Theory and Decision Library A: Rational Choice in Practical Philosophy and Philosophy of Science

Thomas Augustin
Fabio Gagliardi Cozman
Gregory Wheeler *Editors*

Reflections on the Foundations of Probability and Statistics

Essays in Honor of Teddy Seidenfeld

Springer

Theory and Decision Library A:

Rational Choice in Practical Philosophy and Philosophy of Science

Volume 54

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Thomas Augustin • Fabio Gagliardi Cozman •
Gregory Wheeler
Editors

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Editors
Thomas Augustin
Department of Statistics
Ludwig-Maximilians-University of Munich
Munich, Germany

Fabio Gagliardi Cozman
Universidade de São Paulo
São Paulo, Brazil

Gregory Wheeler
Frankfurt School of Finance & Management
Frankfurt am Main, Germany

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Preface

About 5 years ago, we noticed that Teddy Seidenfeld, although as active and cheerful as always, was approaching his 70th birthday(!). We then decided to put together a Festschrift to celebrate Teddy's many contributions to philosophy, statistics, probability, game theory, and related fields. This volume covers a subset of those themes; going through all topics that have received Teddy's attention would require a series of books. We hope the reader will continue this celebration by reading the many papers written by Teddy himself with his many collaborators.

Teddy was born in New York City in May 1948. He took a major in mathematics and philosophy at the University of Rochester and then a PhD in philosophy at Columbia University. He started his academic career as an assistant professor at the University of Pittsburgh, moved to Washington University in St Louis, where he was an associate professor, then to Carnegie Mellon University, where he is now the H. A. Simon Professor of Philosophy and Statistics. In his distinguished career at Carnegie Mellon, Teddy has worked as head of the Department of Philosophy, as graduate director of the logic and computation program in philosophy, as co-director of the machine learning graduate program, and as chairman of the Faculty Senate.

Rather than write a dry review of Teddy's career, we decided to interview Teddy to offer his own thoughts on past events. We are most grateful to Teddy for providing witty and detailed answers to our many questions — in fact, we had to limit ourselves to a small fraction of our questions to keep the interview within a reasonable size. The reader will find the interview in the first chapter of this book, ranging from Teddy's experiences as a baseball fan and chess player to his formative years as an undergraduate and graduate student. The reader will also find insightful answers on Teddy's collaboration with Henry Kyburg and Isaac Levi, his advisers at Rochester and Columbia, respectively, on Teddy's long-standing collaboration with his colleagues Jay Kadane and Mark Schervish at Carnegie Mellon, as well as commentaries on Teddy's perspective on research.

Teddy has written extensively on the foundations of probability and statistics, individual and group decision theory, and on the interface between philosophy and statistics. His contributions range from deep mathematical results on finitely additive or infinitesimal probability to general consequences of coherence and consensus in statistical reasoning and decision-making. His perspective is always based on solid mathematical arguments. Indeed, Teddy seems to have a particular interest in examining the limits of proposed theories, for instance, by visiting cases where measure-theoretic probability theory leads to unexpected and sometimes unpleasant conclusions. Thus Teddy has produced, with his collaborators, a collection of gems on finitely additive probability, for instance, looking at the consequences of finite (not σ-)additivity to conglomerability, sequential decision theory, and convergence theorems. Teddy's analysis of decision theory, and of sequential decision theory in particular, has led to a refined understanding of connections between acts and beliefs, normal and extensive forms, and unbounded utilities. And Teddy has often returned to questions related to convergence and consensus, carefully mapping cases that go beyond the usual convergence-to-certainty results at the core of probability theory. Consensus, of course, does not arise only through convergence; Teddy has also examined the shared preferences of decisions makers and how those preferences may, and may not, coalesce into common preferences.

Through the years, Teddy has been an influential voice within the community interested in "imprecise" or "indeterminate" probability. Indeed, Teddy is one of the leading forces behind the biannual *International Symposium on Imprecise Probabilities: Theories and Applications* (ISIPTA), and one of the founding members, and a past president, of the *Society for Imprecise Probabilities: Theories and Applications* (SIPTA). Teddy has examined the principles that justify representations based on sets of probabilities, from preference axioms to scoring functions. His analysis of decisions without ordering has been an influential guide in the research community dealing with imprecise and indeterminate probabilities. In fact, that analysis was the first articulated study of sets of desirable gambles and lexicographic probabilities. His analysis of the curious phenomenon of dilation, where conditioning leads to uniformly larger probability intervals, has generated much perplexity and discussion. His work on choice functions has opened many paths still under exploration, alerting the community to more general representations than previously thought possible. There are so many topics in imprecise and indeterminate probabilities with Teddy's fingerprints that an attempt to list them all is bound to fail.

Teddy's contributions extend beyond imprecise and indeterminate probabilities as well, especially in relation to the foundations of statistics. Fisher's fiducial argument, direct and indirect inference, randomization, experimental design: these and other topics have received Teddy's sharp analysis.

Putting together this volume, we have invited a number of scholars whose interests intersect with some of the Teddy's contributions. Submitted papers were reviewed by two reviewers each and then revised by the authors. We thank the reviewers for their excellent job and the authors for their excellent contributions and patience during this long process.

The resulting collection begins with Teddy's interview and a novel publication by Teddy and collaborators, produced specially for this volume, on deductive and probabilistic explanations and the value these explanations add to scientific theories.

We now comment briefly on the other contributions in this volume, starting with a general introduction to imprecise probabilities, then turning to aspects of coherence and temporal reasoning, preference representations, and further aspects of (generalized) decision theory, like mixed strategies, choice functions, and new optimality criteria.

Since many papers are concerned with imprecise probabilities, it is natural to start this part of the collection with a gentle tutorial on this topic. Gregory Wheeler's broad overview of the theory of imprecise probabilities recalls and illustrates the most important notions, like lower probabilities, coherence, credal sets, lower previsions, acceptable gambles, preference orderings, and related models, and also gives an introduction to conditioning in this generalized framework.

Matthias Troffaes and Michael Goldstein offer novel foundations for temporal reasoning, relying on random quantities without reference to an underlying possibility space. Future beliefs are also taken as random quantities, and a temporal sure-preference principle guides inference over time. The authors explore its implications for lower previsions, derive an explicit expression for the natural extension, and show how to recover the standard Bayes linear calculus in this framework.

Hailin Liu moves to sequential game theory, looking at the conditions for the equivalence of normal and extensive form representations and important differences between the two. He distinguishes and elaborates settings where two extensive form games with identical normal form lead to distinct outcomes and settings where reduced normal form games fail to capture some reasonable strategies.

Seamus Bradley then discusses dilation, one of the most often-criticized aspects of imprecise probabilities. In short, dilation suggests that whatever event is observed it may lead one to become less certain! By investigating concepts related to uncertainty and informativeness, Bradley achieves a deeper understanding of the phenomenon of dilation, also questioning the standard view on it.

Fabio Gagliardi Cozman studies connections among a host of representations for preferences that capture not only imprecision and indeterminacy in probabilities but also that discard usual Archimedean assumptions. Lexicographic probabilities, sets of desirable gambles, and full conditional probabilities are connected and compared.

Davide Petturiti and Barbara Vantaggi extend different concepts of coherence to the Dempster-Shafer theory of evidence, an alternative to probability theory with deep mathematical ties to sets of probabilities and interval-valued probabilities. Their study shows that the equivalence of different forms of coherence—as a consistency notion, as fair betting scheme, and its formulation in terms of a penalty criterion—is still valid, also providing operational tools for eliciting and interpreting belief functions.

Jean Baccelli provides a three-dimensional perspective on preference relations and their representation through subjective expected utility. Taking into account not only the dimension characterized by utility values but also the often neglected dimensions imposed by state-dependence and act-dependence, the classical uniqueness of representation fails, which questions the common behavioral identification of subjective probability.

Kevin Zollman looks at mixed strategies in decision-making, that is, strategies where actions depend on randomization or, to use Zollman's words, on flipping coins. To discuss the justifications of such strategies and, in particular, to elaborate their difficulties, Zollman analyzes several settings, such as imprecise probabilities, incommensurable values, fairness, imperfect recall, act-state dependence, and game theory.

Jasper De Bock and Gert de Cooman dive into the world of choice functions and their connection with sets of desirable option sets, exploring, in particular, a notion of irrelevance and independence that initially has been implicitly proposed by Teddy Seidenfeld. They show the consequences of this type of irrelevance assessment and relate it to the E-admissibility criterion for decision evaluation.

Enrique Miranda and Arthur Van Camp also explore choice functions. More concretely, they investigate how a classical representation result by Teddy Seidenfeld and co-authors on coherent choice functions behaves when one does not adopt an Archimedean condition. They manage to show that, in general, some main attributes normally associated with representations of choice functions do not survive, and they identify important special cases with valuable properties.

Closing the book, Christoph Jansen, Georg Schollmeyer, and Thomas Augustin focus on decision-making with imprecise probabilities under the criterion of E-admissibility and some extensions. They first propose a relaxed version of E-admissibility, judging the deviation of maximal acts from admissibility. Then they measure the stability of the E-admissibity of acts by properties of the set of probabilities under which admissibility is maintained.

This project took some time, indeed much more time than initially expected. Again, we thank the authors for their patience, willingness to help, and the quality of their contributions, and we are most grateful to the reviewers and the publishers for their invaluable support.

To conclude, we add here a message by noted philosopher Isaac Levi, the John Dewey Professor of Philosophy at Columbia University, Teddy's PhD advisor, and himself a key figure in the study of indeterminate probability. This message was sent a few days before Levi passed away. He was very enthusiastic about this project from the start.

> In a world dominated by Alternative Facts and Fake News, it is hard to believe that many of us have spent our life's work, as has Teddy Seidenfeld, in discussing truth and uncertainty.
>
> I have admired him for so long, first as one of my best students and then as one of my most inspired colleagues, in discussing these themes, which, as we all know, are essential to our understanding what is knowable. I may be at a point in my career when I no longer have the stamina to contribute anything essential to the Festschrift for this important philosopher. And I may not always agree with him. But I do recommend his work to all of you who take the quest for truth seriously.

We hope the reader will enjoy learning from this book as much as we enjoyed producing it.

Munich, Germany — Thomas Augustin
São Paulo, Brazil — Fabio Gagliardi Cozman
Frankfurt am Main, Germany — Gregory Wheeler

Three philosophers: Isaac Levi, Teddy Seidenfeld, Henry Kyburg (left to right)

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Chapter 1
An Interview with Teddy Seidenfeld

Thomas Augustin, Fabio Gagliardi Cozman, and Gregory Wheeler

A-C-W: Dear Teddy, thanks much for agreeing to answer a few questions here about your life and your career. We have had the pleasure to interact and to learn from you; we certainly have many questions not only about you but also about your thoughts on a variety of topics. Here we have a selection of the possible questions.

Teddy: I'd like to begin this virtual-conversation by thanking the editors and each of the volume's authors for the considerable efforts and kind considerations their contributions reflect. There is no greater intellectual pleasure, I feel, than to see others building on and improving one's thinking. It is through a succession of careful reflections that, collectively, we make progress.

A-C-W: You were born in New York City, played chess as a kid; how those experiences (and maybe others...) led you to pursue a career in philosophy of science?

Teddy: Central Brooklyn (Eastern Parkway near Flatbush Ave) during the 1950s and early 1960s was an amazing place in which to grow up. My network of childhood friends developed through the excellent New York City public school system, which I attended from, kindergarten through 12th grade, and from playing stickball in the neighborhood streets. For one dramatic example, though separated by 2 grades, Janice and I attended the same public schools.

T. Augustin
Department of Statistics, Ludwig-Maximilians-University of Munich, Munich, Germany
e-mail: thomas.augustin@stat.uni-muenchen.de

F. G. Cozman
Universidade de São Paulo, São Paulo, Brazil
e-mail: fgcozman@usp.br

G. Wheeler
Frankfurt School of Finance & Management, Frankfurt am Main, Germany
e-mail: g.wheeler@fs.de

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Once you were old enough to cross the street—we walked to school starting in 1st grade—within a 1 mile radius was the Brooklyn Museum, the Botanical Gardens, the enormous Grand Army Plaza branch of the Brooklyn Public Library, and Ebbets Field—where the hapless Dodgers played baseball until they finally won the 1955 World Series. (But soon thereafter they left for the West Coast.) When the Dodgers managed to win games against their fierce rival NY Giants, led by Willie Mays, it was their fate then to lose to the unstoppable powerhouse NY Yankees, led by Micky Mantle. Read Kahn's (1972) *The Boys of Summer*.

As a loyal Dodger fan I learned the important lesson summarized by their eternal slogan, *Wait 'til next year!* That attitude signaled optimism about the opportunities for future improvement, rather than dwelling on the failures of the recent past: Pragmatism 101!

The New York City public schooling that I had, especially in grades 4–12, starting during the second term of Eisenhower's presidency, the Sputnik Era, and moving into the Kennedy years, reflected an emphasis on a hastily revised STEM curriculum. Even as pre-teens we were immersed in Science Fairs and Math Team competitions. No doubt, those years fixed the direction of my future academic interests. But in my own case, alas, the Science Fairs and Math competitions provided another context for applying the Dodger's slogan.

You mention chess. That was a family affair. My uncle (my mother's bother-in-law) was Isaac Kashdan. "Kash" as he was called by his friends, had been arguably the country's leading chess player in the late 1920s and early 1930s—at the end of Frank Marshall's reign and before Reuben Fine and Sammy Reshevsky took the lead. (See Lahde's (2009) collection.) My aunt, uncle and cousin lived in Los Angeles, where Kash's day job was as an accountant. He was principal in creating the West Coast chess scene. And for most of 30 years (starting in the early 1950s) he wrote the chess column for the LA Times and mentored many leading young players.

My mother, brother, and I spent wonderful summer vacations there, often joined by my father on his brief, 2 week holidays from his job with the City of New York. Uncle Kash encouraged my chess interests, particularly as my cousin was mostly indifferent to the game. My uncle's persistent counsel to me was to spend more time with Fine's classic book, *Basic Chess Endings*, and less time pondering the latest dramas of opening theory. It's not just that that was his forte—Kash was known for his endgame prowess, especially with 2 bishops. He knew that a young chess player could improve more rapidly by understanding the endgame first.

From my current perspective, I'd explain Kash's advice as follows: Though chess is a perfect-information, 2-person, zero-sum, finite game, the von-Neumann Minimax Theorem does not afford useful guidance for becoming a better chess player. However, as chess is a sequential game, the general principle of *backward induction* does provide important practical guidance. And in order to apply that principle, the player needs first to understand endgame theory.

My greatest "win" from chess is a lifelong friendship with Steve Alpern.[1] Steve and I met over the chessboard in 7th grade. Steve played one board higher than I did in our Junior High School and High School teams. Despite my uncle's advice, endgame skill does not make up for a significant shortfall in basic talent! It amazes me still that our common interests have not decreased over many years, and that our shared academic interests have increased.

A-C-W: How was it working with Henry Kyburg? What was your work? Did it influence your career (how)? And naturally: how was it working with Isaac Levi? What was your work? Did it influence your career (how)?

Teddy: For my undergraduate studies, I attended the University of Rochester (class of 1969) where I quickly transitioned out of a pre-Med curriculum and into the world of Mathematics and Philosophy. I attribute that evolution to two wonderful and amazing teachers: Stanley Tennenbaum[2] (Mathematics) and Henry Kyburg[3] (Philosophy). Their pedagogical styles could not have been more different.

Tennenbaum's undergraduate classroom was simultaneously spontaneous—he elicited the students' ideas with great enthusiasm—and epistemologically sophisticated, even Socratic. Among his classes I took were *Number Theory* and *Set Theory*. The class meetings were driven by *questions*, many of which came from the students' written logs of what had transpired at a previous class meeting. When responding to a question, you were expected to calibrate your response in one of three general categories: either you offered a "guess," or a "reasonable conjecture," or highest of all, a claim to "knowledge." The ground rules where that a student was committed to providing more thorough justifications for responses in the upper categories. For instance, if you claimed *to know* some bit of mathematics, Tennenbaum could make you stake your course grade on your ability, then and there, to defend your claim against whatever follow-up questions class members might pose. Tennenbaum injected his own follow-up questions, often to help out a poor student who was evidently in over her/his head, guiding the class investigation to a more enlightening direction. It was an electrifying atmosphere.

Tennenbaum helped me to find some appropriate problems for individual work on the combinatorics of finite Boolean algebras. That led me to *logic*, in a broad sense, and to the Philosophy Department. In my first exposure to Philosophy, in the same semester, Kyburg taught an undergraduate course in mathematical logic in the morning (the text was the English translation of Hilbert and Ackermann's (1950)), and an undergraduate inductive logic course in the afternoon. We studied Carnap, and reviewed different interpretations of probability. With that sample of two courses, too quickly, I concluded that Philosophy was applied Boolean Algebra. And that was exactly what I wanted!

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[1] https://en.wikipedia.org/wiki/Steve_Alpern.

[2] https://stanleytennenbaumamericanoriginal.com/.

[3] https://en.wikipedia.org/wiki/Henry_E._Kyburg_Jr.

Kyburg's classroom style in those two undergraduate classes was straight-up lecture. Always, he was very carefully prepared and—in sharp contrast with Tennenbaum's pedagogical style—free of drama. But just like Tennenbaum, Kyburg was generous with his time, encouraging me to study statistics and offering helpful directions into that literature. One mistake I made, which almost ended my interests in *Foundations*, came about when I found Kyburg's (1961) *Probability and the Logic of Rational Belief* in the campus book store. I spent $15.00 for it—Hilbert and Ackermann cost only $3.95 by contrast—and set about, for the first time, to try to read a book written by one of my teachers. *P&LRB* contains many important ideas about statistical inference, and it presents Kyburg's original interval-valued probability theory, later called *Epistemological Probability*. Though the discussion and motivations of his ideas were compelling, I could not follow the formal derivations. Kyburg chose Quine's impenetrable *Protosyntax* for his first-order formalism. Fortunately, the meta-language for the book was English.

Kyburg was Chair of the Philosophy Department. I suspect that has something to do with the series of unusual departmental colloquia and visitors. I recall one term when the colloquia included presentations of competing accounts of *acceptance*: One week we heard Richard Jeffrey. In another Isaac Levi. Harper and Kyburg's (1968) paper, *The Jones Case*, was one byproduct of this debate! My first Epistemology class, which I took with the then newly graduated Risto Hilpinen, who was visiting the Department (in Spring 1968?), covered Hintikka's (1962) book, *Knowledge and Belief*. How many other undergraduate philosophers believed that the *KK*-thesis was the central challenge in Epistemology?

The U of R's Philosophy Department, though small, was a distinguished academic unit. I had the privilege of taking a course on Kantian theory with Lewis White Beck. And a course on Berkeley's Idealism taught by Colin Turbayne. Shortly after Rolf Eberle joined the faculty (1967) he and Kyburg led a small study group in Cohen's (1966) *Set Theory and the Continuum Hypothesis*. Quite an intellectual feast for an undergraduate!

Eberle took over the logic curriculum and used Montague & Kalish's excellent textbook—a good, natural deduction system. In my senior year (I think), I graded student homeworks for Eberle's course. How happy I was that natural deduction allowed applying *backward induction* as a perspective for distinguishing the insightful derivations from among the merely valid proofs!

When it came time to consider graduate schools, Kyburg kindly offered to write a letter of introduction for me to meet Sidney Morgenbesser at Columbia University. (Kyburg took his Ph.D. there in the late 1950s, under the supervision of Ernest Nagel.) Morgenbesser agreed to meet. We chatted for about 45 minutes and I was persuaded to study Philosophy at Columbia, rather than trying a career in Mathematics. If you knew Morgenbesser, you'd understand that he could bring out the Philosopher in any person!

I began my graduate studies at Columbia in earnest during Fall 1970. Isaac Levi joined the faculty there then too. Whereas Kyburg's perspective was formal inference, Levi's was decision making. Levi's teaching was my first serious exposure to decision theory and so-called "Bayesian" methods. Also, during those years Levi was developing his original theory, Levi (1974), of "Indeterminate Probability" [IP]. Having learned Kyburg's theory from its creator, I found it entirely natural for Levi to move towards set-valued probability functions. But I was ignorant of the conflicts between Kyburg's theory and the more-Bayesian oriented perspective that Levi used. Ignorance can be bliss. And it was so in my first student paper for Levi.

Levi showed me Good's (1967) *BJPS* paper "On the Principle of Total Evidence." Good gave an elegant 3-line proof that, in a generic strategic-form sequential decision, the decision maker maximizes expected utility by postponing a terminal decision in order to allow that decision to be a function of new, cost-free evidence, rather than by deciding in the absence of the new evidence. Levi's first assignment for me was to extend Good's result using interval-valued probabilities. I found simple statistical examples where Good's result did not generalize if "using new evidence" was formalized with Bayes's rule for updating a set of probabilities and if IP decisions were made with lower expectations. Also, it came naturally to me to consider the problem using Kyburg's theory. I showed how one could generalize a version of Good's result in Kyburg's system because of Kyburg's *Strength* rule for randomness. All this apparently delighted Levi, who reported the news to Good. That led to Good's (1974) *BJPS* note, "A Little Learning Can Be Dangerous!" I was hooked! Apparently Levi was too, as he took me under his wing.

> *Aside:* This line of thinking evolved 20 years later into collaborated work on *Dilation*, carried out at CMU with Larry Wasserman (Statistics) and Tim Herron (a Philosophy graduate student).

Isaac recognized that I was more comfortable with issues of statistical inference than with decision theory. So, for my Ph.D. thesis, he set me working on Fisher's enigmatic *Fiducial Argument*, with a secondary goal of contrasting Hacking's (1965) and Kyburg's (1974) rival reconstructions. I spent most of 2 years (1974–1975) immersed in Fisher's papers and books: the 5 volumes of his *Collected Papers*, *Statistical Methods for Research Workers*; *The Genetical Theory of Natural Selection*, *Design of Experiments*; and *Statistical Methods and Scientific Inference*. Also during that time were many wonderful hours of conversations with a colleague of exceptional intellectual breadth, Jonathan Lieberson.[4] The world of letters lost a major talent with his death, at only 40, in 1989.

My Ph.D. thesis committee consisted of five Columbia faculty: Levi (as advisor), Morgenbesser (as Chair), and the noted logician Charles Parsons, who served as the third Philosophy Departmental member. Parsons knew me somewhat from his graduate course on Gentzen's natural deduction, and from my helping to grade his undergraduate logic class, which he based on Quine's *Methods of Logic*. I doubt Charles recalls a short conversation we had in Fall 1970 when I first met him at one of the informal departmental get-togethers. Those social events were appropriately called "Smokers," as most every faculty member did. Parsons asked about my academic interests. I responded that I wanted to work on decision problems. But

[4] https://www.nytimes.com/1989/06/17/obituaries/jonathan-lieberson-40-author.html.

to a logician of Parsons' standing those words did not express my meaning, and they evidently caused him some discomfort. He thought I wanted to work with him on decision problems in logic. Parsons hesitated. In a carefully chosen response he suggested, that "Those issues have mostly been solved." The signal was clear. He was not encouraging me. But his mood improved quickly when I added that I had started to work with Levi. "Oh. Fine. Those decisions."

Burton Singer and Bruce Levin, from Columbia's Math. Stats. Department, were the two "outside" members of my thesis committee. I had surreptitious meetings with Singer, after hours, in his office. The procedure was to wait for a signal—a campus phone call—that the Department Chair, Herbert Robbins, had left the building. Apparently, there was an unresolved intellectual feud Robbins had with Ernest Nagel over the foundations of Empirical Bayes theory, stemming from an exchange at a 1961 Wesleyan Conference on Induction. (See Kyburg et al.'s 1963 edited proceedings.) Philosophers were not welcome in Robbins' department!

I defended my thesis on Fisherian Statistics in this 5-on-1 arrangement in Fall 1975. Apparently, a tie goes to the student!

My first academic position started in Fall 1975 as an Assistant Professor in the Philosophy Department at the University of Pittsburgh. It was a powerhouse unit, especially in Philosophy of Science and Philosophy of Logic. During my 6 years there, the senior faculty included Belnap, Gibbard, Grunbaum, Hempel, Rescher, and Sellars, along with Earman, Glymour, Laudan, and Schaffner as joint members from the History and Philosophy of Science Department. Located on the floors just above the Philosophy Department at Pitt was the Math/Stats Department. And during the late 1970s that Department was home to C.R.Rao, one of Fisher's most distinguished students. It was an exceptional treat to discuss Fisher's ideas with Rao, who was generous with his time and more than patient with a young philosopher! Rao was a magnet for other great statisticians. I recall the year J.K.Ghosh visited and instructed several of the junior faculty (I was included) in one course on topics in statistical estimation—which led to work on 2nd order efficiency—and in another on problems of optimal stopping. In a sense, that course created a run in for me with Robbins. Chow et al. (1971) *Great Expectations* for the text!

Both Kyburg and Levi encouraged me to convert my thesis to a book, which I submitted to the *Theory and Decision Library* series. Henry—who had a strong prose style—took great pains serving as my copy-editor and improved my awkward writing. The book came into being, Seidenfeld (1979). Starting in about 1975 my relationships with Kyburg and Levi evolved from being their student, to being colleagues, and most important of all, to being friends. The Kyburg and Levi families had been friends since the 1960s. Janice and I (and eventually our children, David and Adina) were welcomed too. I recall in particular one visit to the Kyburg farm in the late '70s. Sarah Kyburg was the farm's principal farmer and, when he was not at the University, Henry functioned as the farm's principal hired-help. Over the years, in varying numbers, the Kyburg offspring rounded out the workforce. (It was rumored that Henry required Philosophy graduate students do some "applied work" too.). Henry instructed me how to operate the riding hay baler, and then let me loose in a nearby field. I had great fun and, as was evidenced by the number of bales of hay in the field at day's end, Henry's skill as a teacher was not limited to logic! Perhaps it was that same summer, Janice and I took the Levi's cabin in Vershire (VT) for several weeks of holiday. Judy Levi encouraged us to enjoy small town life in New England. Isaac added that the cabin was where he wrote Levi (1967), *Gambling with Truth*.

A-C-W: What are your feelings about Carnegie Mellon University? What makes it unique in its approach to philosophy and to statistics? And: How did you start your longtime collaboration with Jay Kadane and Mark Schervish?

Teddy: A 15 minute walk from Pitt takes you to CMU. Under the Headship first of Morrie DeGroot (1966–1972) and then Jay Kadane (1972–1981), already by the late 1970s CMU's Statistics was among the very top notch departments in Bayesian methods, and in Statistical computing. The Statistics Department had unusual autonomy, even by CMU's atypical "strong department Head" administrative model. From its origins in 1966 until 1981, Statistics was a department unattached to a College and the Department Head did not report to a Dean. Instead, the Department was overseen directly by President Richard Cyert. That relationship was not surprising given the personal friendship and active collaboration of a dozen publications by Cyert and DeGroot (See the list of DeGroot's publications in his (1991) biography in *Statistical Science*, a prominent journal that DeGroot helped to found.).

In 1979, at the same time Mark Schervish joined the faculty, Kadane offered a graduate seminar on de Finetti's (1974), *Theory of Probability*. The opening pace was rapid, I recall, covering about one chapter each week. By about the fourth week's meeting, when we turned to issues of conditional probability, the class retained only a hardcore of three members: Jay, Mark, and me. As I recall, we didn't get to Chap. 5 that year. Instead, the three of us spent the rest of the weekly meetings that term working on problems of finite additivity and the "conglomerative" property of conditional probabilities. Those sessions resulted in our first joint Tech Report which appeared a few years later as Schervish et al. (1984). It was the beginning a wonderful collaboration that continues with almost weekly meetings to this day, 40 years later.

I was a member of the Philosophy faculty at Washington University for 4 years (1981–1985). There I had the good fortune to meet, among other colleagues, Ned McClennen, Bob Barrett, and Joe Ullian. Ned and I had long and intense debates about the theory of sequential decisions, focusing on the role conditional probabilities play in those problems. Alas, we were unsuccessful finding sufficient common ground to reach consensus. Bob was an exceptional philosopher, with a keen analytic mind. He had the rare ability to cut to the heart of an argument, from which skill I benefitted on numerous occasions. Joe, the sole surviving member of this trio, is an indefatigable logician and teacher. During the years I was there, he organized a continuing reading group, which meetings he held in the evenings at his off-campus apartment. We enjoyed many hours together working through Kunnen's *Set Theory*, followed always by one of his celebratory sessions of bottomless ice-cream desserts. As I recall, one of Joe's "small" servings of ice-cream had the magnitude of an inaccessible cardinal!

While at Wash. U., I learned that there were plans afoot to create a Philosophy Department at CMU, with Clark Glymour (then at Pitt's History and Philosophy of Science) to serve as its first Head. Prior to 1985 CMU had only an undergraduate Philosophy program, operating under the auspices of the History Department. Within CMU, three very senior faculty (Herb Simon, Dana Scott, and Jay Kadane) lobbied President Cyert to create a specialized research unit with academic strengths in Logic, Philosophy of Science, and Philosophy of Social Science. Initial recruiting brought the well known philosopher of Economics, Dan Hausman, to CMU. And during the academic year 1984–1985 Glymour's efforts focused on adding the logician and proof theorist Wilfried Sieg (then at Columbia University). I was invited to round out the team as someone working on foundations of Statistics. These efforts came to fruition in Fall 1985 with the four of us standing as the founding "senior" faculty of the new CMU Philosophy Dept.

Once back in Pittsburgh, and with my CMU Philosophy office just up the hall from Jay's and Mark's offices in Statistics, the three of us were able to shift our collaborations into a faster gear. As an overarching theme, we examined multi-agent versions of various aspects of Bayesian theory. For instance, in Schervish and Seidenfeld (1990), Mark and I investigated the asymptotics of Bayesian merging with shared evidence, involving more than two investigators. In Kadane and Seidenfeld (1990), Jay and I considered experimental randomization from a Bayesian point of view and showed how a using a set of probabilities to represent a group of expert opinions, a non-randomized, adaptive, clinical trials might achieve many of the Fisherian goals of randomized allocation. And together, the three of us began what is a continuing research program to understand better and to improve aspects of Bayesian methodology. Kadane et al. (1999) offers an early selection of our papers.

One strand of this ongoing work, which intersects IP theory, uses sets of (finitely additive) probabilities as a representation for a more flexible account of decision making than is allowed under a canonical (e.g. Savage-styled) Bayesian theory. Let me trace the evolution of our thinking in this direction in order to emphasize that, at least for me, progress sometimes is slow and incremental.

I start this short story with an early result, Seidenfeld et al. (1989) that reverberates through our later work, and which led us to one of the differences with Isaac's IP decision theory that remained unresolved over more than 30 years of friendly but sustained debate. I summarize. Consider two canonical Bayesian decision makers who have different degrees of beliefs about some event, and have different values, as reflected in different cardinal utility functions for outcomes. Suppose these two decision makers form a partnership—a group agent—in order to make cooperative decisions. If the partnership respects their shared strict preferences (i.e. if the partnership satisfies the Pareto condition that whenever each of the two agents strictly prefers an option A over an option B then the partnership's strict preferences also ranks A over B), then the partnership cannot be a Bayesian agent unless it is autocratic—one of the two agents makes all the decisions. Expressed differently, the canonical standard of Bayesian coherence does not lift up from individuals to cooperating groups.

Aside: The conflict with Levi's theory arises as he imposes convexity conditions on IP credal states and on IP values: sets of probabilities and sets of cardinal utilities are required to be convex in his framework. Then none of Levi's IP agents (other than the two autocratic models) represents the partnership between the two canonical Bayesian agents as described above. Without going into subsequent rounds in this debate, Levi resolves the challenge by rejecting an unrestricted Pareto condition for the group agent.

We continued this line of reasoning in our (1995) paper on representations for strict partial orders in a domain of Anscombe and Aumann (1963) "horse lotteries." There, we axiomatized all the binary strict preferences that arise by an application of the Pareto condition to an arbitrary set of canonical Bayesian agents. Not only do the representing sets of probability-utility pairs admit failures of the usual convexity condition for IP credal sets, sometimes they are disconnected sets—as in the illustration of the two Bayesian agents described above. However, even this approach fails to distinguish between all sets of probability distributions. That is, the representation of a strict partial order in terms of a set of probability distributions does not yield a unique set.

Our thinking about IP evolved again when we focused on aspects of decision rules that, like Levi's E-admissibility rule, do not reduce to binary comparisons. Consider a menu **M**, a set of feasible options. Call a decision rule R *binary* provided that admissibility from **M** reduces to comparisons by R among all the pairs of elements of **M**. That is, option A in **M** is admissible under a *binary* rule R *if and only if* no other option B in **M** is judged strictly better than A under R. In other words, A is R-admissible from **M** if and only if A is R-admissible from all pair-sets $\{A, B\}$ where B also is in **M**.

In what follows next, in the fashion of de Finetti's theory, let each element of **M** be a real-valued random variable X, defined on a common (finite) state space Ω, i.e., $X : \Omega \to \Re$. Fix attention to decision problems where credal uncertainty (and not personal values) is indeterminate. So, the IP decision maker has a single cardinal utility U for representing the values of outcomes. For simplicity and following a longstanding practice in statistical decision theory, let $U(X(\omega)) = X(\omega)$: the utility of an outcome $X(\omega)$ is the value the random variable assumes in state ω. But the agent's uncertainty about Ω is indeterminate. It is represented by a set **P** of probability distributions over Ω, not necessarily a single probability function over Ω.

In Levi's theory, an option X in **M** is E-admissible if and only if, for some determinate probability function P in **P**, X is "Bayes" with respect to P. That is, if and only if X maximizes the P-expected utility with respect to the options in **M**. Levi (1974) established that E-admissibility is *not* a binary decision rule. Walley (1990 T 3.9.5)] showed that when the menu **M** is a convex set of random variables and **P** is a closed, convex set of probabilities, then E-admissibility reduces to pairwise comparisons. In our (2003) paper, which included Levi as a co-author, we extended Walley's result but—in what is relevant to this summary—also we showed that E-admissibility may be non-binary even when the option space M is convex and the credal set **P** is convex, if it is *not* closed. That is, we showed how, by going beyond binary comparisons for determining admissibility, *E*-admissibility distinguishes among some pairs of convex credal sets even when those credal sets have all the same supporting hyperplanes. This revelation led us (2007) to the realization that, quite generally, Levi's *E*-admissibility rule can distinguish between *each* two distinct IP sets of probabilities, regardless whether or not those sets are convex, or even connected. Each IP set of probabilities has its own, unique pattern of *E*-admissible options when considering even finite decision problems.

A-C-W: You have worked in many topics within philosophy and statistics. But, as you say, a magician has only so many tricks. What are your major tricks? That is: which topics do you consider your major ones?

Teddy: It took us nearly 20 years to see the basic fact reported above. So, rather than having a bag of tricks that we play, instead we rely on patience and persistence to identify and then to examine tacit premises in familiar arguments. Sometimes that method reveals new insights. I'll mention one example that I find particularly salient.

Subjective Bayesian theories, such as de Finetti's (1974), Savage's (1954) and Anscombe-Aumann's (1963), have a common theme: Identify an agent's degrees of belief, her/his credence function, based on her/his coherent preference relation over acts. Acts are functions from states of uncertainty to outcomes. De Finetti's theory relies on coherent previsions: preferences over random variables. Savage's relies on a preference relation over acts that satisfies his first 6 postulates. And Anscombe-Aumman's theory is based on an axiomatized preference relation over horse-lotteries. In each theory, by treating constant acts as constant in value, each coherent preference relation over a domain of act may be represented as a Subjective Expected Utility, for a unique personal probability over states of uncertainty. But as we noted in Schervish et al. (1990), each of the familiar representation theorems identifies a unique personal probability as the agent's credal state but only as a function of which acts are treated as constant in value.

For instance, when acts are monetary gambles, which *numeraire* is to be treated as constant in value over states of uncertainties? The familiar decision theories offer no guidance. That the exchange rates between two standard currencies is not a constant random variable is sufficient to illustrate the conceptual challenge that is left unresolved by the familiar SEU theories, mentioned above. Is a constant gamble that has for its outcome (US) \$1 constant in value? Or is a gamble that has as its outcome 1 Euro constant in value? Depending upon which currency is used as the "rigid rod" for value, the representation of the same preference relation over gambles yields different personal probabilities. How to separate belief from value with these familiar theories? This issue is inherited, but not solved, within contemporary IP theory.

A-C-W: Here are a few questions about more technical issues.
There is now wide controversy on the role of p-values. But you have been discussing them for a long time, and warning about some of their flaws. What is your take on them?

What are your current views of sequential decision making?
You have done major foundational work on decision theories that take into account indeterminacy and imprecision in probabilities. What do you see as the future of this endeavor? What should be pursued next?

Teddy: You mention p-values, which is just one Classical statistical procedure that is hard to reconcile with core Bayesian principles. From the perspective of statistical *inference*, as early as the late 1930s, Jeffreys' (1961, Chaps. 5 and 6) revealed clear differences between Bayesian and Classical hypothesis testing. DeGroot's 1973 diagnosis is hard to best.

Mark, Jay, and I employed a decision-theoretic perspective in order to pose a different question about hypothesis testing. The familiar normative decision theories that serve as the foundations for Bayesian methods use a dichotomous criterion of rationality. For example, in de Finetti's theory, either the agent's previsions are coherent or else they are incoherent—where the standard of coherence is, roughly, the requirement that preference respects uniform dominance (relative to the privileged partition by states). Then, for example, the practice of using a fixed (e.g. .05) level tests regardless sample size is incoherent. But is that the end of the debate? We answer, "No!"

In several papers, dating from the early 2000's, we explored an idea that may be described as examining *degrees* of incoherence. For instance, in Schervish et al. (2000), we asked the question: How incoherent is the practice of using a fixed significance level in, say, an elementary case of a test of a simple null hypothesis versus a simple alternative hypothesis? We were surprised by the answer our theory gave: Not very incoherent! What then, from a Bayesian point of view, is the right concern with the classical practice of fixed level testing? By our lights, it is not how incoherent that practice is but, rather, what are the implicit priors that attach to fixed level testing with increasing sample sizes. With a fixed level test, as sample size increases the implicit prior for the two hypotheses tested approaches an extreme, 0–1 "prior" distribution. But if, in order to minimize the degree of incoherence in the practice of fixed level testing the investigator is committed to an extreme "prior" with increasing sample size, then why bother even to test?

A-C-W: You are one of the central figures in the study of indeterminate and imprecise probabilities. The conference on this topic, ISIPTA, which you have chaired, is now more than twenty years old. And the related society, SIPTA, whose president you had been from 2009 to 2013, will be twenty years old soon. What are your current thoughts on this topic and on the community?

Teddy: I am excited that SIPTA has grown up as a *Society*: It has an increasing, active membership, and since 2014 it has found new, youthful leadership, with fresh and stimulating ideas. The research reported at the 11th biennial meeting, ISIPTA-19, reflects a broadening and deepening of the ideas presented at the initial 1999 meeting. Indeed, the progress over those 20 years is impressive. I look forward, in particular, to seeing contributions that expand the range of applications of IP theory. Of course, that will require novel breakthroughs in IP statistical computing. In that, I will be an enthusiastic spectator!

A-C-W: The theory of finitely additive probability measures has been visited by you many times across the years. Do you feel it should replace countably additive measures in our teaching and our practice, or not? How about conglomerability—should it be a central issue in this debate, or not? Are there other more important issues here?

Teddy: As you remark, and as I've confirmed with this brief intellectual history, Jay, Mark, and I began our 3-way collaboration in 1979 working on non-conglomerability for finitely additive conditional probabilities. Forty years later, we continue to be stimulated by ideas relating to finite additivity. I'll mention two recent ones here, as I think they may be of interest to some who have gotten this far into this story,

The anomaly of non-conglomerability of conditional probability for an event E obtains when there is an indexed partition $H = \{h_i : i \in I\}$, with I serving as the index set, where the unconditional probability of E, $P(E)$, lies outside the closed interval of the conditional probabilities $P(E|h_i)$ over the partition H. That is, let $P_*(E|H) = \inf_{i \in I}\{P(E|h_i)\}$ and let $P^*(E|H) = \sup_{i \in I}\{P(E|h_i)\}$. Non-conglomerability occurs when $P(E)$ does not belong to the interval of values, $[P_*(E|H), P^*(E|H)]$. When conditional probabilities satisfy the de Finetti/Dubins' (1975) conditions of coherence, then each finitely additive but not countably additive probability, each *merely* finitely additive probability fails conglomerability for some event E in some denumerable partition H, i.e. where $|H| = \aleph_0$.

When P is merely finitely additive, then the unconditional probability function P fails to be additive in some denumerable partition. What we saw in Schervish et al. (2017) is that rather than being a phenomenon that is an anomaly peculiar to merely finitely additive probabilities, non-conglomerability of coherent conditional probabilities follows the degree of additivity of the unconditional probability. For instance, if P is a countably additive but not $\aleph_1$-additive unconditional probability function, then for some event, P's coherent conditional probabilities fail to be conglomerable in some uncountable partition H, with $|H| = \aleph_1$. In general, unless P is perfectly additive, i.e. unless the set N of null-events is itself a null event, then P suffers non-conglomerability in a sufficiently large partition.

A second, recent investigation, Schervish et al. (2020), involving merely finitely additive probabilities evolved as a complete surprise, while we were working on a somewhat delicate aspect of proper scoring rules. In Schervish et al. (2009), we came across probabilistic forecasting problems with strictly proper scoring rules where an incoherent forecast was dominated, but only by other incoherent forecasts. No coherent forecast dominated. In order for this circumstance to arise, however, the scoring rule has to display discontinuity: for some forecasts, an arbitrarily small change in the forecast causes a jump in the score. But a merely finitely additive probability also displays discontinuity. The two discontinuities line up in the sense that a finitely additive mixed strategy forecast can match its discontinuity with the discontinuity of the scoring rule. Then, if a forecast is incoherent it is dominated by some coherent finitely-additive forecast. And this phenomenon is quite general. It obtains for all decision problems with losses bounded below.

What our (2020) paper reports is that, by using finitely additive mixed strategies, in the fashion of Wald's (1950) criterion of admissibility with respect to risk in statistical decisions, we established a general *Complete Class Theorem*, a general *Minimax Theorem*, and an extension of (uniform) dominance in to each decision that fails to have a Bayes' model. In other words, if a specific option fails to maximize expected utility no matter "prior" is adopted, then some finitely-additive mixed strategy uniformly dominates. With finitely additive mixed strategies we channeled Wald through de Finetti. How reassuring is that?!

Allow me here to conclude this short story, but without wishing it to end. I continue having too much fun with old and new collaborations. I repeat my gratitude to those scholars who contributed their time and energies to this volume. The celebration begins, now for readers to study and to enjoy what they created.

Pittsburgh, PA
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Chapter 2
The Value Provided by a Scientific Explanation

Ruobin Gong, Joseph B. Kadane, Mark J. Schervish, Teddy Seidenfeld, and Rafael Bassi Stern

Abstract In this essay we investigate how deductive and probabilistic explanations add value to the theory providing the explanation. We offer an analysis of how, e.g., an explanation in the form of an abduction differs from a mere prediction in this regard. We apply this analysis to respond to a challenge posed by Glymour (Theory and Evidence. Princeton University Press, Princeton, 1980) regarding Bayesian confirmation of new theories using old data. Last, we consider additional criteria involving subjunctive conditionals and counterfactual conditionals for distinguishing explanations from mere predictions, which help illuminate why an explanation carries different cognitive value than does a mere prediction.

2.1 Introduction

There are varieties of explanatory forms in the sciences. Two forms that are commonly examined are *deductive* and *probabilistic* explanations. (Two classic twentieth century treatises on the philosophy of scientific explanation are Braithewaite's (1953) and Nagel's (1961). Also, see Koslow's (2019, Chap. 8).) In Sects. 2.2 and 2.3 we examine, respectively, how a deductive or a probabilistic explanation adds value to the theory providing the explanation. And we examine how an

R. Gong
Department of Statistics, Rutgers University, New Brunswick, NJ, USA
e-mail: ruobin.gong@rutgers.edu

J. B. Kadane · M. J. Schervish
Department of Statistics, Carnegie Mellon University, Pittsburgh, PA, USA
e-mail: kadane@stat.cmu.edu; mark@stat.cmu.edu

T. Seidenfeld (✉)
Departments of Philosophy and Statistics, Carnegie Mellon University, Pittsburgh, PA, USA
e-mail: teddy@stat.cmu.edu

R. B. Stern
Departamento de Estatística, University of São Paulo, São Paulo, Brazil
e-mail: rbstern@gmail.com

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explanation differs from a mere prediction in this regard. In Sect. 2.4 we apply this analysis to respond to a challenge posed by Glymour (1980) regarding Bayesian confirmation of new theories using old data. In Sect. 2.5 we consider additional criteria for distinguishing explanations from mere predictions, which addresses why an explanation carries different cognitive value than does a mere prediction.

2.2 Deductive-Nomological[1] [D-N] Explanations

2.2.1 Three Kinds of D-N Explanations

Nagel's first illustration of the deductive model of explanation is a derivation of an elementary arithmetic generality: A derivation of the generality that the sum of the first k odd integers is the perfect square k^2. What is being explained here, the *explanandum*, is a specific arithmetic law that applies to each of infinitely many cases. The explanation is a deduction of that law from a set of more general mathematical laws that serve as the premises of a logical argument.[2]

A more ambitious version of this kind of explanation is found in Sect. 9 of Hilbert's (1971) familiar model of Euclidean plane geometry E using a field of algebraic numbers A: the countable set of numbers arising from finitely many applications of addition, subtraction, multiplication, and division, along with recursion over $|\sqrt{(1+\omega)^2}|$, starting with the integer $\omega = 1$. In Hilbert's model, all of traditional Euclidean plane geometry E is explained (i.e., derived) with this approach, including the parallel postulate. In this model, Euclidean points **A, B**, $\ldots$, are identified with ordered pairs of algebraic numbers (x, y). Lines **a, b, . . .** are identified with ratios of triples of algebraic numbers $(u : v : w)$, where not both $u = 0 = v$. To say that the point (x, y) lies on the line $(u : v : w)$ means that $ux + vy + w = 0$, etc. Then Euclidean plane geometry E, the *explanandum*, is shown to be relatively consistent with a countable model of algebraic numbers A, which entails E under the translation scheme noted above.

A third kind of deductive explanation is illustrated by a species of what Peirce (1955, Chap. 11 in the 1955 collection) calls *abduction*. Here is the deductive form of abduction: A surprising fact F is noted. F cannot be explained based on settled background assumptions, which contributes to *F's* status as a *surprise*. Hypothesis H is proposed to explain F, where H is comprised of lawlike generalities that, together with settled background assumptions provide a deductive explanation of F.

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[1] We follow common usage that scientific laws are more than mere "accidental" generalizations, and the phrase 'nomological universals' designates the added status. See, e.g., Nagel (1961, Sect. 4.1).

[2] We imagine that explanation might run as follows. Use the more general arithmetic law that the sum of the first k positive integers, $1 + 2 + \ldots + k$, equals $k(k+1)/2$ to show that the sum of the first k positive odd integers, $\sum_{n=0}^{k-1}(2n+1)$, equals $2 \cdot [\sum_{n=1}^{k-1} n] + k = 2[(k-1)k/2] + k = k^2$.

Then H is made worthy of further assessment, e.g. H is now worthy of testing with new, experimental data, because of its value as a potential explanation of F.

What is gained by such deductive explanations? The answer depends upon which question is asked. If the underlying question is, e.g., how to determine the sum of the first k odd integers, the deductive explanation may include a schema for computation. If the underlying question is about consistency of, e.g., Euclidean plane geometry, the deductive explanation may show relative consistency, just as Hilbert showed using a reduction of Euclidean geometry to another mathematical theory whose primitives, algebraic numbers, do not include geometric concepts. And as a bonus, computational methods in the reducing theory, e.g., the determinant of n linear equations in n variables, can be used in higher dimensional geometry to identify the dimension and the volume of the parallelotope determined by those n linear equations: where the parallelotope is the induced mapping of the n-dimensional unit square. And the same deductive explanation can provide the answer to more than one of these questions.

If the question is in the form "Why the surprising fact F?" then H is a candidate for a deductive explanation and, so, H rises to the status of being a test-worthy hypothesis. H might also allow prediction of a future F-episode, which provides one schema for testing H. It is our purpose in this paper to understand the value of such deductive explanations noting that, in each of these three cases, the value provided by a deductive explanation does not require uncertainty about the explanandum.

2.2.2 Explanation and Prediction: A Necessary Condition for an Explanation

Important for our purposes, however, is to distinguish an explanation from a mere prediction (or post-diction). In the case of an explanation, at least one of the premises essential to the derivation of the explanandum is *lawlike* or *nomic*. The contrast is between a generalization, e.g., G: All A's are B's, and the enhanced claim that G also is *lawlike*.

We follow Braithewaite's and Nagel's proposals for distinguishing these as follows. A necessary condition **N** for a generalization G to rise to the status of a lawlike assertion is that

[**N**] Either G is a postulate of a theory T or is explained within that theory.

That is, as a necessary condition for G to serve as a law in an explanation that, e.g., a specific A is a B, is the requirement, **N** that G is a consequence within T of some higher level lawlike generalizations, or is fundamental to T. (The postulates of T are assumed lawlike.) Otherwise, if G fails this condition, it provides merely for a prediction that a particular A also is a B. But then G does not *explain* the A-B pattern. Then it merely provides reason to predict that an A is a B.

When considering empirical theories, **N** is not a sufficient condition for *law-likeness*, as is illustrated using an example voiced by Russel (1921, Lecture

5) in connection with his concerns about non-uniqueness of causes. (See, also Braithewaite's 1953, pp. 306–8 discussion of this example.) Here is Russell's example, adapted to our purposes.

We seek an explanation for why the workers at a late nineteenth Century Factory #1 go to lunch at approximately noon on workdays. The intended explanation is a derivation of this pattern of behavior from two lawlike generalizations, *H* and *P*:

H: Factory #1's horn sounds at about noon on workdays.

and

P: Workers at Factory #1 know it is lunchtime when they hear the factory horn.

Russell's example presumes a commonsense background theory T_1 of Industrial Organization that includes these two generalizations, *H* and *P*, as lower level generalizations about Factory #1. (Hence, each of *H* and *P* satisfies condition **N**.) Theory T_1 quantifies over various classes of factories, their methods of communicating, and workers. By design, the factories in a class have a similar organization.

Suppose that this "Russellian" background theory T_1 includes the assumption that Factory #2 is organized for communicating lunch times to its workers just as is Factory #1. The two factories belong to the same class. Their clocks and horns are locally powered and independently coordinated with a common local time. Assume, further, that the background assumptions for the example include the commonplace fact of where the factories are located, and that Factory #2 is located 50 miles to the north of Factory #1. The two factories are located well out of the range of the other's horns. But then, as Russell noted, T_1 also entails the following generalization *H*'.

H' On workdays, the workers at Factory #1 go to lunch when the horn sounds at Factory #2.

Modified slightly from Russell's original point, the generalization *H'* also satisfies the condition **N** for *lawlikeness*, as it too is a deductive consequence of theory T_1. But unlike the lawlike generalizations *H* and *P*, intuitively, *H*' is *not* lawlike. Though *H*' is adequate for predicting when workers at Factory #1 go to lunch on a typical workday, it fails to satisfy relevant *subjunctive conditionals* that also are required, we believe, if a generalization is to serve as an explanation. We formulate one such subjunctive conditional as follows:

S If the horn at Factory #2 *were* to sound at time *t* within the 1/2 hour interval 11:45 AM–12:15 PM, then the workers at Factory #1 *would* go to lunch at time *t*.

There are at least two relevant ways we understand that background theory T_1 defeats the subjunctive conditional, *S*. Based on T_1, we expect that each of the following obtains: