Simulating Copulas: Stochastic Models, Sampling Algorithms, and Applications, 2nd ed. Jan-Frederik Mai, Matthias Scherer, with contributions by Claudia Czado, Elke Korn, Ralf Korn, and Jakob Stöber. World Scientific Publishing Co. Pte. Ltd., 2017, xvii+338 pp., $118.00 (H), ISBN: 978-9-81-314924-3.

It has been 60 years since the theorem at the heart of copula-based modeling was published in Sklar (1959). But it is over the past two decades that interest in the subject increased exponentially. And while 10 years have passed since Li (2000) was (in)famously called out as the formula that killed Wall Street (Salmon 2009), copulas are alive and well. In fact, thanks to their ability to capture complex dependence structures, they are used to solve an increasingly wider range of scientific problems. A result of this continued success is that the second edition of Simulating Copulas: Stochastic Models, Sampling Algorithms, and Applications is a welcome addition to the literature on copulas.

All in all, I think that it is an important research book that complements nicely standard references on the topic. While not aiming at exhaustivity on copulas (e.g., statistical inference is not discussed), the material that the book focuses on is treated rigorously; and yet the authors do not overburden the reader with unnecessary technical details. I especially enjoy the focus on families for which the extendible subfamily can be conveniently expressed. In my opinion, this central unifying theme provides a refreshing and eloquent presentation of a topic for which many books are already available: it is what truly makes this book worth paying attention to.

I also like the level of details for the chapters on Archimedean, elliptical and Marshall-Olkin copulas, as well as the additional chapter related to families with known extendible subclass. Those four chapters (2, 3, 4, and 8), while not solely focused on sampling, form in my opinion the core of the book, organized around the central focus mentioned in the previous paragraph. While an in-depth treatment of Archimedean and elliptical copulas is common in other books on the topic, the focus here is unexpectedly novel and fits nicely within the book’s central theme. As for the detailed presentation of Marshall–Olkin copulas and the additional chapter on the creation of new families, this content is, to the best of my knowledge, missing or barely addressed in standard books on copulas.

While not specific to the second edition, I also like that there is a chapter on pair-copula constructions (Chapter 5). It is unusual for generic books on copulas to mention this topic, let alone to dedicate it a full chapter. My main criticism on this chapter is that it does not appear to have been updated in the second edition. Given the amount of research that has been published in the past 5 years, a lift-up of the chapter would have been a welcomed improvement.

Similarly as above, the following comment is not specific to the second edition: I must admit that I do not find the presentation of univariate sampling (Chapter 6) to be judiciously located in the book. While I understand that the authors could not use contributed chapters as an opener, I think that challenges related to sampling univariate data should appear prior to those related to multivariate data. That being said, the content itself fits well within the context of the book.

In terms of audience, I believe that the level is that of an advanced undergraduate student or a graduate student in mathematics, applied mathematics, or statistics. I think that the book strikes a delicate balance between too technical to be understood by students, and not deep enough to be interesting to (non-applied) researchers. And although, as mentioned above, I would not describe it as a textbook, the general presentation on copulas (Chapter 1) provides a thorough introduction for readers that are unfamiliar with the topic. One caveat is that, given the lack of references to inferential procedures, readers that would enjoy the book more are mathematicians, or statisticians specifically working on copula-related topics. Following the proofs and even understanding some of the sampling algorithms require the concepts of linear algebra and real analysis usually expected to be mastered by the kind of students mentioned above. As for the required ideas from probability theory, there are some topics that go, in my opinion, beyond the basics (e.g., stochastic processes like Lévy subordinators).

As a general comment, I think that it is an interesting and well-written book. But I would only use it as a supplementary resource for a course on simulating copulas. The reason is because it was not, in my opinion, written as a textbook. While I am not aware of the authors’ intent, my intuition stems from the fact that the preface does not contain mentions of or suggestions about organizing a course around this book. Additionally, while pseudo-code is useful, there are no actual code examples or companion software, which I think would be crucial to teach a course on a topic that has so many practical applications. Similarly, there are no suggested exercises, which I find extremely useful for instructors.

A colleague once said something along the lines of copulas having only three use-cases: simulation, simulation, simulation. While I find this view somewhat extreme, it captures well the idea that the material presented in this book, namely sampling copulas, is crucial to their success. Simulating Copulas: Stochastic Models, Sampling Algorithms, and Applications is therefore worth reading for its topicality by anyone wanting to know more about sampling copulas. And I would additionally recommend its unusual presentation of copulas around extendible subfamilies.

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The Book of Why: The New Science of Cause and Effect. Judea Pearl and Dana Mackenzie. New York: Basic Books, 2018, x+418 pp., $32.00 (H), ISBN: 978-0-46-509760-9.

Judea Pearl is a giant in the field of causal inference, whose many contributions, including the discovery of the d-separation criterion, have been immeasurably valuable. He, along with
science writer Dana Mackenzie, has written an important book that relates Pearl's work to a broad audience and makes an argument for its place in the scientific canon.

The book recounts the history of the Causal Revolution. The reader is told that causal inference was in a sorry state of affairs for most of the twentieth century. The scientific community was unable to tackle even the most basic causal inquiry, with grave consequences. For example, when discussing how scientists could not reach an agreement about whether smoking causes lung cancer, the book notes that "millions of lives were lost or shortened because scientists did not have an adequate language or methodology for answering causal questions" (p. 19). However, during this age of darkness, a small light of hope was burning in the form of Sewall Wright and a few other brave men. The light was the spark of the Causal Revolution in the 1930s. The revolution consisted of the introduction of a graphical causal representation in the form of directed acyclic graphs (DAGs), and a set of associated tools, by Pearl and his coauthors. Scientists were finally given the language and methodology they needed to conduct serious causal investigations, and apart from a few pockets of resistance, the scientific community rejoiced:

There is now an almost universal consensus, at least among epidemiologists, philosophers and social scientists, that (1) confounding needs and has a causal solution, and (2) causal diagrams provide a complete and systematic way of finding that solution. (p. 141)

As an autobiographical and expositional text for those working in the field, the book is both informative and entertaining. We are, however, concerned that the presentation will mislead readers whose first acquaintance with the subject is this book. The goals of this review are twofold: to highlight instances where naive readers, especially policy-makers, might be led to unrealistically optimistic conclusions; and to discuss alternative models that are overlooked in Pearl's account of the causal revolution.

**Unfounded Optimism About Causal Models**

The book paints a picture of a field that has come to its conclusion. Causal inference, for most intents and purposes, is solved. The optimism is appealing: the world is just waiting for scientists to uncover its mysteries. A scientific revolution will follow Pearl's causal revolution, at least among those that adopt his language and methodology.

We believe this optimism is unfounded. The central problem that scientists face, especially in the social sciences, is not how to express or analyze causal models but how to pick one that is valid or at least reasonable. The book does not claim to solve this problem. For example, discussing Henry Niles' critique of Sewall Wright's research, Pearl writes:

> Many people still make Niles' mistake of thinking that the goal of causal analysis is to prove that X is a cause of Y or else to find the cause of Y from scratch. That is the problem of causal discovery ... In contrast, the focus of Wright's research, as well as this book, is representing plausible causal knowledge in some mathematical language, combining it with empirical data and answering causal queries that are of practical value. Wright understood from the very beginning that causal discovery was much more difficult and perhaps impossible. (pp. 79–80)

The topic of the book is causal analysis, not causal discovery. In Pearl's model and calculus, the underlying causal structure is assumed to be known. The issue is that scientists often disagree on this structure. Pearl's approach may help clarify exactly where the disagreement lies, but it will not provide adjudication.

The smoking-lung cancer debate, recounted in Chapter 5, is a good illustration. Following Pearl's recipe, the scientific community in the 1950s should have represented the current consensus about the plausible causes of cancer (and all other relevant aspects of the causal nexus) in a DAG. After applying Pearl's calculus, the causal relationships of interest could have been investigated empirically, and the debate would have been resolved. The recipe fails, however, already on the first step. As captured in the exchange between Ronald Fisher and Jerome Cornfield, the central question of the debate was which causal model was the plausible one. Fisher held it possible (or plausible) that a gene was causing both smoking and lung cancer, and Cornfield disagreed. Diagrams 5.1 and 5.2 in the book (p. 176) provide a graphical representation of their positions. The disagreement was, however, not founded in a misunderstanding, so the clarification provided by the DAGs would have been of little use.

The heart of the problem is that large parts of the scientific community share Pearl's skepticism about the prospects of causal discovery. In our experience, the modes of inquiry that adjudicate debates avoid the problem altogether. Cornfield's sensitivity analysis, which played an important role in resolving the smoking and lung cancer debate, is one such approach. Pearl acknowledges its value, but the approach is outside of his dichotomy of causal analysis and causal discovery. The causal structure (e.g., as encoded in a DAG) is not presumed to be known here, nor is that structure the target of our inferences, as in causal discovery. Instead, sensitivity analyses can remain somewhat agnostic about the underlying structure. This agnosticism is one reason they are useful; sensitivity analyses can adjudicate debates because scientists can agree on their validity without reaching full agreement over what constitutes plausible causal knowledge.

The book's treatment of randomized controlled trials (RCTs) also fails to demonstrate an appreciation for this central problem. Pearl writes:

> Once we have understood why RCTs work, there is no need to put them on a pedestal and treat them as the gold standard of causal analysis, which all other methods should emulate. Quite the opposite: we will see that the so-called gold standard in fact derives its legitimacy from more basic principles. (p. 140)

For the purposes of causal analysis in Pearl's model, there is no distinction between a randomized experiment and any similarly unconfounded treatment assignment. However, this neglects that the successful implementation of an experiment ensures the assumption of unconfoundedness in a manner that can survive scrutiny from even the most determined skeptic. We can move from a metaphysical discussion about the correct causal model to a practical discussion about the experimental protocol and
whether it was followed. Experiments allow us to be largely agnostic about the causal structure.

The problem with these more agnostic modes of inference is their limited applicability. As Pearl notes, we can do less without a rich causal model. No one doubts the usefulness of Pearl's framework in situations where it can be applied. What naive readers might miss is that these situations are rare, particularly in the social sciences. And in the cases where reasonable consensus about the causal structure can be reached, Pearl's calculus will often tell us that it is not possible to draw inferences. For example, as noted in the book, Fisher's claim that genetic disposition confounds the smoking-lung cancer relationship was correct. A graph alone cannot encode that this confounding is weak, and Pearl's calculus would have told us that progress was not possible until these genes were identified and measured.

These realizations are sobering: contrary to the impression given to readers, causal inquiry cannot be reduced to a mathematical exercise nor automatized. Causal inference is possible, but it is a daunting task best served by modesty and humility.

Pluralism in Causal Inference

Pearl's self-described “Whig history” of causal inference is selective and narrow. Besides his own contributions, the book focuses on the contributions of his intellectual ancestors. Antagonists are occasionally brought on the stage, but only for the purpose of being proven wrong. Readers will easily be under the impression that the field has seen a slow but inevitable progression towards enlightenment, despite misguided resistance from the establishment, culminating with Pearl as a singular figure.

This account is misleading. No consensus, not even an emerging one, exists about the superiority of DAGs. Causal inference has its roots in many disciplines, and several conceptual frameworks and methodological approaches exist and thrive. Pearl reduces this pluralism to “cultural resistance” (p. 394). Scientists who resist DAGs are, however, not stubborn monks using their quills to defend a last stand against the printing press. The pluralism is instead a reflection of the range of challenges they face. The reason Pearl's model is not used more widely is that many scientists do not find it useful.

An extensive survey of the field is beyond the scope of this review.1 We will instead provide a few examples of alternative causal models and explain why scientists might prefer them. These approaches differ from Pearl's model in that they impose different amounts or different types of structure on the causal problem.

Robins (1986) developed a causal model that is closely related to Pearl's model and predates it. Both models are nonparametric structural equation models. The sole distinction is that Pearl's model invokes additional independence assumptions.2 These assumptions make his model more powerful, but they do not have testable implications. Pearl is quick to point the first part while neglecting the second. For example, Robins' model does not allow for identification of natural mediation effects, and Pearl notes that thanks to his model the “age-old quest for a mediation mechanism has been reduced to an algebraic exercise” (p. 20). The reason Robins did not make these assumptions is not because of a lack of imagination. He was reluctant to do so because they are too strong for the applications he has in mind, and because they cannot be verified even through experimentation. These are exactly the considerations scientists face. The additional assumptions do make the model more powerful, but they change its interpretation, and they make the analysis less robust. Pearl notes in passing that one must feel comfortable with his assumptions, but he neglects to tell the reader what they entail, being quick to instead give them a “green light” on mediation analysis (p. 334).

The next model is the framework our own methodological work has focused on, namely the design-based approach to causal inference. Drawing its origins from the work of both Jerzy Neyman and Fisher, scientists using this approach consider random allocation of treatment as the sole source of stochasticity. The framework is, in this sense, agnostic: it does not presume the existence of any stable laws of nature nor any infinite superpopulation from which we can draw new observations. Inferences are motivated primarily by knowledge of the experimental design, rather than by unverifiable statistical assumptions. As we noted above, however, agnosticism has costs. A design-based model focuses on a circumscribed task. Its purpose is to investigate causal effects of a well-defined intervention for a fixed population of units in one particular setting.

There are situations where scientists need more expressive models to solve their empirical problems. For example, parametric or semiparametric structural equation models can encode not only the existence (or nonexistence) of causal relationships but also how different variables are related. Scientists often find this useful when their substantive knowledge or theories suggest certain functional forms. An economist might, for instance, be comfortable assuming that an increase in income will not cause a reduction in total consumption. The usual trade-off applies, of course, and more elaborate models may introduce conceptual ambiguities and lack of robustness.

Pearl does not hide the fact that his calculus cannot exploit information about characteristics of causal relations, but he does not explain to what degree this limits its usefulness. An illustrative example lies with Pearl's discussion of the local average treatment effect interpretation of the instrumental variable method. The approach relies on a monotonicity assumption, which states that a causal effect is in the same direction for all units in the population. The assumption concerns the characteristics of an effect rather than its existence, so it cannot be encoded in a DAG. In Pearl's words:

1If such a survey were to be written, it could address the two most notable omissions in the book. The first is the contributions of a large group of scholars who were central to the development of modern causal inference. A partial list of this group is: Angrist, Ashenfelter, Campbell, Card, Heckman, Imbens, Manski, Murphy, Robins, and Rosenbaum. The second omission is estimation from data, which the book has a tendency to trivialize. Readers are not made aware of the fundamental difficulties in this enterprise. Under Pearl's model, it is impossible to estimate causal effects well without additional statistical assumptions. Statistical theory in this setting remains an active area of research.

2Robins (1986) also introduces notation and a calculus for causal effects, called g-notation and g-computation, that have direct parallels with Pearl's do-operator and do-calculus. In particular, Robins’ g-notation includes Pearl's do-operator as a special case.
In *do*-calculus we make no assumptions whatsoever regarding the nature of the functions in the causal model. But if we can justify an assumption like monotonicity or linearity on scientific grounds, then a more special-purpose tool like instrumental variables estimation is worth considering. (p. 257)

The statement is correct but misleading. A casual reader would be under the impression that these assumptions and the associated “special-purpose tools” are on the fringes of causal inference. On the contrary, instrumental variable methods are immensely popular in the social sciences. So are regression discontinuity and difference-in-differences designs, which are other methods relying on functional form assumptions (continuity and additivity, respectively). These methods are omitted from Pearl’s account of the causal revolution. If readers were made aware of their existence and popularity, they might question whether “causal diagrams provide a complete and systematic way of finding a solution [to confounding].”

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*Measuring Agreement: Models, Methods, and Applications*. Pankaj K. Choudhary and Haikady N. Nagaraja. Hoboken, NJ: John Wiley & Sons, Inc., 2017, xvii+336 pp., $119.50(H), ISBN: 978-1-11-807858-7.

Method comparison studies are very important in health-related fields and other disciplines. This type of study aims to measure the agreement between new techniques, tools or methods with an established method known as the gold standard. If the methods agree well enough, then the one that is cheaper, more convenient, less invasive, or simpler to use will be selected. This book provides statistical evaluations of agreement between two or more methods using common types of data collected through method comparison studies.

This book can serve as a textbook for graduate students as well as a reference for scientists in biomedical, social or behavioral fields and statisticians who are engaged in method comparison studies. To make the exposition more lucid, the book incorporates numerous hypothetical and real-world case studies. Each chapter is self-contained, and readers are expected to have some basic knowledge in statistics. The concepts required for understanding the contents include correlations, causal relationships, maximum likelihood estimation, hypothesis testing, and other basic concepts of statistics.

Chapter 1 is the introduction to the key concepts, statistical issues, and tools involved in analyzing data in method comparison studies. Simple examples using real-world case studies are provided. The chapter introduces the notational conventions to be used for the rest of the book. Chapter 2 discusses some common approaches for measuring agreement with continuous data. It includes concordance correlation coefficients, limits of agreement, and the total deviation index. Chapter 3 generally discusses issues on predicting, model fitting, and model diagnostics. This chapter may be omitted by readers who are familiar with mixed-effects modelling and large-sample inferences. The same chapter also introduces the technical foundations for the remainder of the book.

Chapters 4–9 discuss various types of experiments and study designs. The methodology of the analysis of paired measurement data, comprising two models (mixed-effects and bivariate normal), is discussed in Chapter 4. Repeated measurement data, with focus on whether data were linked or unlinked, are further discussed in Chapter 5. Measurements are assumed to be homoscedastic in both Chapters 4 and 5. Chapter 6 incorporates heteroscedasticity by defining the variances to be functions of a suitably defined variance covariate. Chapter 7 extends method comparison studies to more than two methods by generalizing the models in Chapters 4 and 5. In many real-world problems, additional information on the number of possibly important covariates is available, hence Chapter 8 is dedicated to the discussion of a unified method that incorporates covariates in the analysis. Chapter 9 deals with the longitudinal data of two methods, which is an extension of the discussion in Chapter 5 so as to allow time effect on the measurements.

Chapter 10 deals with data that do not satisfy the assumption of normality, hence a nonparametric approach is introduced for the evaluation of the similarities and agreement. As usual, there is no assumption about the shape of the data distribution. It does not require the underlying population to meet certain assumptions either. The same chapter also deals with unreplicated and unlinked repeated measurements of data. Chapter 11 briefly discusses a simulation-based methodology used for determining sample sizes. Finally, Chapter 12 is dedicated to method comparison methodologies where data are categorical, regardless of whether they are on the nominal or ordinal scale. While the book attempts to cover both continuous and categorical measurement data, only this one chapter looks at the categorical case.

The book has a good balance of theory and applications and is easy to follow. Case studies are used for illustrating the concepts and theories in almost all the chapters. Each chapter starts with a preview and ends with a chapter summary, bibliographic note, and exercises or study questions. These exercises are designed to help readers to master the methodological details so as to be able to apply the knowledge to their own problems. Unfortunately, the solutions to the questions are not available in the book, nor are they accessible as a manual for instructors. Nevertheless, as a supplement to the book, the R code and many of the datasets are publicly available at [http://www.utdallas.edu/~pankaj/agreement_book/](http://www.utdallas.edu/~pankaj/agreement_book/).

Overall, this book is a valuable addition to the existing literature on the topic of agreement evaluation. It will benefit readers who may not have a good statistical background as well