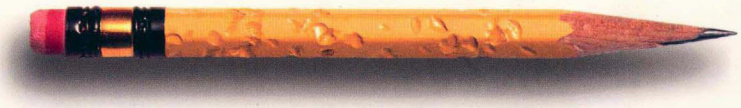
THINKING, FAST AND SLOW



DANIEL KAHNEMAN

WINNER OF THE NOBEL PRIZE IN ECONOMICS

THINKING, FAST AND SLOW

DANIEL KAHNEMAN

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THINKING, FAST AND SLOW

In memory of Amos Tversky

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THINKING, FAST AND SLOW

INTRODUCTION

Every author, I suppose, has in mind a setting in which readers of his or her work could benefit from having read it. Mine is the proverbial office water-cooler, where opinions are shared and gossip is exchanged. I hope to enrich the vocabulary that people use when they talk about the judgments and choices of others, the company's new policies, or a colleague's investment decisions. Why be concerned with gossip? Because it is much easier, as well as far more enjoyable, to identify and label the mistakes of others than to recognize our own. Questioning what we believe and want is difficult at the best of times, and especially difficult when we most need to do it, but we can benefit from the informed opinions of others. Many of us spontaneously anticipate how friends and colleagues will evaluate our choices; the quality and content of these anticipated judgments therefore matters. The expectation of intelligent gossip is a powerful motive for serious self-criticism, more powerful than New Year resolutions to improve one's decision making at work and at home.

To be a good diagnostician, a physician needs to acquire a large set of labels for diseases, each of which binds an idea of the illness and its symptoms, possible antecedents and causes, possible developments and consequences, and possible interventions to cure or mitigate the illness. Learning medicine consists in part of learning the language of medicine. A deeper understanding of judgments and choices also requires a richer vocabulary than is available in everyday language. The hope for informed gossip is that there are distinctive patterns in the errors people make. Systematic errors

are known as biases, and they recur predictably in particular circumstances. When the handsome and confident speaker bounds onto the stage, for example, you can anticipate that the audience will judge his comments more favorably than he deserves. The availability of a diagnostic label for this bias—the halo effect—makes it easier to anticipate, recognize, and understand.

When you are asked what you are thinking about, you can normally answer. You believe you know what goes on in your mind, which often consists of one conscious thought leading in an orderly way to another. But that is not the only way the mind works, nor indeed is that the typical way. Most impressions and thoughts arise in your conscious experience without your knowing how they got there. You cannot trace how you came to the belief that there is a lamp on the desk in front of you, or how you detected a hint of irritation in your spouse's voice on the telephone, or how you managed to avoid a threat on the road before you became consciously aware of it. The mental work that produces impressions, intuitions, and many decisions goes on in silence in our mind.

Much of the discussion in this book is about biases of intuition. However, the focus on error does not denigrate human intelligence, any more than the attention to diseases in medical texts denies good health. Most of us are healthy most of the time, and most of our judgments and actions are appropriate most of the time. As we navigate our lives, we normally allow ourselves to be guided by impressions and feelings, and the confidence we have in our intuitive beliefs and preferences is usually justified. But not always. We are often confident even when we are wrong, and an objective observer is more likely to detect our errors than we are.

So this is my aim for watercooler conversations: improve the ability to identify and understand errors of judgment and choice, in others and eventually in ourselves, by providing a richer and more precise language to discuss them. In at least some cases, an accurate diagnosis may suggest an intervention to limit the damage that bad judgments and choices often cause.

ORIGINS

This book presents my current understanding of judgment and decision making, which has been shaped by psychological discoveries of recent decades. However, I trace the central ideas to the lucky day in 1969 when I asked a colleague to speak as a guest to a seminar I was teaching in the De-

partment of Psychology at the Hebrew University of Jerusalem. Amos Tversky was considered a rising star in the field of decision research—indeed, in anything he did—so I knew we would have an interesting time. Many people who knew Amos thought he was the most intelligent person they had ever met. He was brilliant, voluble, and charismatic. He was also blessed with a perfect memory for jokes and an exceptional ability to use them to make a point. There was never a dull moment when Amos was around. He was then thirty-two; I was thirty-five.

Amos told the class about an ongoing program of research at the University of Michigan that sought to answer this question: Are people good intuitive statisticians? We already knew that people are good intuitive grammarians: at age four a child effortlessly conforms to the rules of grammar as she speaks, although she has no idea that such rules exist. Do people have a similar intuitive feel for the basic principles of statistics? Amos reported that the answer was a qualified yes. We had a lively debate in the seminar and ultimately concluded that a qualified no was a better answer.

Amos and I enjoyed the exchange and concluded that intuitive statistics was an interesting topic and that it would be fun to explore it together. That Friday we met for lunch at Café Rimon, the favorite hangout of bohemians and professors in Jerusalem, and planned a study of the statistical intuitions of sophisticated researchers. We had concluded in the seminar that our own intuitions were deficient. In spite of years of teaching and using statistics, we had not developed an intuitive sense of the reliability of statistical results observed in small samples. Our subjective judgments were biased: we were far too willing to believe research findings based on inadequate evidence and prone to collect too few observations in our own research. The goal of our study was to examine whether other researchers suffered from the same affliction.

We prepared a survey that included realistic scenarios of statistical issues that arise in research. Amos collected the responses of a group of expert participants in a meeting of the Society of Mathematical Psychology, including the authors of two statistical textbooks. As expected, we found that our expert colleagues, like us, greatly exaggerated the likelihood that the original result of an experiment would be successfully replicated even with a small sample. They also gave very poor advice to a fictitious graduate student about the number of observations she needed to collect. Even statisticians were not good intuitive statisticians.

While writing the article that reported these findings, Amos and I discovered that we enjoyed working together. Amos was always very funny,

and in his presence I became funny as well, so we spent hours of solid work in continuous amusement. The pleasure we found in working together made us exceptionally patient; it is much easier to strive for perfection when you are never bored. Perhaps most important, we checked our critical weapons at the door. Both Amos and I were critical and argumentative, he even more than I, but during the years of our collaboration neither of us ever rejected out of hand anything the other said. Indeed, one of the great joys I found in the collaboration was that Amos frequently saw the point of my vague ideas much more clearly than I did. Amos was the more logical thinker, with an orientation to theory and an unfailing sense of direction. I was more intuitive and rooted in the psychology of perception, from which we borrowed many ideas. We were sufficiently similar to understand each other easily, and sufficiently different to surprise each other. We developed a routine in which we spent much of our working days together, often on long walks. For the next fourteen years our collaboration was the focus of our lives, and the work we did together during those years was the best either of us ever did.

We quickly adopted a practice that we maintained for many years. Our research was a conversation, in which we invented questions and jointly examined our intuitive answers. Each question was a small experiment, and we carried out many experiments in a single day. We were not seriously looking for the correct answer to the statistical questions we posed. Our aim was to identify and analyze the intuitive answer, the first one that came to mind, the one we were tempted to make even when we knew it to be wrong. We believed—correctly, as it happened—that any intuition that the two of us shared would be shared by many other people as well, and that it would be easy to demonstrate its effects on judgments.

We once discovered with great delight that we had identical silly ideas about the future professions of several toddlers we both knew. We could identify the argumentative three-year-old lawyer, the nerdy professor, the empathetic and mildly intrusive psychotherapist. Of course these predictions were absurd, but we still found them appealing. It was also clear that our intuitions were governed by the resemblance of each child to the cultural stereotype of a profession. The amusing exercise helped us develop a theory that was emerging in our minds at the time, about the role of resemblance in predictions. We went on to test and elaborate that theory in dozens of experiments, as in the following example.

As you consider the next question, please assume that Steve was selected at random from a representative sample:

> An individual has been described by a neighbor as follows: "Steve is very shy and withdrawn, invariably helpful but with little interest in people or in the world of reality. A meek and tidy soul, he has a need for order and structure, and a passion for detail." Is Steve more likely to be a librarian or a farmer?

The resemblance of Steve's personality to that of a stereotypical librarian strikes everyone immediately, but equally relevant statistical considerations are almost always ignored. Did it occur to you that there are more than 20 male farmers for each male librarian in the United States? Because there are so many more farmers, it is almost certain that more "meek and tidy" souls will be found on tractors than at library information desks. However, we found that participants in our experiments ignored the relevant statistical facts and relied exclusively on resemblance. We proposed that they used resemblance as a simplifying heuristic (roughly, a rule of thumb) to make a difficult judgment. The reliance on the heuristic caused predictable biases (systematic errors) in their predictions.

On another occasion, Amos and I wondered about the rate of divorce among professors in our university. We noticed that the question triggered a search of memory for divorced professors we knew or knew about, and that we judged the size of categories by the ease with which instances came to mind. We called this reliance on the ease of memory search the availability heuristic. In one of our studies, we asked participants to answer a simple question about words in a typical English text:

> Consider the letter *K*.
> Is *K* more likely to appear as the first letter in a word OR as the third letter?

As any Scrabble player knows, it is much easier to come up with words that begin with a particular letter than to find words that have the same letter in the third position. This is true for every letter of the alphabet. We therefore expected respondents to exaggerate the frequency of letters appearing in the first position—even those letters (such as *K*, *L*, *N*, *R*, *V*) which in fact occur more frequently in the third position. Here again, the reliance on a heuristic produces a predictable bias in judgments. For example, I recently came to doubt my long-held impression that adultery is more common among politicians than among physicians or lawyers. I had even come up with explanations for that "fact," including the aphrodisiac effect of power and the temptations of life away from home. I eventually realized that the transgressions of politicians are much more likely to be reported than the

transgressions of lawyers and doctors. My intuitive impression could be due entirely to journalists' choices of topics and to my reliance on the availability heuristic.

Amos and I spent several years studying and documenting biases of intuitive thinking in various tasks—assigning probabilities to events, forecasting the future, assessing hypotheses, and estimating frequencies. In the fifth year of our collaboration, we presented our main findings in *Science* magazine, a publication read by scholars in many disciplines. The article (which is reproduced in full at the end of this book) was titled "Judgment Under Uncertainty: Heuristics and Biases." It described the simplifying shortcuts of intuitive thinking and explained some 20 biases as manifestations of these heuristics—and also as demonstrations of the role of heuristics in judgment.

Historians of science have often noted that at any given time scholars in a particular field tend to share basic assumptions about their subject. Social scientists are no exception; they rely on a view of human nature that provides the background of most discussions of specific behaviors but is rarely questioned. Social scientists in the 1970s broadly accepted two ideas about human nature. First, people are generally rational, and their thinking is normally sound. Second, emotions such as fear, affection, and hatred explain most of the occasions on which people depart from rationality. Our article challenged both assumptions without discussing them directly. We documented systematic errors in the thinking of normal people, and we traced these errors to the design of the machinery of cognition rather than to the corruption of thought by emotion.

Our article attracted much more attention than we had expected, and it remains one of the most highly cited works in social science (more than three hundred scholarly articles referred to it in 2010). Scholars in other disciplines found it useful, and the ideas of heuristics and biases have been used productively in many fields, including medical diagnosis, legal judgment, intelligence analysis, philosophy, finance, statistics, and military strategy.

For example, students of policy have noted that the availability heuristic helps explain why some issues are highly salient in the public's mind while others are neglected. People tend to assess the relative importance of issues by the ease with which they are retrieved from memory—and this is largely determined by the extent of coverage in the media. Frequently mentioned topics populate the mind even as others slip away from awareness. In turn, what the media choose to report corresponds to their view of what is cur-

rently on the public's mind. It is no accident that authoritarian regimes exert substantial pressure on independent media. Because public interest is most easily aroused by dramatic events and by celebrities, media feeding frenzies are common. For several weeks after Michael Jackson's death, for example, it was virtually impossible to find a television channel reporting on another topic. In contrast, there is little coverage of critical but unexciting issues that provide less drama, such as declining educational standards or overinvestment of medical resources in the last year of life. (As I write this, I notice that my choice of "little-covered" examples was guided by availability. The topics I chose as examples are mentioned often; equally important issues that are less available did not come to my mind.)

We did not fully realize it at the time, but a key reason for the broad appeal of "heuristics and biases" outside psychology was an incidental feature of our work: we almost always included in our articles the full text of the questions we had asked ourselves and our respondents. These questions served as demonstrations for the reader, allowing him to recognize how his own thinking was tripped up by cognitive biases. I hope you had such an experience as you read the question about Steve the librarian, which was intended to help you appreciate the power of resemblance as a cue to probability and to see how easy it is to ignore relevant statistical facts.

The use of demonstrations provided scholars from diverse disciplines—notably philosophers and economists—an unusual opportunity to observe possible flaws in their own thinking. Having seen themselves fail, they became more likely to question the dogmatic assumption, prevalent at the time, that the human mind is rational and logical. The choice of method was crucial: if we had reported results of only conventional experiments, the article would have been less noteworthy and less memorable. Furthermore, skeptical readers would have distanced themselves from the results by attributing judgment errors to the familiar fecklessness of undergraduates, the typical participants in psychological studies. Of course, we did not choose demonstrations over standard experiments because we wanted to influence philosophers and economists. We preferred demonstrations because they were more fun, and we were lucky in our choice of method as well as in many other ways. A recurrent theme of this book is that luck plays a large role in every story of success; it is almost always easy to identify a small change in the story that would have turned a remarkable achievement into a mediocre outcome. Our story was no exception.

The reaction to our work was not uniformly positive. In particular, our focus on biases was criticized as suggesting an unfairly negative view of the