Preface

What is the maximum level a certain river is likely to reach over the next 25 years? What is the likely magnitude of the strongest earthquake to occur during the life of a planned nuclear plant, or the speed of the strongest wind a suspension bridge will have to stand? The present book does not deal with such fundamental practical questions, but rather with some (arguably also fundamental) mathematics which have emerged from the consideration of these questions. All these situations can be modeled in the same manner. The value X_t of the quantity of interest (be it water level or speed of wind) at time t is a random variable. What can be said about the maximum value of X_t over a certain range of t? In particular, how can we guarantee that, with probability close to one, this maximum will not exceed a given threshold?

A collection of random variables $(X_t)_{t \in T}$, where t belongs to a certain index set T, is called a stochastic process, and the topic of this book is the study of the supremum of certain stochastic processes, and more precisely to find upper and lower bounds for these suprema. The key word of the book is

INEQUALITIES.

It is not required that T be a subset of the real line, and large parts of the book do not deal directly with the "classical theory of processes" which is mostly concerned with this situation. The book is by no means a complete treatment of the hugely important question of bounding stochastic processes, in particular because it does not really expand on the most basic and robust results which are the most important for the "classical theory of processes". Rather, its specific goal is to demonstrate the impact and the range of modern abstract methods, in particular through their treatment of several classical questions which are not accessible to "classical methods".

The most important idea about bounding stochastic processes is called "chaining", and was invented by A. Kolmogorov. This method is wonderfully efficient. With little effort it suffices to answer a number of natural questions. It is however not a panacea, and in a number of natural situations it fails to provide a complete understanding. This is best discussed in the case of Gaussian processes, that is processes for which the family $(X_t)_{t \in T}$ consists of jointly Gaussian random variables (r.v.s). These are arguably the most important of all. A Gaussian process defines in a canonical manner a distance d on its index set T by the formula

$$d(s,t) = (\mathsf{E}(X_s - X_t)^2)^{1/2} . \tag{0.1}$$

Probably the single most important conceptual progress about Gaussian processes was the gradual realization that the metric space (T, d) is the key object to understand them, irrelevant of the other possible structures of the index set. This led R. Dudley to develop in 1967 an abstract version of Kolmgorov's chaining argument adapted to this situation. This provides a very efficient bound for Gaussian processes. Unfortunately, there are natural situations where this bound is not tight. Roughly speaking, one might say that "there sometimes remains a parasitic logarithmic factor in the estimates".

The discovery around 1985 (by X. Fernique and the author) of a precise (and in a sense, *exact*) relationship between the "size" of a Gaussian process and the "size" of this metric space provided the missing understanding in the case of these processes. Attempts to extend this result to other processes spanned a body of work which forms the core of this book.

A significant part of this book is devoted to situations where one has to use some skills to "remove the last parasitic logarithm in the estimates." These situations occur with unexpected frequency in all kinds of problems. A particularly striking example is as follows. Consider n^2 independent uniform random points $(X_i)_{i \le n^2}$ which are uniformly distributed in the unit square $[0,1]^2$. We want to understand how far a typical sample is from being very uniformly spread on the unit square. To measure this we construct a one to one map π from $\{1, \ldots, n^2\}$ to the vertices $v_1, \ldots, v_{n^2}$ of a uniform $n \times n$ grid in the unit square. If we try to minimize the *average* distance between X_i and $v_{\pi(i)}$ we can do as well as about $\sqrt{\log n}/n$ but no better. If we try to minimize the *maximum* distance between X_i and $v_{\pi(i)}$, we can do as well as about $(\log n)^{3/4}/n$ but no better. The factor $1/n$ is just due to scaling. It is the fractional powers of $\log n$ that require work.

Even though the book is largely self-contained, it mostly deals with rather subtle questions such as the previous one. It also devotes considerable energy to the problem of finding *lower* bounds for certain processes, a topic considerably more difficult and less developed than the search for upper bounds. Therefore it should probably be considered as an advanced text, even though I hope that eventually the main ideas of at least Chapter 2 will become part of every probabilist's tool kit. In a sense this book is a second edition (or, rather, a continuation) of the monograph [1], or at least of the part of that work which was devoted to the present topic. I made no attempt to cover again all the relevant material of [1]. Familiarity with [1] is certainly not a prerequisite, and maybe not even helpful, because the way certain results are presented there is arguably obsolete. The present book incorporates (with much detail added) the material of a previous (and, in retrospect, far too timid) attempt [2] in the same direction, but its goal is much broader. I am really trying here to communicate as much as possible of my experience working in the area of boundedness of stochastic processes, and consequently I have in particular covered most of the subjects related to this area on which

I ever worked, and I have included all my pet results, whether or not they have yet generated activity. I have also included a number of recent results by others in the same general direction. I find that these results are deep and very beautiful. They are also sometimes rather difficult to access for the non-specialist (or even for the specialists themselves). I hope that explaining them here in a unified (and often simplified) presentation will serve a useful purpose. Bitter experience has taught me that I should not attempt to write about anything on which I have not meditated enough to make it part of my flesh and blood (and that even this is very risky). Consequently this book covers only topics and examples about which I have at least the illusion that I might write as well as anybody else, a severe limitation. I can only hope that it still covers the state-of-art knowledge about sufficiently many fundamental questions to be useful, and that it contains sufficiently many deep results to be of lasting interest.

A number of seemingly important questions remain open, and one of my main goals is to popularize these. Of course opinions differ as to what constitutes a really important problem, but I like those I explain in the present book. Several of them were raised a generation ago in [1], but have seen little progress since. One deals with the geometry of Hilbert space, a topic that can hardly be dismissed as being exotic. These problems might be challenging. At least, I made every effort to make some progress on them. The great news is that when this book was nearly complete, Witold Bednorz and Rafał Latała solved the Bernoulli Conjecture on which I worked for years in the early nineties (Theorem 5.1.5). In my opinion this is the most important result in abstract probability for at least a generation. I offered a prize of $ 5000 for the solution to this problem, and any reader understanding this amazing solution will agree that after all this was not such a generous award (specially since she did not have to sign this check). But solving the Bernoulli Conjecture is only the first step of a vast (and potentially very difficult) research program, which is the object of Chapter 12. I now offer a prize of $ 1000 for a positive solution of the possibly even more important problem raised at the end of Chapter 12 (see also [3]). The smaller amount reflects both the fact that I am getting wiser and my belief that a positive solution to this question would revolutionize our understanding of fundamentally important structures (so that anybody making this advance will not care about money anyway). I of course advise to claim this prize before I am too senile to understand the solution, for there can be no guarantee of payment afterwards.

I am very much indebted to Jian Ding and James Lee, who motivated me to start this project (by kindly but firmly pointing out that they found [2] far too difficult to read), and to Joseph Yukich, whose unflinching advice helped me to make this text more of a book and less of a gigantic research paper.

I must apologize for the countless inaccuracies and mistakes, small or big, that this book is bound to contain despite all the efforts made to remove them. I was very much helped in this endeavor by a number of colleagues, and in particular by Albert Hanen who read the entire book. Very special thanks are also due to Tim Austin, Witold Bednorz, Jian Ding, Rafał Latała, Nicholas Harvey, Joseph Lehec, Shahar Mendelson and Marc Yor (to whom I owe in particular the idea of adding Appendix A). Of course, all the remaining mistakes are my sole responsibility.

I am happy to acknowledge here the extraordinary help that I have received over the last 10 years from Albert Hanen. During that period I wrote over 2600 pages of book material. Albert Hanen has read every single of them in complete detail, often in several versions, attempting with infinite patience to check every single statement. He has corrected thousands of typos, hundreds of mistakes and helped me clarify countless obscurities. Without his endless labor of love, my efforts to communicate would have been significantly less productive during this entire period. I am very grateful to him.

The untimely death of Marc Yor while this book was in production is an irretrievable loss for Probability Theory. Marc had donated much time to improve this work (as well as the author's previous two books), and it is only befitting that it be dedicated to his memory.

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Springer
http://www.springer.com/978-3-642-54074-5

Upper and Lower Bounds for Stochastic Processes
Modern Methods and Classical Problems
Talagrand, M.
2014, XV, 626 p., Hardcover
ISBN: 978-3-642-54074-5