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Gigerenzer the Decided

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a tale of difficult distinctions

The journey ventures into a land of rationality that is different from the familiar one we know from many stories in cognitive science and economics –tales in which humans live in a world with unlimited time and knowledge, where the sun of enlightenment shines down in beams of logic and probability. The new land of rationality we set out to explore is, in contrast, shrouded in a mist of dim uncertainty. People in this world have only limited time, knowledge, and computational capacities with which to make inferences about what happens in the enigmatic places in their world.

– Gigerenzer , Todd and the ABC research group (1999)

I argue that the conceptual distinction between single-event probabilities and frequencies is of direct relevance for psychology, and vice versa.

In short, the proper functioning of a mental algorithm depends on the way in which information is represented. So, to analyze probabilistic reasoning, we must attend to the difference between, at least, the frequency and the single-event representation of probability. If evolution has favored one of these forms of representations, then it would be frequencies, which prelinguistic organisms could observe and act on. Attending to this distinction suffices to make several apparently stable cognitive illusions disappear.

– Gigerenzer (1994)

Introduction

Humans are intriguing beings. They are also very complex beings. The result of millions of years of evolution, they have developed sophisticated means to deal with their environment. So where should we begin to investigate the working of these complex beings? Fortunately, the complexity partly lies therein that human beings have developed amazingly simple rules of thumb as a basis for their behavior. As a start one could study these fast and frugal heuristics.

This, in a nutshell, is the view of the psychologist Gigerenzer. It is, of course, not an idea that came from nowhere, but is the result of years of research. Specifically, it is to a large degree shaped by his disagreement with another and closely related psychological theory of the working of the human mind: the Heuristics and Biases theory of Kahneman and Tversky. This disagreement itself was to a considerable extent shaped by Gigerenzer's earlier work on the history of probability theory and statistics, which sprang from his interests in psychophysical models of measurement with which he started his career.

As not everything can be treated in the detail it deserves, the focus in this paper will be on Gigerenzer's criticism of Tversky and Kahneman and his competing research program as advanced since the late 1990s. After an introductory overview of the work of Gigerenzer in the first section, some general themes of the background of Gigerenzer's research are introduced in the second section. The third section provides an overview of Gigerenzer's involvement in the collaborative research on the history of probability theory and statistics as it forms the basis of his criticism of Kahneman and Tversky, treated in the fourth, and of his own research program, treated in the fifth section. After concluding remarks have ended the paper, Tversky and Kahneman's answer to Gigerenzer's allegations are summarized in an appendix.

1. Introductory overview of the work of Gigerenzer[1]

Born in 1947, Gigerenzer receives a degree in psychology from the University of Munich in 1974. Although 27 may seem relatively old to obtain a university degree, it is the average by German standards. Three years later in 1977 Gigerenzer receives his PhD and five years after this, in 1982, his Habilitation, both from the University of Munich. In different positions Gigerenzer is affiliated with the University of Munich from 1974 up to 1984. After this he holds from 1984 until 1995, in chronological order, professorships at the universities of Konstanz (Germany), Salzburg (Austria), and Chicago (USA). From 1995 to 1997 Gigerenzer is director of the Max Planck Institute for Psychological Research in Munich. Since 1997 he has been the director of the Max Planck institute for Human Development in Berlin.

The publications of Gigerenzer are characterized by a gradual shift from exclusively German in the first four years to almost exclusively English in the first years of the twenty-first century. As a broad approximation Gigerenzer's publications are furthermore characterized by an emphasis on mathematical measurement models in social psychology and psychophysics in the first five years, to an emphasis on (the history of) probability and statistics during the 1980s, to an emphasis on (bounded) rationality during the last fifteen years. Gigerenzer is married to Lorraine Daston, historian and director of the Max Planck Institute in the history of science[2], with whom he professionally collaborated at the time of his work on the history of statistics during the 1980s.

2. Other introductory themes

Gigerenzer is, at least in the early years of his career, both a psychophysician and a cognitive psychologist, a combination he shares with for example Kahneman. The formalist or mathematical character of the measurement models of psychophysics demands an explicit

[1] This introduction is drawn on the basis of Gigerenzer's CV as it is available from the website of his research group, see http://www.mpib-berlin.mpg.de/en/forschung/abc/index.htm

[2] Daston labels herself an historian of ideas, specializing in the mathematics of the eighteenth century (Daston (2001), p.9).

opinion of the psychologist about the usefulness of mathematics in psychological research. Before setting out the content of Gigerenzer's work it seems hence relevant to first introduce both psychophysics and cognitive psychology, and the view on mathematics especially the former implies.

What is Psychophysics?

Psychophysics, or physiological psychology as it is sometimes referred to[3], is the scientific field that investigates the physiology of the senses. A total of seven senses is distinguished, but the larger part of psychophysical research concentrates nevertheless on the visual sense[4]. Psychophysics employs an experimental methodology in which the relationship between physical stimuli and their responses in the form of sensational perception is investigated. Most of the time the sensation and the perception are equated, i.e. no perception means no sensation[5]. Sometimes, however, a distinction is drawn. In this case a sensation is more narrowly defined as the first stage in a chain of a (bio) chemical and neurological process that may lead to a perception[6].

Psychophysics originates in the second half of the nineteenth century and counts among its main early figures such authors as Helmholtz, Fechner and Wundt[7]. As a result of his attempt to construct scales for the perceived sensations and the resulting proposition of the just noticeable difference (jnd), especially Fechner stands out as a key figure. His psychological scales are taken to derive naturally from our everyday language and experience. Hot and cold, for example, are psychological scalings of temperature stimuli. This basic framework of stimulus-response and the notion of psychological scales have survived unchanged to this day. If we consider the psychophysical work of Gigerenzer, but also of Kahneman and Tversky for instance, it is evident that the main question is still how to measure the

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[3] See for example Boring (1929). Physiological psychology in this seminal history on experimental psychology refers predominantly to the pre-Fechnerian first half of the nineteenth century.

[4] See the Wikipedia on psychophysics: http://en.wikipedia.org/wiki/Psychophysics

[5] http://www.cogsci.princeton.edu/cgi-bin/webwn?stage=1&word=sensation

[6] See the Wikipedia on sensation: http://en.wikipedia.org/wiki/Sensation

[7] Murray (1993), Boring (1929), Kline (2000), Michel (1999), Danziger (1990), and many others.

sensational responses given the stimulus-response framework[8]. Gigerenzer himself describes psychophysics in slightly different terms as arising out of the divergence between our attempt to describe the world around us, and the question of how we perceive that world:

> "Psychophysik, so könnte man sagen, entsteht aus der Divergenz zwischen der physikalischen Beschreibung der Welt und der Beschreibung derselben mittels unserer Wahrnemung."[9]

The stimulus-response framework of psychophysics and its related discussion on measurement has been taken over by behaviorism and cognitive psychology in the twentieth century. As a result, it is difficult to distinguish on a methodological basis between the psychological research programs of psychophysics, (neo)-behaviorism, mathematical psychology, and cognitive psychology. This is not to say there has not been any disagreement about the right method for the stimulus-response measurement. What it does imply is that it is problematic to frame these different views on the measurement of stimuli and responses in terms of the well-known branches of psychology. Put differently, it is often impossible to put on the basis of the methodology employed a distinctive label of a psychological sub-field on publications by psychologists as Kahneman, Tversky and Gigerenzer. In an historical account that focuses on the methodologies employed these terms therefore necessarily remain somewhat vague[10].

What is cognitive science?

It is notoriously difficult to pin down exactly what entails cognitive science. A sensible first step could be to look up the definition of cognition as provided by the Oxford Dictionary. Cognition is:

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[8] One might object that in recent years the stimulus-response framework is increasingly replaced with a game theoretic approach. I would fully agree.

[9] Bredenkamp and Gigerenzer (1984)

[10] This is a minor point here, but may surface more clearly would one at a later stage want to pinpoint more precisely the difference between the two camps as described in this paper in methodological terms.

> "The action or faculty of knowing taken in its widest sense, including sensation, perception, conception, etc., as distinguished from feeling and volition; also, more specifically, the action of cognising an object in perception proper."[11]

Taking the definition literally hence implies that cognitive science must be the scientific investigation of knowing, taken in its widest sense but distinguished from feeling and volition. And indeed, as a first approximation this seems a reasonable definition.

In a next step this definition can be compared with definitions of cognitive science as provided by authoritative sources. Two of the most used of these sources are Wikipdia, the free and biggest online encyclopedia, and the Stanford Encyclopedia of Philosophy. This yields the following:

> "Cognitive science is usually defined as the scientific study either of mind or of intelligence."[12]

and,

> "Cognitive science is the interdisciplinary study of mind and intelligence, embracing philosophy, artificial intelligence, neuroscience, linguistics, and anthropology."[13]

There is thus a subtle difference between the use of the concept of cognition in everyday English, and cognition as it is understood by scientists in the collocation cognitive science. The scientific use points in the direction of a science that investigates intelligent beings in their totality, the everyday definition of cognition points in the direction of cognitive science investigating knowledge, or rational behavior, as opposed to non-rational behavior such as emotions. However, if we read a bit further into the description of cognitive science as provided by the Stanford Encyclopedia , we find that "[t]he central hypothesis of

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[11] http://dictionary.oed.com

[12] See the Wikipedia on cognitive science: http://en.wikipedia.org/wiki/Cogntive_Science

[13] Thagard (2004)

cognitive science is that *thinking* is best understood in terms of representational structures in the mind and computational procedures that operate on those structures."[14] (my emphasis), which suggests that emotions, feelings and so on, do not form part of cognitive science.

It must hence be that either cognitive science considers emotions, feelings and the like to be non-existing relics of the past, or that it considers these to be included in the study of the mind and of intelligence. At first sight this may seem to be a minor issue. The reason that I stress it is that I believe that it may very well be at the basis of much of contemporary debate in cognitive science and cognitive psychology, including the controversy between Gigerenzer, and Kahneman and Tversky. The basic question, then, is the following: if one investigates the working of the mind of intelligent beings, does that only include that part of behavior that can be deemed 'rational', or does it include also the behavior which is the result of emotions, passions, drifts, feelings, volition, and so on? We shall come back to this question below. Let us first further specify the cognitive sciences.

A sub branch of cognitive science is cognitive psychology. With respect to cognitive science generally it restricts itself to the study of (intelligent) behavior of human beings, as opposed to for example the investigation of intelligent behavior of chimpanzees, dolphins and robots. It furthermore restricts itself to the individual, thereby ignoring theories of super-human phenomena such as cultures. That said, the relation with other sciences such as artificial intelligence and economics is immediate. The interdisciplinary emphasis of cognitive science also is a characteristic of cognitive psychology.

As mentioned, the methodology employed by cognitive science and cognitive psychology is the psychophysical experiment of stimulus and response. Subjects in laboratories are given (physical) stimuli after which the corresponding response is measured by the experimenter or reported by the subject. In contemporary cognitive psychology these are often called judgments of the stimuli. The biggest part of the cognitive psychological literature hence consists of theories of how the stimulus given to the subjects relates to the responses measured or judgments made.

[14] Thagard (2004)

This brings us to the description of what cognitive science/psychology is not, which is perhaps the clearest exposition of what it entails. Cognitive psychology theorizes about the human mind in its totality. In its totality, it views the mind as a process that gives a certain kind of response when given a certain stimulus. What it does not investigate is how different parts of the brain relate to the behavior of the individual. The work of Damasio (c.f. 1994, 2003) is thus typically not part of cognitive science. What cognitive psychology neither does is investigate how more than one individual may act together in a group, or what the nature and functioning is of the institution in which we trade the goods we produce. Cognitive science, in other words, is neither sociology or economics, nor neuroscience.

That said, it should be added the different fields may be combined, which is precisely what we see happening at this moment. Cognitive psychologists may use neuroscientific knowledge as a basis, a constraint, a source of information for their cognitive psychological theories. Economists and sociologists may use descriptions of human beings from cognitive psychology for their theories about human interaction and markets.

Finally, some comments should be made about the metaphors employed by cognitive science and cognitive psychology. The most common metaphor in cognitive science is that of the human mind as a computer. For instance, man's cognitive capacities are often called computational capacities, and theories about the functioning of the human mind are often stated in the typical if/then language of computer's software. This does however not imply that the mind is equated with a computer[15], if only because there is no such thing as *the* computer. Another metaphor that has risen to some prominence in cognitive psychology is the metaphor of the mind as an intuitive statistician. Metaphors and their upgrading to full theories of mind in cognitive science and cognitive psychology are an important element in the work of Gigerenzer. Cognitive scientists in general, however, seem to be aware of the metaphors they use.

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[15] Although some cognitive scientists come pretty close. The most visible discussion on cognitive science's use of the computer as a metaphor of the mind is the debate following Searle's criticism on what he labelled 'strong AI' in his well-known 'Chinese room' article. See Searle (1980), Boden (1988), and others.

Gigerenzer on mathematics

During the first few years of his academic career an important part of Gigerenzer's publications deal with mathematical methods of measurement in psychophysics[16]. This research is in the same vein as, or is at least closely related to, the theoretical work on measurement by Tversky. In Gigerenzer (1977) for instance he mentions the paper by Tversky and Krantz (1969) on the experimental additivity of interdimensional characteristics as an example of the research he himself is also involved in.

The view that comes out of these early publications is one in which mathematics in psychology generally speaking is worth pursuing as it forces the scientist to be precise and consistent. Although the emphasis on mathematical models of measurement, and psychophysics generally, disappears in later years, it does not seem that Gigerenzer's ideas about mathematics have changed. That is, in Gigerenzer's continuous view psychological theories, classifications, measurement models and so forth, should be put in mathematical terms if possible[17]. As an example of his view of mathematics, consider the next quote from an early paper on taxonomy and taxonometry (taxonomy by means of mathematics).

> "mit mathematischen Methoden [können] die Kapazitätsschwäche unserer Sprache und damit unserer Urteilsschemata ausgeglichen und die Kommunikation über Verhaltensstrukturen zuverlässig verbessert werden."[18]

3. Gigerenzer and the history of probability and statistics

In 1975 Ian Hacking published his *The Emergence of Probability*. This book, which discusses the first few decades in which the mathematical interpretation of the concept of probability was taken at hand by such scholars as Pacal, Fermat and the Bernoulli's, soon became a classic.

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[16] See for example Gigerenzer (1977a, 199b, 1984,1986), Bredenkamp and Gigerenzer (1984) and Gigerenzer and Sarris (1982)

[17] Gigerenzer's recent attempt to get into contact with economics could perhaps partly be explained by the wish to become once again more formal in his theories.

[18] Gigerenzer (1977a), p.740

Not only did it became the main point of reference for those writing on the history of probability and statistics, it was also an important catalyst for further research.

In the years following the publication of this book, the historical investigation of probability theory and statistics was taken up by a number of researchers who over the years frequently collaborated in a range of publications, seminars, and conferences. As a very rough and imprecise approximation, the main contributors to this collaborative research can considered to be Lorenz Krüger, Michael Heidelberger, Lorraine Daston, Mary Morgan, David Murray, Ted Porter, Kurt Danziger, John Beatty, and Gerd Gigerenzer. The first in this series of publications following Hacking is *Probability and conceptual change in scientific thought* (1982), a book edited by Heidelberger and Krüger and comprised of different contributions by researchers who participated in a workshop in Bielefeld in July 1981. *The Empire of Chance* (1989), a collaboration of Gigerenzer, Swijtink, Porter, Daston, Beatty, and Krüger which puts together all expertise on the subject, acts as the pinnacle of the collaborative research, after which the different contributors went their separate ways.

Only a few years after finishing his PhD, Gigerenzer thus became involved in a broad collaborative interest in the history of probability theory and statistics. In the context of this collaboration some specific expertise of Gigerenzer in this research can be pointed out and some personal opinions or emphasis can be unraveled. It is nevertheless difficult to tell exactly where Gigerenzer starts and the collaborative research ends (and vice versa). Although Gigerenzer stands out with respect to the other contributors in that he remains throughout primarily a practicing experimental psychologist, it is problematic, if not right out impossible to tell whether a certain view or description of probability reflects a general shared view of probability, or a personal view of Gigerenzer on how to use probability in psychology. Furthermore, the literature on the history of probability and statistics generally is permeated with a strong sense of what constitutes good science and good psychology. Often it is not clear whether this is a reflection of writing on this particular subject, or of an attempt to use historical arguments to advance a specific view on how to do science/psychology. Put differently, given the fundamental character

of probability theory and statistics and the fact that it has penetrated each and every corner of our life (including our language), the idea that even an attempt at historical objectivity is doomed to fail should be kept in mind. Gigerenzer, in other words, *is* statistics and probability in psychology, and Gigerenzer *is* history of probability theory and statistics.

Despite all these shortcomings there is much to tell. The best way to proceed, then, seems to be to give an account of the general themes in the historical writings on probability and statistics, which then function at the same time as general themes in the work of Gigerenzer. Given a selection of the of historical accounts of probability theory and statistics[19], these themes are, to be sure, what I infer to be the main issues. In this account I will consider the history of probability and statistics generally known.

Probability, statistics and rational men

The rise of what became known as classical probability in the period between roughly 1650 and 1830, is a theme in the larger story of the Enlightenment[20]. The classical probabilists set themselves to the task of describing in mathematical terms both the beliefs of the newly conceived rational man and the frequencies of worldly events this rational enlightened man wanted to explain. From the first moment onwards the mathematical interpretation of probability therewith had a dual meaning.

The interpretation of a duality in the meaning of probability is due to Hacking (1975), and taken over by other authors writing on the subject[21]. Following Hacking it is often referred to as the Janus-faced

[19] i.e. Hacking (1975), Heidelberger , Krüger (1982), Heidelberger, Krüger and Rheinwald (1983) Daston (1982, 1986), Danziger (1990,1997), Krüger, Gigerenzer and Morgan (1987), Krüger, Daston and Hedelberger (1987), Gigerenzer and Murray (1987), Gigerenzer, Swijtink, Porter, Daston, Beatty and Krüger (1989), Porter (1986) Stigler (1986), Klein and Morgan (2001), Kurz-Milcke and Gigerenzer (2004).

[20] Here I do not elaborate on the relation between science, mathematics and the Enlightenment. See for careful expositions on these subjects for example Outram (1995), Daston (1986) and Hampson (1968).

[21] The first to make this dual interpretation, however, was Carnap (1944). Some, like Daston (1986) distinguish between more than two meanings of probability, an issue that unfortunately I have to leave to be treated elsewhere. See for example Gigerenzer (1994)

character of probability. On the one hand, probability in the seventeenth century referred to a rational degree of belief. It was, to put it in Hacking's terms, an "epistemological" notion "dedicated to assessing reasonable degrees of belief"[22]. The domain of this notion was primarily the courtroom in which an ever-recurring problem was (and is) when to convict an individual if certain evidence is lacking but it is nevertheless 'probable' that the accused has committed the crime. On the other hand, probability referred to the attempt to produce stable, long run frequencies. With the rise of insurances, annuities, and large scale lotteries there was an increasing interest in, and need for, constructing reliable measurements of death rates, ship returning rates, and so forth. Also these frequencies were put in terms, and understood to be, probabilities. The Janus-faced interpretation of the meaning of probability by Hacking and others does of course not imply that it was at the time understood as such. On the contrary, the beliefs of rational (or reasonable) men should correspond to the calculated frequencies of real world events. If they did not something had to be wrong with the calculation of the frequencies.

During the time of classical probability the mathematical description of the beliefs of rational man became increasingly problematic. The well known example here is the St. Petersburg paradox which intrigued scholars for many years. The reason for this is that it so clearly showed the discrepancy between the optimal action according to the mathematical logic employed, and the action a rational man was going to take. It was intuitively clear to everyone that no rational man would bet more than a seventeenth century's equivalent of four or five euros on a coin flipping gamble with a 2^n euro pay-off. This implied for the mathematical probabilists a problem with the mathematics, not with the rational beliefs of educated men. The proposed solution of Daniel Bernoulli to distinguish between mathematical and moral expectation, was hence an attempt to save the mathematics.

But there was a second reason why the attempt to describe the intuitive beliefs of rational man in mathematical terms failed, which can best be seen in the light of a general critique on the concept of the enlightened rational man. As the discussion in classical probability

[22] Hacking (1975), p.12

advanced throughout the eighteenth century it was increasingly the notion of rationality itself that was questioned. It became less and less clear what exactly entailed rationality, and which men could be said to be rational with respect to their beliefs. In first instance, rationality referred to the intuitive beliefs of educated (enlightened) men. But after it was shown that not only did educated men held different beliefs, but also that educated men far from always acted according to their rational beliefs, rationality became interpreted as referring to the beliefs of all human beings[23]. The interpretation of rationality shifted to the idea that all beliefs by human beings were fundamentally rational but that through religion, superstition, education and so forth these rational beliefs were corrupted. When also this interpretation became problematic the concept of rational intuitions of the enlightened man were abandoned as incorrect.

From this moment on mathematics concentrated on the second meaning of probability (that of the description of frequencies of real world events). The breakdown of the classical probability of the Enlightenment thus coincides with the rise of statistics. The disentanglement of the frequencies of real world events from the intuitions of rational man enabled this meaning of probability a rapid rise to prominence. The reason is that the mathematicians in their description of frequencies of real world events no longer were bound by the idea that these frequencies should somehow correspond to rational beliefs. Instead they could freely gather data and discuss the best way to construct frequencies. Thus the nineteenth century became the century of the rise of statistics. This rise of statistics is characterized by what Stigler labels a vertical and a horizontal development[24]. Vertical in the sense that the mathematics of statistics itself greatly advanced and in the sense that this meaning of probability became much better understood, horizontal in the sense that statistics spread to an increasing number of sciences by which it changed in fundamental ways.

After a century of development of the mathematics of statistics, it was Kolmogorov who in 1933 provided an axiomatic basis for the mathematics of statistics. From that moment on the mathematics of

[23] Hampson (1968), p.195

[24] Stigler (1986), p.4

statistics was largely agreed upon. The question of how and where to apply statistics, however, was far from settled. Especially in economics and psychology, where statistics became only of serious influence during the first half of the twentieth century, the debate fiercely raged. As in the other sciences in which statistics had risen to dominance, the debate in psychology in first instance circled around the question whether the nature of the subject of scientific inquiry was deterministic or indeterministic[25]. As for instance Maxwell had argued in the case of physics, a minority of psychologists began to argue the fundamental indeterministic character of the psychological science. The two most prominent scholars advocating indeterminism in psychology were Thurstone and Brunswik, and it is these two authors that are the historical speciality of Gigerenzer. Let us consider them in some more detail.

Determinism vs. indeterminism in psychology

The work of the psychologists Thurstone and Brunswik is analysed by Gigerenzer in the light of the discussion between determinism and indeterminism in psychology, and western science generally. To be sure, probability theory and statistics are not necessarily connected only with indeterminism. On the contrary, until well into the nineteenth century probability and statistics neatly fitted with the deterministic worldview held. The rationale behind employing probability and statistics in a deterministic world is that although an event can be predicted with certainty if all causes and relevant factors are known, in reality this is hardly ever possible. Thus, in a deterministic worldview it is held that if one knows all the relevant factors influencing the coin tossed, there is no surprise as to which side it is going to land on. In reality, however, we cannot with enough precision, if at all, determine all the relevant influencing factors and their values. Probability theory and statistics are here useful because they provide predictions of frequencies and probabilities in the absence of full information of the causes of an event.

[25] Although closely related, the rise of statistics in economics is different of character from that in psychology. For histories of statistics in economics see for example Klein and Morgan (2001),…..

In an indeterministic worldview, on the other hand, it is held that probability is a fundamental character of our world. It is argued that some processes are of an irreducible probabilistic nature. This implies that knowing all the causes does not provide us with certainty about the event. For a number of reasons the idea of an indeterministic world met with a lot of opposition. For one, as the business of science had always been to produce certain knowledge in a world of superstition and false beliefs, it was one thing to say that our knowledge of the world would always be imperfect. It was quite another thing to say that some phenomena were driven by chance and that nothing definite could ever be said about their causes. Maxwell personifies the indeterministic worldview for the natural sciences. In psychology Thurstone and Brunswik were the main advocates of an indeterministic approach.

Gigerenzer makes a distinction between two branches in the (American) psychology of the first half of the twentieth century: on the one hand the experimental psychology which employed the stimulus-response framework which originated in nineteenth century German psychophysics, on the other hand the correlational psychology that used correlational statistics after K. Pearson[26]. The main area of interest of experimental psychology was perception, of correlational psychology it was intelligence. On a few grounds these two psychologies differed fundamentally from one another. Experimental psychology studied the individual and tried to construct objective methods for measuring the sensational responses given by the subjects in the laboratory. It held a fully deterministic view of psychology that was created and maintained in the image of classical physics. The experiments were set up in a laboratory and used the (physics') laboratory method of isolation and control. Thus, many potential stimuli were controlled, after which one stimulus was varied and its corresponding sensational response measured. Correlational statistics on the other hand, used the correlational statistics of K. Pearson to construct the psychological characteristics of an average man. It would go to far, however, to label this an indeterministic

[26] This division corresponds with Danziger's (1990) experimental vs. applied psychology distinction. To be sure, the experimental vs. correlational division was the standard division made in early twentieth century (American) psychology.

approach to psychology[27]. The need to use correlational statistics was considered to result from the inherent imperfectness of our knowledge of nature. Nevertheless, the use of statistics in constructing averages was central to its method and to its view on how to do psychology. The main difference between correlational and experimental psychology is not so much a difference between determinism and indeterminism, but a difference of the subject of investigation. Experimental psychology investigated the individual in the laboratory, correlational psychology the psychological characteristics of an average man.

From 1927 onwards, both Brunswik and Thurstone advocated a way to reconcile the two opposing camps, Brunswik unsuccessfully, Thurstone successfully[28]. The alternative contained two main elements. Firstly, with respect to experimental psychology it was proposed to take the subject out of the laboratory and study it in its environment. Brunswik and Thurstone therewith attempted to combine the focus on the individual and the use of stimulus-response of the experimentalists with the conviction of the correlationalists that the psychological subjects should be investigated in their natural environment without any constraints. Secondly, Brunswik and Thurstone proposed to use statistics to solve the problem of the instable environment of these stimulus-response experiments in the real world. More specifically, they proposed to consider the stimulus perceived by the individual as a draw from a distribution (normal as a first approximation). The assumption was that the presentation of an object (say a light bulb or a sound) does not always lead to the exact same psychological value of the stimulus. When an individual is presented with, say, two sounds of which he has to decide which is the louder, the probability he will say *a* to be louder than *b* will depend on the overlap of the two distributions of the stimuli. In this respect it combined the correlationalists conviction that psychology should concern itself with averages with the experimentalists focus on

[27] Gigerenzer, Swijtink, Porter, Daston, Beatty and Krüger (1989),p. 60

[28] Gigerenzer is mainly interested in Brunswik. Although the work of the two, of course, differs, I attempt to summarize in the following the main propositions (on which they agreed, or so it seems) without going in the precise details of the arguments made.

individuals. The averaging of the responses given by the subjects functioned as a solution for the assumption of the distributional character of the subject's responses. It is in this respect that Brunswik spoke of subjects as "intuitive statisticians"[29].

In the proposed method of measuring responses in psychology by Thurstone and Brunswik, the human subject was the central figure. The experiments all depended on how, and with what magnitude the subject perceived the stimulus. Although the object was a given, the responses of the individuals differed because there are not two individuals exactly the same and differences will occur as to the magnitude of the stimulation resulting from the object presented. "Intuitive statistician" in this regard is not a statement about the constitution of the individual's mind but a statement of how to think of individual responses with respect to an aggregation of individual responses[30].

The psychological measurement method advanced by Brunswik and Thurstone did not survive and gave way to the measurement theory as formulated by S. Stevens. Stevens did away with the intuitive statistical part of the measurement and simply stated the magnitude of the stimulus to be equal to the measured magnitude of the object presented. If the volume of a tone was 50 dB, the magnitude of the stimulus was 50 dB. In a way he thus denied the individuality of the subjects. He did, for instance, not consider the possibility that some individuals had better ears than others. Subjects became interchangeable neutral measurement instruments. It also implied that the subjective processes that made the stimulus result in a certain response could be considered independently of the methods and instruments of measurement. The emphasis therewith shifted to the (mathematical) analysis of the measurement data. The concept of the "intuitive statistician", invented by Brunswik as a reference to the distributional character of the perceived stimulus, disappeared with the rest of the theory but made an unexpected re-appearance in the

[29] Despite the fact that Gigerenzer puts the discussion on Brunswik and Thurstone repeatedly in a determinism versus indeterminism story, it remains somewhat vague (to me at least) whether these authors should be considered indeterminists. In Gigerenzer (1987a, 1987b) his answer seems to be yes. In Gigerenzer (1994) it is decidedly no.

[30] Or so I conclude on the basis of Gigerenzer texts.

years following the Second World War when American psychologists gave a new interpretation to the term in their analyses of judgment under uncertainty.

The re-emergence of rational man

The period of the 1930s to the 1960s that saw the rise of statistics in psychology and economics also saw an as yet unexplained[31] re-emergence of rational man. Again was posed the idea of a rational man that would take decisions according to the optimal solution as calculated by mathematics. The notion of probability used this time to determine the optimal decision was the subjectivist (or Bayesian, or epistemic) interpretation of probability, mixed with the utility maximizing interpretation of rationality as advanced by von Neumann and Morgentern[32]. Rationality thus was given a rather specific meaning and deviations from the mathematically determined optimal rational behavior became quickly interpreted as (irrational) errors. But for whatever reason the re-emergence of rational man occurred, it was (again) a wrong turn. Its narrow focus on one type of probability combined with a useless understanding of rationality forms the basis for Gigerenzer's critique on Kahneman and Tversky. The dominant notion of rationality used by so many social scientists also functions as the benchmark by which Gigerenzer contrasts his own cognitive psychological theory. In the following we will first address Gigerenzer's critique on Kahneman and Tversky, after which we will turn to Gigerenzer and his rationalities.

4. Gigerenzer's critique on Kahneman and Tversky

Gigerenzer's critique of the cognitive psychology of Kahneman and Tversky runs along two closely related lines: statistics and rationality. His critique is both an extension of his historical work on statistics, as a negative definition of the psychological research programme he later starts to advance. Gigerenzer's work can hence both thematically and

[31] That is, unexplained according to Gigerenzer c.s.. In view of for example Carnap (1944), logical positivism could be a first place to look.

[32] I would suggest that also elements of the Keynesian/Ramseyan theory of rational partial beliefs play a role. This is however not accounted for in the expositions of Gigerenzer c.s..

chronologically be organised as: history of statistics => critique on Kahneman and Tversky => bounded rationality programme. The intimate connection of these three themes in the work of Gigerenzer is reflected in his publications, which hardly ever deal with just one of his interests. The publications used for this section[33] can nevertheless be considered as the core of his critique on Tversky and Kahneman. It should furthermore be noted that not all psychologists consider the difference as fundamental as Gigerenzer. Generally speaking cognitive psychologists seem to consider the difference between Gigerenzer and Kahneman and Tversky to be a difference of degree, and not so much a fundamental difference of insights[34].

One of the things Gigerenzer cannot stress enough is that *the* theory of probability or statistics does not exist. Most scientists are familiar with the difference between the Bayesian, or subjectivist, and the frequentist, or objectivist, school in statistics. These two schools in statistics often arrive at different solutions to statistical problems. But it runs deeper than that. The fundamental difference of opinion of R. Fisher, and Neyman and E. Pearson on how to do something as fundamental as hypothesis testing, for instance, has never been solved. Almost all textbooks on statistics after the Second World War have ignored this difference and have presented statistics as one consistent and unattested body of knowledge, a fallacious account of past and present debate in statistics. The fact that Kahneman and Tversky only use Bayesian statistics is hence a narrow focus that is constraining and unnecessary. The fact that they use Bayesian statistics to construct the optimal (or normative, or rational) decision really misses the point, according to Gigerenzer. If highly educated mathematicians do not agree on the best way to apply statistics, it is absurd to suppose that individuals should behave according to one of these methods.

But even within the realm of the Bayesian statistics Tversky and Kahneman make assertions that are at best only part of the truth. In this regard the recurring example in Gigerenzer's work is that of the

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[33] i.e. Gigerenzer (1991,1993,1994,1996), Gigerenzer, Hell and Blank (1988), Gigerenzer and Murray (1987), Sedlmeier and Gigerenzer (1997,2000), Hertwig and Gigerenzer (1999)

[34] Frey and Benz (2004)

phenomenon of base rate neglect, as discovered by Tversky and Kahneman. In different experiments Kahneman and Tversky show that people often neglect the base rate distribution of a population they have to use to solve a problem. A well-known experiment is the cab-accident problem, which runs as follows. A witness has seen how in the middle of the night a cab caused an accident and drove away before anyone could register its number plate. In the town there are only blue (25%) cabs and green cabs (75%). The witness is positive he has seen a green cab, but experiments under similar conditions have shown he is only right in 80% percent of the cases. What is the probability the cab indeed was green? Kahneman and Tversky show that individuals fail to take the base rate of the cabs (25% blue, 75% green) into account and thus commit the fallacy of base rate neglect. Gigerenzer, however, argues that the answer depends on which base rates one takes. Should one take the percentages of each colour cab in town? Or should one for instance use the base rate of the percentages of each colour cab involved in accidents? Or the percentages of each colour that work at night? Or in the specific part of town in which the accident occurred? One can, in other words, not accuse the individual of ignoring one pair of base rates when it is not clear which base rates one should use in the particular case. To use another illustration of this point by Gigerenzer, if you are invited to the dean's party and are asked to guess the profession of a to you unknown 50 year old male, it does not seem a good strategy to base your guess upon the country's base rates of male professions. A-select draws in reality almost never occur.

The critique on Kahneman and Tversky's base rate fallacy can be placed under the heading of Gigerenzer's critique on biases generally. In a range of publications Gigerenzer's shows that the cognitive illusions of Tversky and Kahneman can be made to "disappear, reappear, or even invert"[35]. He employs both theoretical and experimental arguments to make his case. The base rate neglect as mentioned above functions in Gigerenzer's work as a good example of how a bias can be shown to disappear when the theory upon which it is built is carefully scrutinized. But also for instance the overconfidence bias and the conjunction bias can be made to

[35] Gigerenzer (2000), p.243

disappear. In both these cases Gigerenzer shows that the reported bias is a result of the experimental setting. In the case of the overconfidence bias it is shown that individuals distinguish between single events and long run frequencies. If subjects are given questions of the type: which city has more inhabitants a) Hyderabad, or b) Islamabad, and have to indicate how confident they are their answer is correct (choosing between 50,60,70,80,90 and 100%), their average confidence is indeed higher than the average of correct answers. However, if they are asked how well they did after an experiment with fifty such questions, their estimates are, on average, practically the same as the true average of correct answers. Thus, Gigerenzer argues that human beings show no overconfidence bias when faced with frequencies. This only leaves a supposed overconfidence bias in the case of single events, which is dismissed by Gigerenzer on theoretical grounds. Building on the authority of Richard von Mises and de Finetti, Gigerenzer reminds the reader that probability applied to a single event is meaningless. The arguments against, and falsifications of, the conjunction fallacy follow a similar pattern. If subjects are asked for frequencies they do pretty well, (subjective) probabilities of single events make no sense.

Placing Gigerenzer's critique on the Biases of Kahneman and Tversky under an again more general heading it is to be understood as resulting from Gigerenzer's disagreement with what he labels 'the normative issue'. In the normative-descriptive framework as employed by Kahneman and Tversky, observed judgments are compared with a norm. But what is normative here? Gigerenzer shows that on the basis of probability theory and statistics it is not possible to construct *one* correct answer (or *one* norm) in judgments under uncertainty. There is furthermore far from any agreement on which situations probability and/or statistical inference may be applied to. A unique norm, in other words, cannot be constructed and a normative decision theory is hence meaningless[36].

[36] In the Nobel prize interview (Kahneman (2002)) Kahneman more or less concedes on this point by noting that the reason he and Tversky put so much emphasis on the normative issue was the hope and belief in the 1970s of ultimately being able to construct an unambiguous tool for (political/administrative) decision making.

The normative decision theory which is the result of conceiving of the human mind as a (Bayesian) intuitive statistician has for some time[37] been used by Gigerenzer as an example of a philosophy of science theory labelled tools-to-theories. The idea is that what initially starts out as a tool (sometimes) becomes a theory of its own. In case of Edwards, Kahneman, Tversky and others the tool of the metaphor of the intuitive statistician as it was originally proposed by Brunswik transforms into a theory of the mind as an intuitive statistician. The 'normative issue', then, naturally follows from this transformation. Needless to say Gigerenzer disagrees with tools transforming into theories.

But Gigerenzer's attack after attack on the normative issue is also the result of a more general dissatisfaction with the project of cognitive psychology, or so I would argue. What lies at the background of Gigerenzer's criticism of Tversky and Kahneman c.s. is the focus on decisions that are made on the basis of cognition. Gigerenzer does not agree with cognitive psychology leaving out other characteristics of human psychology, like emotions, passions, and so forth. What lies at the bottom of Gigerenzer's ideas of how to do cognitive psychology is the conviction that when human decision making is investigated, the *entire* human being should be considered, not just a part of it. This provides the clue to the final, and to my mind most fundamental, criticism on Kahneman and Tversky, namely on their use of the concept of rationality; as well as to the research program on the different rationalities as Gigerenzer starts to promote it from roughly 1997 onwards.

When Kahneman and Tversky start their collaborative research there already exists quite an extensive literature on how human beings deal with probability and statistics[38]. However, all this research follows Savage (1954) in that it only investigates 'small world' situations. A 'small world' in Savage's theory is a controlled laboratory setting in which exists clear agreement on the probabilities used. The 'small world' theory only considers problems for which the cognitive capacities of the mind are required. Just like the cognitive sciences generally it does not argue that all behavior is cognitive or rational,

[37] Roughly on and off since 1987

[38] For an overview see Peterson and Beach (1967)

but simply restricts itself to the behavior in which no emotions and so forth are involved. Examples include the judgments which of two light bulbs is brighter, or the judgment of which amount of money is more preferred.

Kahneman and Tversky apply this theoretical framework to real-life situations. The Heuristics and Biases theory they propose is an extrapolation of intuitive statistics research to the real world. Kahneman and Tversky implicitly argue that every situation in life that involves in one way or another probabilistic and/or statistical inference can be analysed in terms of the theory put forth by Savage[39]. Therewith, Gigerenzer argues[40], Tversky and Kahneman reason that all decisions human beings make in real life that involve uncertainty are made on the basis of cognition, or rationality. By implication deviations from the calculated optimal decision thereby become irrational errors or fallacies. This then is at the basis of Tversky and Kahneman's theory, and equally at the basis of Gigerenzer's critique.

The employed definition of rationality Kahneman and Tversky take over from Savage's small world theory assumes subjects to correctly calculate, given all the relevant information, the optimal solution that maximizes pay-off, utility or whatever. Apart from the fact that in real life this would often take far too long, it also ignores emotions, passions etc.. Furthermore it leads to behavior that we never observe. In this respect, the relevant passage in *Cognition as Intuitive Statistics* (1987), finally, is worth quoting at length:

> "In sum, the belief that Bayes' theorem or another theory of statistical inference is rational thinking empties rationality of its most interesting aspects. Rationality is without purpose, without weights for conflicting goals, without information search and exploration, without judgments of relevance, without reflection on content, without use of contextual information and cost-benefit considerations. To the disappointment of philosophers like Popper, it also leaves out the context of discovery, the truly

[39] Indeed, one of the main criticisms in the decision theory camp on Kahneman and Tversky is precisely this un-authorized extrapolation of the 'small world' theory to the real world.

[40] That is to say, in my reading of Gigerenzer.

> psychological problem of creativity explored by the Würzburg and Gestalt schools. And the fascination with such mechanical rationality tends to divorce the psychology of inductive thinking from all these aspects, if only by dismissing them as sources of irrationality. Our view is that the task of a psychology of thinking is to understand how these various sources direct human thought rather than to judge whether thinking is rational or not." (p.181)

5. Gigerenzer and his rationalities

The dissatisfaction of Gigerenzer with the cognitive psychology of Kahneman and Tversky is the result of a general dissatisfaction with cognitive psychology. His objection is principally the fact that cognitive psychology is only concerned with the cognitive part of psychology, instead of the human being in its entirety. Gigerenzer does not disagree with the idea that some part of our behavior can be labelled 'cognitive', but with the fact that such a narrow focus ignores the influence of other psychological characteristics.

When in 1997 Gigerenzer becomes the director of a Max Planck research group in Berlin he comes into a position in which he can advance his own approach. Roughly from this moment on his dissatisfaction with the standard paradigm, as well as his proposed alternative, is formulated in terms of rationality. His critique on the dominant interpretation of the individual calculating the optimal response or action and behaving accordingly is that it is unrealistic. In reality people do not have all the relevant information to calculate the optimal action, nor the capacities to do so (especially if we consider the limited time in which judgments and decisions often have to be made). The emphasis on the cognitive part of human behavior has led cognitive psychologists to theories in which every judgment is made and every decision taken as if it were deliberate, cognitive, rational behavior. This is a mistaken view according to Gigerenzer.

Gigerenzer formulates his alternative in terms of other, different, types of rationalities. In this, he explicitly follows the bounded rationality framework as set out by Simon. In the bounded rationality account of human behavior as it is put forth by Simon, behavior of human beings should be understood as the result of two

phenomena. These two phenomena are understood to cut together like a pair of scissors. They are necessary complements. On the one hand we find the incompleteness of the information upon which humans have to base their judgment and decisions, as it is in reality never the case that we posses all the relevant information for the decision to be taken. The other blade of the pair of scissors consists of the limited computational capacities of human beings. We simply do not have the capability to put all the available information together in a careful and complex calculation. Human beings are rational in the sense that they try to make the best judgment or decision. They are however only boundedly so given both the incompleteness of the information and the limitness of their computational capacities[41].

Then how does the decision and judgment making happen given these two blades of scissors? Gigerenzer argues, again in the spirit of Simon, that this happens on the basis of heuristics. A crucial factor that Gigerenzer adds is that these heuristics are fast and frugal. The idea is that in our decision and judgment making we humans employ heuristics that lead to quick response and demand as little computational capacities as possible. In contrast to the heuristics as advanced by Kahneman and Tversky, Gigerenzer's heuristics are not simplified versions of complex (rational) decision making rules but simple rules of thumb that work well in a certain context. To use the same example as before, in an often used experiment of Gigerenzer subjects are asked for a series of pairs of cities which is the larger one in terms of inhabitants. He shows that subjects use a list of cues on which they base their decision. The first cue is whether the subject knows both cities. If only one city is known, or far better known, the process stops and that city is decided to be the larger one. If both cities are equally well known, a second cue may for example be if the soccer clubs of both cities are known. If the soccer club of one city is (far better) known than the soccer club of the other city, than that city is taken to be the larger one. And so forth. Individuals thus use fast and frugal heuristics that yield the best result for a given decision or judgment problem.

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[41] In line with Gigerenzer's earlier historical work as well as in line with his critique on Kahneman and Tversky, the bounded rationality he favours is explicitly defined as *not* involving utilities and probabilities.

Gigerenzer's research at his centre for Adaptive Behavior and Cognition (ABC) investigates how and why these heuristics (sometimes described as forming part of an 'adaptive toolbox') function. The how question investigates which heuristics are used in which situations and how a choice is made between different heuristics. The example described above of the heuristic involved in choosing the larger of two cities is an example of this research. But Gigerenzer is also interested in answering why these heuristics function so well. In an extension of the two-city experiment he conducts for instance an empirical investigation of the correlation between the size of a city and how often it is mentioned in a newspaper. As these correlations are fairly high he thus shows why the simple rule of thumb works so well.

Given an understanding of humans and their environment in terms of bounded rationality, Gigerenzer investigates the heuristics humans employ to deal with the incomplete information and limited computational capacities at their disposal. Because such a definition is still too broad, in his research institute he has made a sub-division of four areas of research (along a fifth called 'Methods, Metaphors, and Theory Construction' which elaborates on his tools-to-theories research), namely 1) bounded rationality, 2) ecological rationality, 3) social rationality, and 4) evolutionary rationality. The bounded rationality research area explores the general structure as set out above. The other three rationalities each explore different structures that determine the type or pattern of information the subjects face. Ecological and evolutionary rationality reflect the idea that over the course of our evolution our heuristics have evolved in such a way as to optimally employ the structure of our environment. Research in the evolutionary branch (which is situated in, or linked with, evolutionary psychology) investigates how and why these heuristics evolved over (evolutionary) time. The research conducted under the heading of ecological rationality investigates these heuristics themselves. Social rationality, on the other hand, reflects the idea that humans also need heuristics to deal with the quickly changing structure of our social environments. Research under this heading investigates these heuristics.

At the moment of writing it is the focus on these last three types of rationalities, which emphasize the type of the environment in which

the decision or judgment has to be made, that most effort is put into. It thus seems as if the emphasis on the heuristics themselves has shifted somewhat to the investigation of the role of the environment in the decision and judgment making process. Another indication in that direction is Gigerenzer's recent interest in linking (his) psychology with other sciences, notably behavioral biology[42] and economics[43]. It seems (but this is speculation) that Gigerenzer has come to the conclusion that all sciences that have something to say about behavior should be linked together. A less friendly interpretation is that he wants to advance his program in as many areas as possible. What seems to happen in any case at this moment is that Gigerenzer is offering his psychological worldview to economist as a better alternative to base economic theories upon, and is looking to biology to sharpen his human decision making theories and to provide them with a biological grounding.

Conclusion

This paper has provided an overview of the work of the psychologist Gigerenzer, in which the focus has been on the more visible parts of his research. Gigerenzer starts his academic research in the late 1970s as a cognitive psychologist whose main interest is on questions of measurement in psychophysics. In the early 1980s he becomes involved in a broad collaborative research program on the history of probability theory and statistics, which lasts from roughly 1981 to 1989. Gigerenzer's criticism of the psychology of Kahneman and Tversky is to a large extent shaped by this historical research. At the risk of exaggeration one could summarize Gigerenzer's criticisms in the verdict that Kahneman and Tversky do not understand probability, statistics and rationality.

Gigerenzer's own research program at the ABC in Berlin from 1997 onwards continues in turn on his critique of Tversky and Kahneman. It is an implication of it. Gigerenzer starts where he argues Tversky and Kahneman go wrong. His alternative builds on the bounded rationality work of Simon in which decision-making should be understood as the interaction of imperfect information and limited

[42] e.g. Hutchinson and Gigerenzer (2005)

[43] e.g. Gigerenzer and Selten (2001)

computational capacities. In recent years Gigerenzer attempts to surpass the framework as set out by Simon by distinguishing between different types of rationalities and by linking his research program with other sciences that make claims about human behavior.

The psychologies of Gigerenzer on the one, and of Kahneman and Tversky on the other hand are both virtually the same and fundamentally different. Both approaches consider decision making the main approach to investigate human psychology. Both also agree that human decision making is directed by heuristics and offer explanations on how and why these heuristics work, and how they relate to one another. However, it is impossible to theorize about decision making *without* utilities and probabilities in Kahneman and Tversky's worldview, where in Gigerenzer's worldview it is impossible to theorize about decision making *with* utilities and probabilities. Despite Gigerenzer's convincing and decided rhetoric, the dispute remains as yet undecided.

Appendix: Tversky and Kahneman's response to Gigerenzer's allegations

The highest trees catch the most wind is the Dutch saying. It is thus not surprising that the theory advanced by such visible cognitive psychologists as Kahneman and Tversky has been attacked, disproved and adjusted by a wide range of authors. However, Tversky and Kahneman have only responded to what has probably been their most vigorous opponent, and even this response amounted to no more than one article and a very brief postscript.

It is nevertheless difficult to summarize what the response in *On the Reality of Cognitive Illusions* (1996) exactly entails. Also Gigerenzer's immediate reply *On Narrow Norms and Vague Heuristics: A Reply to Kahneman and Tversky (1996)* (1996) does not add a lot to the clarification of the dispute. To a considerable extent this has to do with the fact that both parties more or less evade a discussion. The main accusation of Kahneman and Tversky is their being misrepresented by Gigerenzer. In their fierce rhetoric this is put in the following terms:

> "Gigerenzer's reports on out work and on the evidence cannot be taken at face value. [..] The position described by Gigerenzer is indeed easy to refute, but it bears little resemblance to ours. It is useful to remember that the refutation of a caricature can be no more that a caricature of a refutation." (p.584)

Gigerenzer answers by arguing that the real problem does not lie on the level of the current (theoretical, empirical) discussion, but on a philosophical level.

> "I welcome Kahneman and Tversky's (1996) reply to my critique [..] and hope this exchange will encourage a rethinking of research strategies. I emphasize research strategies, rather than specific empirical results or even explanations of those results, because I believe that this debate is fundamentally about what constitutes a good question and a satisfactory answer in psychological research on reasoning." (p.593)

The discussion that does develop consists of two points. Firstly Kahneman and Tversky retort Gigerenzer's argument that the biases disappear when the questions are formulated in terms of frequencies by pointing out that along Bayesian probabilities for singular events they have also studied judgment of frequencies. They refer to a list of publications in which they show that also in judgments of frequencies people commit mistakes. In a second argument under the heading "The Normative Issue", Kahneman and Tversky argue that in contrast to what Gigerenzer repeatedly shows, 'bias', 'fallacies', or 'mistakes' are real and thus indicate the existence of some norm.

> "This position [of Gigerenzer –FH], which may be characterized as normative agnosticism, is unreasonable permissive. Is it not a mistake for a speaker to assign probabilities of .99 both to an event and to its complement? We think that such judgments should be treated as mistaken; they violate accepted constraints on the use of probability in everyday discourse." (p.586)

As said, the main part of Gigerenzer's reply consists of arguing that the real difference between him and Kahneman and Tversky consists of a difference in opinion of which philosophy of science practice to follow. His response to Kahneman and Tversky's accusation of him misrepresenting them is that Kahneman and Tversky in turn misrepresent and fail to understand him. The final and most fundamental criticism of Gigerenzer on Kahneman and Tversky then is that the latter's heuristics and biases are post hoc explanations that prove everything and nothing, and cannot be falsified. Indeed, in his reply Gigerenzer in the end commits himself to a Popperian falsificationist, point of view. In the last sentence of the last postscript he thus states:

> "As I see it, there are two ways in which a theory can fail: by being wrong or by being indeterminate. The latter may be worse for scientific progress, because indeterminate theories resist attempts to prove, disprove, or even improve them. Twenty-five years ago, extending on Ward Edward's work, Kahneman and

Tversky opened up a fertile field. Now it is time to plant theories." (p.596)

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