than compatible with my looking at various metaphysics as bases for how we would understand the world but not as right or wrong. After the course I urged Mates to make his own translation of that book, for otherwise all he had said would be lost. He was persuaded, and I read through his penultimate draft and made comments for him. That was published as *The Skeptic Way: Sextus Empiricus's Outlines of Pyrrhonism*. I consider my getting him to make that translation a major accomplishment. I think that was the last academic work he did.

Late in the 1980s I started work on predicate logic, which I discussed with Mates (see the annotation to *Predicate Logic*, p. 20). But then the landlord of my rent-controlled house, with whom I was on very good terms, had a stroke, and he and his wife wanted to move into that house. I asked for some time to find another place, and they agreed. Since my work on predicate logic with Mates had gone about as far as it seemed it could, and rents were so expensive in the Berkeley area, and I had no job, and Berkeley was a place for anomie and anonymity, I decided to move away. I ended up choosing Cedar City, Utah, where I had been an actor in the Shakespeare Festival years ago—a friendly town, I thought, and a beautiful place to live with hiking and a small college. Not a good choice, but that's another story.

Addendum: The Advanced Reasoning Forum (1999–)

In the summer of 1999 Desidério Murcho and Célia Teixeira came to study with me in Cedar City. I had met them when I visited Lisbon, Portugal to give lectures for a month. Desidério began working on translating *Critical Thinking* into Portuguese, which later Walter Carnielli took on (see p. 33 above). Flush with an advance on royalties from my contract for *Critical Thinking*, I thought to gather some of my friends and colleagues for a meeting in Cedar City on logic and reasoning. We needed a name for the group we were forming that would have "ARF" as an acronym; Walter Carnielli suggested "Advanced Reasoning Forum". In early September Walter Carnielli (Brazil), Peter Adams (California), Fred Kroon (New Zealand), Alex Raffi (Nevada), Mircea Dumitru (whose recent Ph.D at Tulane University I had seen and discussed with him), Alan Venable (the development editor for *Critical Thinking*, California), Desidério Murcho, Célia Teixeira, and I met for five days, giving talks, discussing, hiking, seeing a falconer fly his bird, and beginning a collaboration that has grown over the years. Bill Robinson wanted to be there, but couldn't make it.

The next year we had a meeting in Bucharest, Romania, organized by Mircea Dumitru with support from New Europe College. There Staszek Krajewski and Bill Robinson joined us. I was making money from the critical thinking text, and with my wife Carolyn Kernberger sponsored meetings in 2001 in Berkeley, where Benson Mates joined in, and then at New Year's time 2003 we sponsored a meeting in João Pessoa, Brasil. Much about those meetings and ARF can be found at the ARF website, <www.AdvancedReasoningForum.org>.

In 2011 I was invited to the Brazilian Logic Meeting where I gave a lecture on the world as process and met many students and scholars, inviting them to visit me at my research center at my ranch Dogshine, near Socorro, New Mexico. Esperanza Buitrago Díaz took up the offer and came for ten months, which led to our collaborating (see p. 9 and p. 17 above). Then we set up a program of ARF Fellowships. The next year, 2013, Henrique Almeida Antunes came as an ARF Student Fellow (see p. 9 and p. 47). In 2016 Juan Francisco Rizzo (Pancho) came as an ARF Student Fellow. He and I had met corresponding when he was working with Manuel Dahlquist in Argentina on a Spanish-language version of *The Pocket Guide to Critical Thinking* (p. 33 above). We had so many discussions that my work would have been much worse without his help, though we never published together. Esperanza, Henrique, and Pancho then became members of ARF. Also in 2016. Victoria Pöhls had an ARF Student Fellowship, though her interests later turned to literary criticism.

When I got back the rights to the books I published with Wadsworth, I needed to make them available again. So I set up ARF as a publishing company, starting with the critical thinking books. Later, as I noted that some books which I read for my studies were deep and clear and important yet were out of print and hard to obtain, I organized a series for ARF to publish called *Classic Reprints*, starting with Benson Mates' *Stoic Logic* and Arne Naess' "*Truth*" *as Conceived of by Those Who Are Not Professional Philosophers*, which I had learned about from Mates.. Naess showed by surveys and interviews that there are many conceptions

of truth, none subscribed to greatly more than another, despite philosophers claiming precedence for whatever view they hold which they are sure everyone does, too. We continued that series with more reprints in logic and then ones on conventional gestures and on ethics (see the ARF website).

We had no more meetings, as the textbook market in the United States collapsed and with it funding that I could provide. But we continued to talk and correspond. We added two more members, Michael Rooney, who worked with me on editions of *Critical Thinking*, and João Marcos, who collaborated with Walter Carnielli and who visited me briefly at Dogshine. With each of them I have had many interesting—no, exciting—discussions. Then working on the world as the flow of all, I began corresponding and talking with Chad Hansen, Chris Sinha, and Vera da Silva Sinha, who enriched our ARF online discussions and research, and became members of ARF. Eduardo Ribeiro (Eddie) contacted me about my work around that time, which led to our talking and corresponding and continuing to learn much from each other; he became a member of ARF, too. And Kris Hardy, who started as our webmaster, was so clear and insightful in discussions about the work on the world as the flow of all and time in relation to his expertise in computer science that I learned a great deal, and he became a member of ARF, too. With so many new members, we had a "rolling" ARF meeting for much of 2023, meeting once a month for an online video discussion led by one of us. Over the years, reading each other's work, commenting, getting excited about developing new ideas, the Advanced Reasoning Forum has been good for us.

I look back now. Through it all so many people I have learned from, who have given generously of their time and interest. Perhaps I, too, helped them some. Though the best of my help I have tried to make available to all in *The BARK of DOG*.