It always irritates me to read a review in which the reviewer wishes the authors (or, in this case the editors) had produced a different book. Nonetheless, I will argue exactly that since they do not seem to have produced the book they claimed or even the book they intended. The preface is extremely brief (not over a half page) and not very informative. It essentially reduces to the statement that this anthology attempts to shed light on the questions of what are the important developments in present mathematics and where is it heading. In fact, only the first is addressed in any serious way, but that is fair since, as some sage said, prediction is hard, especially of the future.

What is the audience for the book? The editors bring up Hilbert’s 23 problems as having set the agenda for mathematics of the 20th century and claim that their book is intended to do the same for the 21st. All I can say to that is that if that is what 21st century mathematics is going to be, I am glad my career was in the 20th. By my (necessarily arbitrary) count, there were 22 (of 62) articles in physics, engineering and applied math, seven each in computer science and number theory, six in geometry, five in probability and statistics, three in economics and finance, and two each in algebra and analysis. The remaining ones were either interviews, a catch-all that, for want of a better term, I call philosophy, or not mathematics at all. Imagine that, more articles on economics than on either algebra or analysis!
I do not know what instructions were given to the authors nor what editorial control was exercised, but the level of the articles range from highly technical and unreadable except by an expert to one horror on mathematics in the entertainment industry that was written in a celebratory mode but was, from a mathematical viewpoint, content-free. I learned from that article that rotations of space can be described by unit quaternions, but not how. (It is by conjugation, thinking of 3-dimensional vectors as purely imaginary quaternions and there is a kernel, the subgroup \{1, -1\}. There is now more mathematics in this review than in the article in question.)

The articles are not numbered and they are not classified, but rather sorted alphabetically by author’s (first) name. I imagine I could find a more useless way of sorting them if I tried (by size, maybe). Since no one will want to (or be able to) read more than a fraction of the articles, a classification would have been very helpful. Notice how this contrasts with Hilbert’s list of problems, whose statements, at least, were generally accessible to all the mathematicians of his day.

I could not read every article, nor report on each one if I had, but I attempted a few and I will discuss those below.

The first article I looked at was called, “Mathematics of Financial Markets” by Mark Davis. The article discusses some models of stock prices. I had always thought that a mathematical model of a real world situation should be constructed in something like the following way. Study the situation in sufficient detail to understand how it works. Choose relevant variables and write down, as a result of the studies, equations that describe the interactions. Solve them, exactly if possible, numerically if necessary, and compare them with reality. If they fit, then you suppose that you have captured the situation in sufficient detail and you can say that you have an explanatory theory and use it to make further predictions. If they don’t fit, then go back to step one and refine your analysis. Judging from this paper, what you do is conjecture a conclusion, abandon it without discussing its accuracy and conjecture another conclusion until you find one that no one objects to. The first theory of stock price variations was that stock prices move by a process akin to Brownian motion. This led to certain predictions, although there is no discussion of how well it mirrors reality. The original model led to a Gaussian distribution of stock price movements. This model was eventually abandoned, not because it didn’t work (that question is not discussed), but because the tail of the Gaussian distribution would allow negative stock prices and stock prices cannot be negative! Is this a serious objection? To me it seems fatuous, since while stock prices cannot be negative, that is a figment of the laws that created limited liability corporations and the value underlying a stock can certainly be negative (cf. the Enron Corp.). At any rate, this “model” was replaced by a new model that led to an exponential distribution of prices that could not go negative. Does it have any actually predictive value? That does not seem to be discussed. The point I am trying to make is that these models are a priori rather than based on an analysis of the stock markets. In fact my reaction to the whole subject is summarized in, “Garbage in, garbage out”.

The next paper I looked at happened to be the immediate preceding paper, “Some open problems and research directions in the mathematical study of fluid dynamics”. Now this is genuine mathematics and, although I know nothing of the subject, I thought I might like to at least get an idea of how it worked. It started well enough with Navier-Stokes partial differential equation which is derived by identifying the relevant variables as pressure, density (assumed constant, at least as a first approximation) and velocity and applying Newton’s law. Another constant, viscosity, is used (although this depends on temperature, but I am not objecting to simplifying assumptions). The questions that concern him are the existence, uniqueness, and regularity of the solutions of these equations and the rest of the article is concerned with these questions. I would have liked to have seen a discussion of what failure of these questions actually means. Does lack of regularity imply turbulence or is something else involved? Since real systems lead to real behaviour, what could non-existence of solutions mean? If the solutions turn out to exist but be non-unique, does that mean that the physical system is indeterminate or that the equations have been oversimplified? Instead what we get are technical details that are likely to interest only experts for whom this survey is unnecessary.

The next article I attempted was “From finite sets to Feynman diagrams” by John Baez and James Dolan. Baez, a cousin of the famous folk singer, is well known for his regular internet posting, “This week in physics”, although his degrees and current affiliation are in mathematics. I was expecting something in physics. To my surprise what he wrote was an introduction to category theory. It begins with a clear and convincing—to me—explanation of why category theory ought to be interesting to mathematicians. He then develops a bit of the theory, including some ideas that will be new to most people. The whole article seems both clear and accessible, without omitting anything essential. It may not be the only such essay in this volume, but it is the only one I have read so far.

The last article I will mention explicitly is that by Marie-Françoise Roy, “Three problems in real algebraic geometry and their descendants.” If you are going to have a collection of this kind, this is pretty much a model article. It takes three problems (the first being the Hilbert problem of showing that every real polynomial in several variables that is never negative is a sum of squares of rational functions) and states them, discusses the motivation, gives a sketch of the mathematics involved in solving them and discusses further questions along the same lines. The article has mathematical content to be sure, but is not overly technical and explains why the questions are interesting and even how the future might develop. If only the other articles had been like this.