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RESEARCH ARTICLE

Kahneman, Tversky, and Kahneman-Tversky: three ways of thinking

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ABSTRACT
This homage to Danny Kahneman and Amos Tversky describes how each of them thought about psychology. It outlines the principal results of their collaborative research, which was their most original and most influential. Why? In search of an explanation it examines their joint thinking during their collaboration.

KEYWORDS Decision making; dual systems; expected utility; judgement; probability; rationality

Preface

Two apologies: first, for any mistakes in this article, of which there must be at least one; and, second, to Aidan Feeney, the new editor of Thinking & Reasoning, who kindly invited me to write an article about Danny, but who got one about Danny and Amos instead. Five brave readers critiqued an earlier draft: Monica Bucciarelli, Ruth Byrne, Maya Bar-Hillel, Sunny Khemlani, and Aidan Feeney, and my revisions took into account many of their points. (Readers will know whom to blame for the mistakes that remain.) My thanks to them and to many other people who—whether they know it or not—have helped me with this article, foremost of course are Danny and Amos, whom I first talked to when they visited Peter Wason at University College London in 1969, but with whom I subsequently spent many happy hours—sometimes cheering up Danny when we were both at Princeton, and sometimes being cheered up by Amos when I spent two separate semesters at Stanford. He and I also passed several days walking and talking our way round Heidelberg, and on occasion advising an extra-mural committee, who left us alone most of the time. The analogy between Danny's and Amos's research and my colleagues and my efforts

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to separate reasoning from logic helped to maintain an intellectual bond among us. Thanks also to a fellow emigrant from the West Riding of Yorkshire, the late Anne Treisman, for advising me on pupil dilation, attention, and visual memory; to Maya Bar-Hillel, who was one of Amos's favourite students, for her understanding, her expertise on JDM, and being the sage of "stumpers"; and to Eldar Shafir for his knowledge of Amos and JDM when we co-taught a course on rationality (with the late Gil Harman). Finally, I am so grateful to Barbara Tversky for her kindness and wisdom, for her insights into spatial cognition and gestures and into our intellectual friends and foes, and for her unsurpassable knowledge of Danny and Amos.

For Barbara and for Maya

Danny Kahneman died at the age of 90 on March 27th this year. His death prompted several memoirs, and the most informed is due to Maya Bar-Hillel (2024). Amos Tversky died at the age of 59 in 1996. He said that Kahneman was the greatest living psychologist. Kahneman said that Tversky was the smartest and funniest person that he ever met. And their collaboration made the most profound contribution since WWII to understanding human nature—just how profound may be difficult for you to imagine unless you lived in the intellectual world before the impact of their joint research. This world took for granted an all-embracing rationality. We could be silly, neurotic, or insane, but otherwise we made our way through life, opting for choices in broad accordance with their expected utility, judging the likelihoods of outcomes in ways consistent with Bayes theorem and the probability calculus, and reasoning about events without systematic errors in logic. Economics was feasible. Amen. We all make mistakes of course, but nothing serious enough to jeopardise our fundamental rationality. For instance, if we draw one poker chip at a time from a book bag in which they are in one colour or another in a ratio of, say, 70 to 30, we make a Bayesian updating of our estimate of the probability of the majority colour. We are just a little too conservative (e.g., Phillips & Edwards, 1966). If we buy a new car, our choice is close to captured in the axioms of expected utility. Features common to all the relevant cars have no bearing on our preference. If we prefer one car to a second, and the second to a third, then we prefer the first car to the third. And if one car has the best mileage, or another superior feature, and is no worse than any of the others in some respect, it is the one we buy (von Neumann & Morgenstern, 1947). If we make a mistake in reasoning, then it occurs in our understanding of the premises or in describing our conclusion, but never in our logic (e.g., Cohen, 1981; Henle, 1962).

You tended to absorb these beliefs at your supervisor's knee. They were unquestionable and unquestioned. And they persisted long past their own utility. Here's a touchstone. In the early 1990s, I went to see a colleague in his office at Princeton. He wasn't there, but someone else was—a VIP, the Provost of the University. He asked where the Professor was. I didn't know. Then he asked: do you know the work of Kahneman and Tversky? Of course, I said. Tell me about it. I outlined the famous "Linda" study of the conjunction fallacy (to be described later). He said, not in astonishment, but in a cold condemnation suggesting faked data: "I do *not* believe it". Two years later, he became the President of a major American university. As you may have guessed, he was an economist, and an evident believer in "rational man" alias Homo Economicus, who is unchanging in preferences, selfish, self-interested, calculating, and unerring. His clones still survive in some regions. But there has been a climate change of opinion for which Danny and Amos were the chief agents. It was a revolution that fostered the new discipline of behavioural economics. And it has influenced medicine, the law, political policy, and business. Rationality is no longer taken for granted.

This article offers a tentative explanation of how their joint research had such an enormous impact. It begins with their early researches and how each of them thought about psychology. It outlines the principal discoveries of their collaboration with its post-script of dual systems. It concludes with why the collaboration created their best work—a way of thinking about psychology. It says little about the studies of others or about their individual researches after their collaboration petered out.

The early thinking of Kahneman and of Tversky

When you begin your graduate career, you tend to think that you should think as your adviser does. With experience, however, you realise that psychologists differ in how they think. Some are devastating critics, able to pull to pieces almost any experiment or theory (Jonckheere, 1981). Some devise extraordinarily original theories and test them with extraordinarily original experiments: one such individual published a paper with no citations to any psychological studies—none were pertinent (Whitfield, 1951). Some like to read the literature on a topic before beginning to investigate it; others follow Francis Crick's maxim that "reading rots the brain". Some like to talk to their colleagues about their current project; some would sooner discuss yachting in the Baltic. Some focus their research on a central theme; some leap from topic to topic. As to beliefs about experimental procedure, design, and statistical analysis, the last time a consensus existed was in 1879. Most of us, of course, are close to the mean on these variables, and only those who are exceptional in every sense stand out. Psychologists, it seems, are not unique. C. P. Snow's novel, *The Search*, written about science by a former scientist, illustrates an analogous diversity among crystallographers (Snow, 1935).

Danny and Amos differed in how they thought about psychology.

Danny was interested in many aspects of psychology, and he was astute about what research to do. His great gift was intuition about human thinking. Perhaps its origins were in his childhood in a Jewish family dominated by people and talk, and talk about people (Kahneman, 2007). Nature scarcely existed: he never learned to identify flowers or to identify with animals. He was neither agile nor athletic. He kept his cool, and survived an encounter with an SS officer when he was walking home after curfew in Nazi-occupied Paris. His first notebook at an age of around 11 had a prescient title: "What I Write of What I Think". His concern for whether God existed and for what was right and wrong soon gave way to an interest in what made people believe in God and in bizarre systems of morality. He majored in psychology in two years at the Hebrew University of Jerusalem with a minor in mathematics, at which, as he admitted, he was mediocre.

Danny served as a psychologist in the Israeli Defense Forces, and helped in the assessment of budding officers using a method pioneered in WWII by the British War Office Selection Boards (Kahneman, 2011, Ch. 20). A group of candidates is presented with a collective challenge, such as to build a bridge over a stream. The observers are on the lookout for signs of leadership and "officer material". The Israeli team reported plenty of incidents redolent of character and potential. But the feedback from the officer training school showed that their assessments were … useless. Yet they were unfazed and continued to do their job. Danny's response was to identify an illusion of "validity": people persist in thinking of themselves as experts despite incontrovertible evidence to the contrary. He had also identified a domain worth investigating: predictions, and the gulf between impressions of accuracy and the reality. Years later, Richard Thaler and he visited a large investment firm. They discovered that its managers suffered from the illusion of validity about their assessments of their employees' advice on what stocks to buy. The managers were also unfazed when they saw the evidence that they were assessing luck, not ability. "We don't know ourselves," they might have said—to echo the title of Fintan O'Toole's (2021) personal history of a similar disjunction in Irish history.

Danny's doctoral thesis was about the semantic differential. In retrospect, he wrote, it enabled him to study correlations. His first teaching job—in statistics—led him to what became a characteristic way of thought. He made a list of intuitive judgments, noting which were valid and which were invalid. One was our predisposition to invent causal explanations. After he gave a talk at a flying-school on the virtues of positive reinforcement, one of the instructors took exception. When *he* praised cadets' outstanding skill, they did worse next time, and when *he* told them off for abysmal performance, they did better next time. Danny had an epiphany, which he always claimed was the most satisfying moment in his career (Kahneman, 2011, p. 175). The instructor's ability to dream up a causal link between admonition and outcome had supplanted statistical necessity. Like many variables in life, exceptional flying whether good or bad is rare and almost certain to be followed next by a regression towards the mean. Danny and Amos later replicated the flying instructor's judgement: psychology graduate students in an experiment offered his sort of account of the effects of "feedback", and overlooked its sufficient statistical explanation (Kahneman & Tversky, 1973). To provoke discussion, they both recommended the question: Why do highly intelligent women tend to marry less intelligent men? The discussants will offer ingenious answers—women like to feel they can out-think their spouses—rather than cite regression.

One other result had a long term influence on Danny's thinking. In Walter Mischel's well-known "marshmallow" test, a child's choice between an immediate treat and a better but delayed one had various correlates, from the child's social competence to educational achievement (Mischel, 1961). The idea that a single measure—the answer to one question—could illuminate several psychological characteristics was an outcome to aim for.

Danny's first important discovery was in collaboration with Jackson Beatty, and depended on pupil dilation. When participants have to add 3 to each of a series of numbers, the mental effort causes their pupils to dilate. In contrast, when they have only to chat, their pupils maintain their normal contraction. This research was the basis of his first book in which he defended a standard "box-and-arrow" theory of what determines the allocation of the limited resources of attention (see Kahneman, 1973, Figure 1.2). Yet at heart Danny's interest was in human thinking and its social consequences, and at Princeton, which was his final academic home, he aligned himself with the social psychologists, not the cognitive faculty.

Amos was a different sort of thinker. He had a remarkable quick wit and intelligence, and an exceptional inferential ability. He was very funny, full of apposite jokes, and terrific to talk to—one of those individuals who brings out the best in you. He was the first person I ever heard say: "For those who like that sort of thing, that's the sort of thing they like," a belittlement that he may have borrowed from Max Beerbohm. He was athletic—a runner—and he served as a paratrooper in the IDF. And with tremendous bravery, he risked his life to save a soldier under his command. He was an autodidact as a mathematician, and his Ph.D. adviser was Clyde Coombs, one of the principal figures in the development of mathematical psychology. While Amos was still a young psychologist, he co-authored an introduction to the topic (Coombs et al., 1970), and he made major contributions to it throughout his career, working on the theory of measurement and on multi-dimensional scaling. He developed a mathematical theory of choice with help from Maya Bar-Hillel (Tversky, 1972). He demonstrated that individuals can have intransitive preferences, as in this example from everyday life (Tversky, 1969). You decide to buy a car, and tell the salesman that the simplest model will be fine. He points out that power steering is a bargain, and so you accept the upgrade. His persuasiveness convinces you to add a series of cheap but desirable features. But, when you see the total cost, you prefer your original choice of the simple model. He also formulated a very influential mathematical theory of similarity (Tversky, 1977). Unlike earlier accounts, his axioms are based, not on the distance between two entities, but on their set-theoretic features, and the degree of similarity increases with those features they share, and decreases with those features unique to each of them. Different weightings on these three sets of features explain why you tend to judge, say, that North Korea is similar to China to a greater degree than China is similar to North Korea. The axioms are formal, and so to test them experimenters must find their actual interpretations in the relevant entities.

Amos was a virtuoso in capturing psychological assumptions in axioms for mathematical models, and in teasing out their consequences. A familiar distinction in the theory of computability is between a function and an algorithm for computing it. A function is an abstract mapping from a set of inputs to a set of outputs as in the degrees of similarity between pairs of countries. It may be expressed in axioms. An algorithm is a procedure that computes the values of the function, but any computable function has an infinite number of different algorithms. Not every function has an algorithm; and not every algorithm is tractable—it takes too much time and memory for any device or brain. Nonetheless, Marr (1977) emphasised that a cognitive theory should if possible be framed both as a function and as a computable algorithm. Its computability shows that it doesn't take too much for granted. Amos theorised at the level of the functions to be computed in carrying out a cognitive task. His focus became vivid to me. He asked me to read a draft of his on probability. It was impressive. What do you think the underlying mental processes is? I asked. That's an odd question, he said, no-one ever asked me it before. His theory was at the level of a function; my question was at the level of an algorithm. You'd better ask Danny, he said. Indeed, Danny did discuss mental processes in his papers, whereas outside collaborations Amos's papers never did—the legacy of mathematical psychology, perhaps. A corollary was Amos's lasting aim to be precise and to be right—just as mathematicians settle for nothing else but a correct proof.

The Kahneman-Tversky discoveries

My aim is to explain why Danny and Amos's joint researches had more impact than their individual studies. But a review of their discoveries will help to pin down their collaboration. It began in 1969, and in just over a decade it led to two extraordinary bodies of work. It started with a serious paper with a joke title: Belief in the law of small numbers (Tversky & Kahneman, 1971). The law of *large* numbers is a major statistical theorem that proves that the larger the size of a random sample the closer its statistics are to those of the population from which it was drawn. But the law is not easy to use, because you have to realise its relevance to a problem, and to know how to calculate the required size of a sample. (These days various websites will do the calculation for you.) Danny himself had erred, failing to test an adequate number of participants to replicate a result (Kahneman, 2011, pp, 165, 168). He drafted a questionnaire based in part on Jacob Cohen's (e.g., 1969) analysis of statistical power, to examine errors in psychologists' thinking. A typical question was:

> An experiment with 20 participants yields a significant result in favour of your prediction (z = 2.23, $p < .05$, two-tailed). What is the probability that a replication with a group of 10 participants yields a significant result on a one-tailed test?

Amos gave the questionnaire to attendees at a meeting of the Mathematical Psychology Group of the APA, and most of them erred on most of the questions. The experts were divided about the question above, and the majority thought the probability was around 0.85, which is a vast overestimate. A more reasonable one is around 0.48. In those days, it was rare if ever to publish the mistakes of experts. The paper even admonishes readers: "Explicit computation of power, relative to some reasonable hypothesis, for instance, Cohen's … small, large, and medium effects, should surely be carried out before any study is done" (ibid, p. 110). This advice has become a mandatory requirement for many journals. Danny and Amos then discovered that non-experts also tended to ignore sample size in making everyday judgments (Kahneman & Tversky, 1973).

Their next body of research was on judgments, and it led to their first great paper, a synopsis of results (Tversky & Kahneman, 1974). Here are three examples:

1. In a sequence of random tosses of a coin, which is more likely to occur in a sequence of heads and tails: H-T-H-T-T-H or H-H-H-T-T?

Most people judge that the first sequence is more likely. It looks more like a random sequence to them, but each outcome in the sequence is independent of the others, and so the two sequences are equiprobable. The *representativeness* heuristic has biased the participants' judgments.

2. You select a word at random from an English text. Is it more likely that the word starts with *r* or that *r* is its third letter?

Most people judge that a word starting with r is the more likely of the two. They are wrong. It is easier for them to find words in the mental lexicon by their first letter. The *availability* heuristic has biased their judgments.

3. One group of participants had 5 seconds to estimate the product: $2\times3 \times 4\times5 \times 6\times7 \times 8$
   And another group of participants had 5 seconds to estimate the product:
   $8\times7 \times 6\times5 \times 4\times3 \times 2$.

The first group's estimates had a median of 512, and the second group's estimates had a median of 2,250. People tend to calculate a couple of the initial steps, which yield 24 for the first calculation and 336 for the second calculation. These values act as anchors for an adjustment based on an estimate for the result from the remaining steps. But few people know that a factorial's value increases at a rate even faster than exponential: the correct result is 40,320. The *anchoring and adjustment* heuristic has biased their judgments.

Danny and Amos pointed out that even experts rely on heuristics, which are useful and essential. Indeed, Fisher, the inventor of analysis of variance, had a heuristic view about experimentation: "the object of our study is not the individual result, but the population of possibilities of which we do our best to make our experiments representative" (Fisher, 1934, p. 1). The study that made "Linda" famous described her as outspoken in her student days, majoring in philosophy, concerned with issues of discrimination and social justice, and so on. The participants rank ordered the probabilities of various descriptions. And they tended to rank a representative conjunction:

Linda is a bank teller and is active in the feminist movement

as more probable than its conjunct: Linda is a bank teller, but less probable than its other conjunct: Linda is active in the feminist movement (Tversky & Kahneman, 1983). To judge that a conjunction can be more probable than one of its conjuncts is, of course, a blatant violation of the probability calculus.

A recent study resonates with these findings. The participants had to estimate the probabilities of events whose contents were unlikely to elicit particular heuristics, e.g.: the probabilities that:

A) intelligent alien life will be discovered,
B) more resources will be put into contacting them,
and their conjunction: (A & B).

Intuitive estimates of the probability of the conjunctions were rough averages of the probabilities of their two events—a generic heuristic. Other participants tended to compute the product of the two probabilities. Together, the three estimates fix the probabilities of each of the four possible outcomes: A & B, A & not-B, not-A & B, not-A & not-B. But the heuristic of a rough average requires at least one of the four to have a negative probability, such as minus 0.1, to satisfy the calculus's requirement that the four sum to 1.0. A conjunction fallacy is a special case of such "subadditive" distributions (Khemlani et al., 2015).

Naive individuals do not know how to estimate the probabilities of conjunctions. So as a probabilist remarked, anyone who knew how to use the calculus in the Roman era would soon have won the whole of Gaul (Hacking, 1975). That's why the calculus needed to be invented. With the benefit of hindsight bias—another of Danny's early ideas—Amos's and his joint discoveries about judgments concern human intuitions about probabilities, values, and preferences. These intuitions are sensible, but fallible.

Their next body of research led to their most revolutionary paper. At Amos's suggestion, they investigated decision making. Danny began by reading Amos's co-authored book on mathematical psychology (Coombs et al., 1970). He discovered that utility was graphed as a function of wealth whereas gambles were defined in terms of gains and losses. The difference puzzled him, because he realised that what affects you in daily life is, not your wealth itself, but changes to it. Everyone likes a gain and hates a loss, a difference which could be innate, but a loss is much worse. Ask yourself, for instance, how much you need to win to accept a bet on a coin toss if a loss costs $100: for most people it is about $200 (Kahneman, 2011, p. 285). Amos and Danny made psychological value itself into a function of changes to the status quo—the reference point—in gains or losses. It was a crux, because it led them to the heart of human decisions. Consider this case:

Which would you choose:

> a sure gain of $900,
> a 90% chance of a gain of $1000, and a 10% chance of nothing?

The two options have the same expected utility, but like Danny and Amos you should prefer the sure thing. Amos had the excellent idea to flip gambles from gains to losses.

Which would you chose:

> A sure loss of $900,
> A 90% chance of a loss of $1000, and a 10% chance of a loss of nothing?

These two options have the same expected utility, but like Danny and Amos you should prefer to risk a greater loss for a small chance of losing nothing. As their experiments corroborated, people do not like to risk a sure thing for the chance of a gain—they are "risk averse" for gains, but they do take a risk of losing a little more for the chance of losing nothing—they are "risk seeking" for losses. Danny and Amos were not the first to make such observations, but the theory of expected utility did not explain them. In contrast, their "prospect" theory was a function that made sense of the results. It maps increasing sizes in gains onto increasing sizes in positive values, but the increase in value for a gain from $10 to $100 dollars is greater than the increase in value for a gain from $910 to $1000. The gain is the same monetary amount in the two cases, but the increment in value declines, just as most psychophysical curves do, e.g., the more intense the light, the bigger the change in intensity that is necessary for you to detect the difference. The prospect function maps an increase in losses onto an increasing negative value, and the increase also slows down in a psychophysical way. But the curve for losses is steeper than the curve for gains. This key difference explains risk aversion for gains, and risk seeking for losses. It is the fulcrum of the theory that contravenes the rational symmetry of von Neumann and Morgenstern. Another major component, however, is the weights that multiply values in order to govern decisions. The paper begins with a series of results that are plausible but obvious violations of standard decision theory, and then makes sense of these results with the prospect theory (Kahneman & Tversky, 1979).

One later consequence of the theory concerns whether a choice is *framed* in terms of gains or in terms of losses (Tversky & Kahneman, 1981). For example, ask yourself which of two programs is preferable during an epidemic expected to kill 600 people:

> One program saves 200 people;
> The other has a 1/3 probability of saving 600 people and 2/3 probability that no people will be saved.

Three-quarters of the participants in an experiment preferred the first program: they were risk averse for a gain in lives. An alternative framing flips gains to losses:

> One program loses 400 people who will die;
> The other has a 1/3 probability that nobody will die, and 2/3 probability that 600 people will die.

Over three-quarters of the participants preferred the second program: they took the risk of a greater loss of life for the chance that no-one died. Yet the two pairs of programs are equivalent. Their framing alone, as prospect theory predicts, switches the participants' preferences. Framing is not unique to values. It also occurs in inferences, depending on what the premises make salient to intuitions. Another later modification generalised prospect theory to deal with any number of outcomes, and to cope with both those that were risky, i.e., of a known probability, and those that were uncertain, i.e., of an unknown probability (Tversky & Kahneman, 1992).

The first paper devoted to prospect theory became the most cited paper for all concerned—Danny's, Amos's, and *Econometrica's*. Two pre-WWII thinkers would have welcomed it too: the polymath Frank Ramsey and his mentor in economics John Maynard Keynes, because they recognised irrational factors in judgement and reasoning. The award of the Nobel prize in economics to Danny in 2002, which Amos would have shared had he lived, shows that their views had become legitimate among economists.

Dual systems: Danny's post-script to the collaboration

The main reason that their collaboration came to an end seems to have been geography (cf. Lewis, 2016, Ch. 12). Amos continued to teach at

Stanford for the rest of his life, but Danny moved to Vancouver in 1978 and then onwards elsewhere. So they could no longer get together for regular research discussions. They tried more than once to revive the partnership right up to the end of Amos's life. They wrote an occasional joint paper and each of them continued with independent research. Danny, for example, collaborated with Richard Thaler. And his later research with Shane Frederick bolstered his view that there were two sorts of thinking: fast and slow (e.g., Kahneman & Frederick, 2005). Amos and he used to solicit each other's intuitive judgments, and in the early 1980s, Danny started to use "intuitive" as a theoretical term, and to contrast it with "extensional" thinking. His first reference to a dual system of thinking seems to have been in a commentary in which he wrote: "Tversky and I always thought of the heuristics and biases approach as a two-process theory" (Kahneman, 2000, p. 682). He embraced the idea in his Nobel lecture (Kahneman, 2002). Later, in his brilliant book (Kahneman, 2011)—now over 400 weeks on the *New York Times*'s best-seller list—he made sense of everything from pupil dilation to prospect theory in terms of two systems. System 1 handles fast intuitive thinking, and system 2 handles slow and deliberative thinking. Many psychologists had advocated such a dual system, and Peter Wason was the first to introduce it into the psychology of reasoning (see Manktelow, 2020). Its historical progenitors include William James, Blaise Pascal, and—inevitably—Aristotle. The essentials of the modern account, however, are due to a precursor no-one has hitherto acknowledged. And you deserve a prize if you already knew. He distinguished between two faculties of thought, *intuition* and *ingenuity*, and he wrote:

> The activity of the intuition consists in making spontaneous judgments which are not the results of conscious trains of reasoning. These judgments are often but by no means invariably correct…
>
> The exercise of ingenuity in mathematics consists in aiding the intuition through suitable arrangements of propositions, and perhaps geometrical figures or drawings.

The author of this remarkable anticipation is Alan Turing (1939, sec. 11), the chief founder of the theory of computability. Not all psychologists accept the distinction, but it is hard to deny when individuals create two different outputs from the same input depending on how hard they think, and it has at least one computer simulation so it does not take too much for granted.

How Kahneman-Tversky thought

Not every discipline stimulates collaboration, and not every individual does, either. Yet it can amplify a way of thinking in like-minded individuals, conjoining two sets of expertise. A rare collaboration, however, creates a way of thought that differs from the thinking of either of its members.

Their interaction yields an interaction. Striking examples are Wilbur and Orville Wright, who out-thought their rivals (Johnson-Laird, 2005), and G. H. Hardy and Srinivasa Ramanujan, who were a modern number theorist and an auto-didact more skilled in some domains than any other mathematician, but with only a "shadowy" idea of proof (Hardy, 1927, p. xxx). Danny and Amos's collaboration was also exceptional. Datum one: an eminent reviewer, who rejected one of their early papers, wrote that they should cease their collaboration, because "they bring out the worst in each other". Datum two: "I had enjoyed collaborative work before, but this was something different," Kahneman (2011, p. 168). Datum three: the magic of their collaboration disappeared when they admitted a third person into it: Danny confessed that they became competitive with one another. We know much about how they did research together: Danny has told us, but Amos seems not to have written anything on the topic, alas. And there is much that we do not know, such as whether they ever discussed how children developed the ability to make judgments and decisions.

Danny and Amos had different personalities and different habits, but they became great friends. When they collaborated, they were flexible enough to accommodate all but one of their major differences (more on this, later). Danny was intuitive. He would settle for a quick solution, perhaps change his mind, or worry that it might be wrong—he was a natural pessimist, whereas Amos was an extreme perfectionist—his motto: "let's get this right," and he was justifiably optimistic about his ability to do so. Danny was an insomniac—he enjoyed flying because only then could he sleep—and he was an early bird. Amos was a nighthawk—he worked while others slept. They soon settled into a habitual routine. They lunched together and then worked together for the afternoon. They brought different characters and different aptitudes to the table, but above all sheer enjoyment in each other's company. Work was talk. Like a duet of enthusiastic Quakers, they discussed everything about their research until they reached agreement. They clarified one another's formulations, completed one another's sentences, traded jokes with one another. They thought out loud, and had a tacit agreement not to debunk each other's ideas. Danny thought the biggest benefit was that they could each draw out the implications of the other's half-baked ideas. Part of their mutual enjoyment was perhaps that it repressed a nascent rivalry between them, which was to surface later. There was no division of intellectual labour, and so the ownership of all the ideas was joint—not that they could have separated most of them into his and *his*. They tossed a coin for who was the senior author of their first paper, and thereafter alternated the order of authorship during their heyday.

Their prolonged interactions seemed to create a composite psychologist of superior ability, to whom I will refer in the singular as Kahneman-Tversky. No-one knows how he decided what problems to work on. We can guess that Danny was keen to examine how people made judgments. We know that Amos suggested that they study how people make decisions, perhaps it was to revisit unfinished business in his earlier account. Kahneman-Tversky made the decision. He combined profound intuition about human thinking, subtlety in developing experiments, skilled theorising that could be axiomatic, and a dialectical process of writing.

A long tradition in psychology (and physiology) is for investigators to experiment on themselves, e.g., Galton on association, Ebbinghaus on long-term memory, and Craik on the creation and consequences of an artificial blind spot (do not try this at home). But self-experimentation in cognition is difficult when you know what your theory predicts. A quasi split-brain is the answer when the results are to guide the construction of theory and to supply the contents of experiments. So Kahneman-Tversky, harking back to pioneer experimental psychologists, spent hours alternating between one half giving the other half a judgement to make and vice versa, or a decision between gambles to make. So the ideal experiment was a series of questions, more akin to a demonstration than to the manipulation of multiple independent variables. Those yielding unanimity were a target for theory and for test. When one half knew less than the other half about something, it could be useful as a fresh eye on the topic—as when Danny could not understand why overall wealth was treated as a reference point for utility. Kahneman-Tversky had a taste for minimal theories that yield predictions that are easy to understand, and that finesse the need to explain anything else. So a theory needed to be only at the level of a function. It was enough to predict experimental results, and obviated an algorithmic account of mental processes. Likewise, a progress report calls for nothing else than a defence of what it describes—no need to define "rationality", just take it for granted.

The most striking difference from before the collaboration was in the writing of papers. Most of us do research, and then write it up. For Kahneman-Tversky, the writing was part of the research. He dramatises an experiment so that sensible readers cannot help but notice the pull of their own intuitions against the canons of the probability calculus or expected utility theory. He devises decisions to convince himself, his experimental participants, and in the end his readers that a mental representation is the ultimate lever of choice.

What was extraordinary was the time Kahneman-Tversky devoted to writing—to repeat, during the heyday of just over ten years, the output was eight published papers. Neither half of the composite was a native speaker of English, and in their joint composition of a paper, they split some duties. Danny was the arbiter of English prose, and he did the typing. Amos couldn't type, but he was in charge of the direction the paper took and its organisation. Progress was slow but real. They composed each sentence together, and laboured over the choice of every word. They spoke draft sentences aloud of course—a boon for getting the prose to flow. But it wasn't just a question of "the intolerable struggle with words and meanings". They had the harder struggle of ideas and thoughts. In a good afternoon session, they wrote only a handful of sentences. There was no rush for the composite psychologist: Amos's perfectionism and confidence cancelled out Danny's hustle and worry. The 1974 paper took a year to write.

Kahneman-Tversky was not just polishing the prose, though the writing appeals to more than just experts—it minimises jargon, avoids mnemonics, and is lucid. The formulation of prospect theory was close to complete in early 1975, but it took three years of writing and rewriting to get the details right, and prospect theory itself cycled through many versions, at least 20. The paper had to be suitable for submission to the most prestigious journal for economics and decision making, *Econometrica*. So it is written in a style appropriate for a readership of economists, and includes an appendix outlining how prospect theory can be axiomatized. Part of its effectiveness is that it presents a series of problems, and so readers get an intuitive grasp of their compelling nature, before they have to cope with the new theory. Reviewers and critics have many points of view, and so most authors think about what sceptics might say against their arguments. Kahneman-Tversky had an ingenious process, and played the role of an ambitious graduate student looking for flaws, and edited the paper to make this critic's task as thankless as possible. The ploy led to good ideas, e.g., the theory allows decision-makers to collect similar prospects together and to sum their probabilities (Kahneman, 2007, p. 179). One of the last things to be done after three years of writing was to invent the theory's name. Kahneman-Tversky reasoned that if the theory ever became well-known then it needed a distinctive name. Consult a thesaurus, and you will discover that no other name seems so apposite as *prospect* theory.

The one difference between Danny and Amos that Kahneman-Tversky could not accommodate in full was their different reactions to criticism. They wrote a handful of replies, but not in the same frame of mind. Amos was a stringent critic, and his reaction to criticism was "bring it on" (expletive deleted). Danny hated controversy—so much so that he gave up publishing journal articles, because, as told me, he couldn't stand reviewers' hostility. If a co-author was prepared to shepherd a joint paper through the press on his behalf, fine; otherwise, he preferred to write books. Of course criticism could provoke him, and he'd get angry, which in turn led him to feel ashamed of himself. This distaste for overt hostility was part of the reason he championed the idea of adversarial collaborations. Instead of duking it out in opposing belligerent papers, the antagonists instead carry out joint research, perhaps under the supervision of a neutral third party. Their aim is to write a paper together—to seek compromise, or otherwise clarity. Danny took part in at least three. He was also prone to pessimism about the fate of his research. One day in the 1990s after he had come back from an encounter with one of his least favourite critics, he summed up his view of psychology to me: It's all a game, and whoever has the last move wins. I demurred: No, no, no; in the long run, the truth will out. But he wouldn't agree. After he won the Nobel prize, I used to tease him a little, and say about a point of contention, Isn't it all a game? He would look a little sheepish, but at the same time quietly pleased. Over a lunch to celebrate his retirement, he said that his work on well-being was coming to an end, and then out of nowhere he remarked: Neuro-economics will bury Amos's and my work. Not if you have the last word, I said.

Conclusion

In the end, the reason for Danny's and Amos's outstanding research together and its enormous impact may be their joint creation of a superior composite scientist. He surpassed their individual skills. And he put much more original thought into theory, experiment, and writing, than they alone could have done. Contrary to settled opinion, they established that judgments depend on intuitive heuristics, and that decisions follow from intuitive reflections on what matters in reality, the values of gains and losses. Human intuition at last supplanted universal rationality.

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