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Statistical priesthood *)

(Savage on personal probabilities [1])

D. van Dantzig

S a m e n v a t t i n g

In verband met de huidige ontwikkeling van de ekonomische beslissingstheorie heeft de subjectivistische opvatting van het waarschijnlijkheidsbegrip tegenwoordig weer meer aanhangers dan enige tijd geleden. Eén daarvan is L e o n a r d J. S a v a g e, die deze opvatting in zijn boek "The foundations of statistics" [1] nadrukkelijk verdedigt. Daarom wordt in dit artikel het boek van S a v a g e en de subjectivistische opvatting in het algemeen aan een kritisch onderzoek onderworpen. Ter vergelijking worden de opvattingen van J a c o b B e r n o u l l i, en ook die van de schrijver van dit artikel, kort samengevat. Het boek bevat vele en ernstige gebreken. Noch zijn beschouwingen, noch zijn voorbeelden wettigen het vermoeden dat schrijver veel statistische ervaring bezit.

Voorts wordt uitvoerig aangetoond, dat het „beginsel van de kleinste spijt" op niet ter zake doende gronden kan leiden tot strategieën die verre van optimaal zijn en altijd bijna het slechtst mogelijke resultaat hebben. Dit van S a v a g e afkomstige beginsel mag daarmede wel als definitief weerlegd worden beschouwd.

1. Statisticians' subjectivity

When asked about the nature of their work, most statisticians will, I think, answer that it deals with the characteristics of events (or objects), determined among a class ("population") of such events by means of a "random" (or "aselect") procedure, i.e. a procedure determining which event in the class will be realized by a method which does not depend on the individual characteristics of this event, or by means of another, sufficiently similar procedure. Most modern statisticians will hardly consider the conclusions they arrive at (like the statements made by art critics, metaphysicians or "verstehende" psychologists), as being of a "subjective" nature, i.e. depending on their mood, their taste, their "Einfühlung", their philosophy, or other personal characteristics, other than their greater or lesser knowledge of their science. I wonder whether any statistician would accept as a reasonable explanation of a case in which he does not find a significant result, whereas his colleague does, that this is due to differences in their metabolism, their hormonic constitution or their Oedipus complex. It cannot be denied, of course, that a man cannot be separated from his emotions, and that the set of propositions he accepts as being true, or probable, is not completely inde-

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pendent of his personality, and his emotional, traditional and social background. But most statisticians will consider, justly, I think, this dependence on their personality to be sufficiently remote from their daily work not to endanger the "objective", or at least "interpersonal" ("multi-subjective"), validity of their results, and, in any case, believe that the *value* of their work is closely related to the degree to which it is independent of their personal likes and dislikes. Although, admittedly, there are cases of doubt, and there might be at least one controversy in statistics concerning the preferability, or even validity, of some specific statistical methods — and thereby not without influence on actual results — which some onlookers might perhaps contribute partially to personal characteristics and backgrounds of some of the participants, to their personal preferences and dislikes.

2. The art of guessing

Nevertheless since a few decades some philosophers have tried to establish the thesis that the probabilities attributed by a person to possible events necessarily are of a subjective nature. This thesis is sometimes said to go back to Jacob Bernoulli. This, however, does hardly do justice to his logical perspicacity. The main ideas, developed by Bernoulli in the beginning of the fourth part of his "Ars Conjectandi" may shortly be expressed as follows.

1. The knowledge which an individual — or even the whole of mankind — at a given moment has at his disposal is often insufficient to obtain certainty about the occurrence or non-occurrence of a possible event.

2. On this basis he can only obtain a definite "degree of confidence", which "is to certainty as a part to its whole", and which is called "probability". It depends on the knowledge he has at his disposal, and therefore may vary from one individual to another.

3. The "art of guessing" (Ars Conjectandi) consists of estimating, *as precisely as possible*, the correct values of the probabilities (relative to the (body of) knowledge at one's disposal) of different possible events.

4. The degree of confidence to be attributed to a possibility depends on the weights of the arguments for and against it.

5. The weight of an argument of a rather general type depends on the number of those cases — within a class of cases in which it may hold — in which it is valid, provided these cases "can occur equally likely".

6. Except in the simple cases of games of chance, where the equiprobability is artificially enforced, we can know only rarely whether different cases are equally likely or not, or attribute a priori correct probabilities to them.

7. We can then, knowledge a priori of probabilities lacking, estimate them a posteriori by means of a large number of observations.

8. Bernoulli's theorem (the mathematical "law of large numbers") proves, and serves to prove, that under an indefinitely increasing number of observations the probability of approaching the true ratio ultimately *exceeds every degree* of confidence < 1, so that there is no "asymptotic" degree of confidence less than certainty, which cannot be surpassed by increasing the number of experiments.

Evidently it is quite a different thing to say that a probability may depend on a body of knowledge — which may vary from one person to another — than to maintain that it depends on the personal *preferences* of the individual, so that it may vary, even if the body of data remains the same. This difference is usually overlooked by subjectivists. Anyhow, their thesis cannot be supported by Bernoulli's authority, who, obviously, believes that, on given data, there are correct and false values of probabilities, and correct and wrong ways of estimating them. Bernoulli's theory is decidedly not a subjective one.

3. New subjectivism

Before the war perhaps the most fervent supporter of the subjectivistic creed was Bruno de Finetti, then an actuarian in Trieste, and a mathematician of great merit. Although it seemed for some time that he fought for a lost case and that a final clarification in the foundations of probability theory had been reached, some other mathematicians have since then accepted more or less similar ideas. At present the confusion seems to be greater than it was ever before.

Recently Leonard J. Savage has in his book, curiously called "The Foundations of Statistics", joined de Finetti in his struggle for subjectivism. Apart from de Finetti, Savage bases his theory upon F. P. Ramsey (F. P. Ramsey, The foundation of mathematics, 1931, pp. 157–211, published posthumously). Savage derives from Ramsey, and from the von Neumann-Morgenstern and the Chicago school of econometrists, the idea that the expectations of profit for any individual (judged according to a definite utility function which may be different from the monetary value, the only one which Ramsey considered) admit a complete ordering, and that therefrom the utilities which different gains, if they occur, have for him, as well as the probabilities of possible gains, may be derived.

Savage, like his predecessors, thinks that the only consistent interpretation of probability is a subjective degree of confidence that a person has in the truth of a proposition (p. 3), and that this is the only probability concept essential to science and other activities that call upon probability (p. 56). This implies that except for violation of axioms, it is meaningless to distinguish between correct and erroneous evaluations or estimates of probability, a difference which Jacob Bernoulli intended to teach. Actually it is difficult to see why subjectivists do not advocate, besides subjective statistics, subjective kinematics, based on the „Kopfuhr", subjective geometry, based on personal estimates of lengths with optical delusions forbidden, and subjective physics, based on individual force, individual energy and personal magnetism. By this extreme subjectivism the statistician, rather than being a scientist whose work can be controlled by any one of his colleagues who, if using the same data and correct methods, will arrive at the same conclusions, gets a status like that of an ancient priest, whose statements the layman, unable to reproduce the statistician's order of preferences, has to accept without a possibility of criticism. Statistics, inasmuch as it would remain a science at all, would become a "sacred and secret" science.

Combining the subjectivist's view with the statement in modern physics that radiation consists of probability waves, the reader would have to conclude that the inhabitants of Hiroshima and Nagasaki have been killed by waves of subjective degrees of expectation. No little straining of the signification of words seems to be needed in order to maintain this.

4. Persons or ghosts?

When trying to evaluate the merits of Savage's book one is first of all struck by the author's honesty in dealing with his opponents' views, his modesty in dealing with his own views, and his efforts at careful phrasing. He also sometimes is quite witty, as on p. 27: "Many are convinced that such statements about probability to a person mean precisely nothing, or at any rate that they mean nothing precisely". Indeed.

Another, and more fundamental point in the author's favour is his trying to avoid a confusion often caused. Proponents of a subjectivistic theory often confuse

1°. the view that probability theory deals with the subjective probabilities actually attributed by an individual person to all possible events and

2°. the view that it deals with the probalitities the individual *ought to* attribute to the events according to some given standard.

The second view can also be described by saying that the theory deals with the probabilities which a fictitious, idealized person would attribute to the events. If one takes the first view consistently, the axiomatic treatment of probability theory would require the empirical proof that the individual under consideration *actually* attributes such probabilities to the events that the axioms are satisfied, e.g. that no circularity in their order ($P\{A\} > P\{B\} > P\{C\} > P\{A\}$) occurs. When, however, an individual tries to perform other and easier orderings, e.g. to compare visually (without actual measurement!) lengths or to arrange colours, he usually runs rapidly into inconsistencies. Therefore I, for one, have not the slightest doubt that no individual would succeed in ordering his probabilities of all (or even of many) events (or preferences of all possible acts) without inconsistency. Ordering the extremely complicated complex of emotions and experiences, which preferences for sets, or even probabilities of their consequences, are, is much more like ordering individual clouds in the air or individual waves in the sea according to, say, their size. It just *cannot* be done consistently, i.a. because they are not uniquely individualizable, and because whatever partial order *could* be established would be valid only for a single moment. Hence, if probabilities are defined as characteristics of an individual, then there *are* subjective probabilities, but there is, the axioms not being consistently satisfied, no *calculus* of subjective probabilities. Except perhaps a statistical one! For it might be possible to make an objective statistical study of the inconsistencies committed by the individual members of a population of statisticians whenever they make use — if any — of subjective probabilities.

The second view, which Savage calls the "normative" one, because a real person *should* behave as much as possible like the idealized one, does not suffer from this inconsistency. As soon, however, as one imagines a fictitious person, one can attribute to him any consistency (or other characteristic) one likes. Savage's "rational person" then becomes a member of the somewhat ill-famed family to which also belong i.a. Laplace's superior spirit, who knew with absolute precision and completeness the present state of the world and thereby could predict with certainty every future event, Maxwell's „demon", who could admit or send back the molecules of a gas impingeing on a semi-permeable wall and thereby decrease the entropy, Flammarion's „Lumen", who could travel with a speed greater than that of light and thereby observe phenomena with the time-order reversed. Although his doubtful ancestry alone cannot seriously be held against the youngest descendant, most scientists will agree today that such spectres on the whole have done more harm than good to the philosophy of science. Nevertheless, it must be considered as a merit of Savage that he *did* make the distinction between the two interpretations, and deliberately chose the second one.

The reader would, however, get an entirely wrong impression of the author's book, if we mentioned these favourable aspects only, without going into some of the serious arguments which can and must be brought forward against it. The length to which we have to go for this purpose may be justified by the importance of contributing to the dissipation of the confusion created by the modern subjectivistic creed.

5. Lack of logical precision

Notwithstanding the use of axiomatic methods and a considerable display of formal logic, the treatment is not logically rigorous. The most fundamental defect is the lack of precise delimitation of the sets (of "states", "acts" and "consequences") which are used. In the introductory example (p. 14), based on the not quite startling idea that one rotten egg spoils an omelet, he considers two "states of the world" ("good", "rotten") and three acts ("break a doubtful egg into a bowl", containing five good eggs, "break it into a saucer" and "throw it away"). These lead to 6 possible consequences, i.e. the consequences are a function of two variables, viz. acts A and states S: $C = f(A, S)$, say. On the same page, however, the author considers acts as functions of states, having consequences as "values": $C = A(S)$. This would require that the set of states and the set of consequences, i.e. the range and the domain of the functions A, were clearly circumscribed. The author wants, instead of deriving preferences between acts from preferences between consequences, to order consequences by means of preferences between acts, which is not quite the natural thing to do. For this purpose he assumes implicitly (notably without an additional existence postulate which should have been inserted here) on p. 25 that *all* functions from states to consequences are acts, in particular those which are constant, i.e. which lead to the same consequence whatever state the world is in. This would necessitate him to consider in his egg-example instead of three acts 36 acts [1]) (the number of different functions of a two valued argument to a set of 6 consequences) [2]). Moreover, the constant act, leading, whatever state the world is in, to the highest evaluated consequence (the "highest income") would always be preferred to all others. If this „act" were actually possible, everyone would choose it, and not only the author's book but the whole decision theory would become superfluous. Hence, not only the actually choosable acts, but even the "possibly possible" ones must be assumed to be ordered according to preference.

6. Acts and meta-acts

A second, more fundamental, objection also results from the author's lack

[1]) On p. 15 the author without any ado changes the number of states from 2 to 4. This would necessitate considering $6^4 = 1296$ possible egg-acts.

[2]) More generally, if a, s and c are the numbers of acts, states and consequences, the original definition requires $c = as$, if, as in his example, all consequences are different, whereas the latter leads to $a = c^s$.

of defining his set of acts precisely. If the term is taken in its ordinary sense, the *establishment* of an order of preferences between acts is itself an „act". Hence if *all* possible acts are to be ordered, the ordering itself is one of them. This would not only lead to an infinite regression like the one the author wishes to shun on p. 58, but to logical contradictions.

For this reason the set of acts must be delimitated, and the *acceptance of an ordering* between them must not itself *belong* to them, in the same way as in logic metamathematics must be distinguished from mathematics. I.e. there are necessarily acts which fall outside the ordering, and for which propabilities and utilities are *not* defined. In the ordinary statistical theories these acts correspond with (possible acceptances of) hypotheses, to which, just for this reason, no probabilities are attributed there. If the author had realized this, he would have laid the customary restrictions on the applicability of Bayes' principle of inverse probabilities (p. 47), which he fails to do, thereby introducing implicitly and without a word, the forementioned difficulties, if not contradictions, into his theory.

7. Staticity

Another serious drawback of the theory is its staticity. Although Savage considers "learning by experience", he does so only on the basis of Bayes' theory by adapting a posteriori probabilities to observation, which is insufficient already for a theory of induction, for it does not take into account the fact that several of the most important discoveries in science concern phenomena which were considered impossible, or even unimaginable before, i.e. which had a priori probability zero, hence also on whatever observation a posteriori probability zero; or which even did not occur at all in the original probability field. For this reason the author's criticism (p. 62) of the objectivistic view on the ground that "it is the business of the probabilist to analyse the concept of experience" shoots beside the mark as *no* probability theory is able to perform this task (although several philosophers and probabilists believe it is) and backfires, as the author's subjectivistic theory does so least of all. Any theory of *inductive inference* requires the admission that whatever event now is assumed to have probability zero, perhaps even to be logically contradictory, may have to be accepted in future, hence, never to attribute a probability *exactly* $= 0$ (or $= 1$) to any event whatsoever. But this does away with the present form of probability theory, requiring $P\{A \text{ and non} - A\} = 0$, and leads to a more relativistic theory, which might be resumed in the form:

Never say "never".

In the same context the author criticizes views expressed by Féraud and myself (with the t omitted from my name) (p. 63): "Very crudely, it seems to be their position that in any context it is allowable for a person to act as though some event of sufficiently small (objective) probability, chosen at his discretion, were impossible". Speaking for myself alone, first of all the words "(objective)" and "chosen at his discretion" are Savage's invention, and moreover my point is not so much the question whether this procedure in "allowable", as rather the fact that a real (not"rationalized") person *does* act in this way: nobody *does* take account of extremely small probabilities, e.g. of the order of $10^{-10^{10}}$, or, usually, even of 10^{-10}. Hence the argument is as "personalistic" and as "behaviouristic" as Savage might desire and perhaps even more so than most of those he uses himself with respect to his superbeing. So, apparently Savage does not recognize behaviourism when he meets it. Jacob Bernoulli even wanted a lower limit for the "moral certainty" admissible in judicial decisions, e.g. 0,99 or 0,999, values which would be considered quite unacceptable today, to be fixed *by the law*.

Secondly, I stated that extremely small probabilities *cannot* be distinguished from each other (or from zero) because their computation always is based upon assumptions (like independence or constancy of probabilities) which are only approximately fulfilled. So for a correct description of actual human behaviour not only Christiaan Huygens' mathematical expectation $\Sigma p_i x_i$ of gains x_i with probabilities p_i is insufficient but also Daniel Bernoulli's "moral expectation $\Sigma\, p_i\, U(x_i)$, where the non-linear function $U(x)$ is the "utility" of a gain x. Instead, a function which is non-linear in the p_i either should be used, such that extremely small probabilities contribute zero, *however large* the corresponding gains may be.

Apart from the shortcomings in the inferential part, the staticity of the theory reveals itself in its not taking account of a person's changing his *valuations* (utilities), a point which I have stressed on another occasion [2]. "Learning by experience" consists largely of learning that the value I suppose *now* that *known* consequences of a *given* act under *given* (e.g. certain) conditions will have for me may prove afterwards to be quite different. A man may strive for a long time, to get a job in New York, because he *wants* to go to New York, and finally, after having succeeded,. find out that he does not like New York at all. In other words, not only the probabilities, but also the *utilities* change in time, although the author, except for a rather meaningless example, on p. 100, does not mention this at all. Apparently, his "rational person" is so far idealized that he never changes his tastes at all, and never is disappointed when he gets something he wanted.

It might be of some use to state here shortly my own views on the controversial points.

1. Statistical work has value only inasfar as its results are independent of the preferences of the individual statistician who performs it. Although such an independence in any absolute sense cannot be reached, it can be obtained to a practically sufficient degree, which is not essentially less than the one obtainable in other sciences.

2. Strictly speaking statistics needs as a mathematical tool no calculus of probabilities, but only a calculus of (finite) frequency quotients. The concepts of probability and of infinity are introduced for mathematical convenience only.

3. Statistics uses the empirical hypothesis that apparatus ("lotteries") exist, admitting random choices of one among any given number of elements. Such apparatus do not exist in absolute perfection and their degree of perfection can only be defined *after* development of their theory. Their rôle is analogous to that of rigid bodies in euclidean geometry and of perfect clocks in dynamics. Empirical interpretation of probability statements is only possible with reference to such random apparatus or to natural phenomena empirically found to behave statistically sufficiently like these.

4. Because of imperfection of random apparatus and of simplifying mathematical assumptions probability statements of very great precision have no empirical correlate. In particular the distinction between very small probabilities and zero has none. In accordance with this, actual human behaviour is only understandable on the assumption that possible events having theoretically extremely small probabilities are actually neglected.

5. Subjective expectations, valuations and preferences and their changes from person or in the course of time can and should be investigated by means of "objective" statistical methods. Trying to use them as a basis of statistics is like trying to gauge a fever thermometer by means of the patient's shivers.

8. Terminology

The author's terminology, inasmuch as it is personal, is rather objectionable. His substitution of "personalistic" for "subjective" is beside the point, because he deals with the assumed point of view of a highly idealized, that is impersonal "person". The expression "necessary views" is grammatically wrong: the term "necessary" should (if at all!) be applied to the attribution of definite probabilities to events; not to the *views* of these or other authors. The term "verbalistic" is applied by some modern economists to older schools of economics, using verbal (non-mathematical) reasoning only. The author uses this strongly deprecatory term for statistics dealing with assertions instead of acts. The term "regret", which some authors use for the *difference* between a loss and a minimum loss, is called by author "loss". We shall not follow him in this terminological innovation which almost certainly must lead to misunderstandings.

9. Examples

When considering the examples the author gives, one wonders at the cover (for which, of course, he is not responsible) stating that "he is also engaged in statistical consulting with research workers in a wide variety of fields". If this is true, could not he have found then in his experience some more interesting examples than the completely unrealistic, not to say silly, ones he uses in his book? Among nearly 200 titles the bibliography contains just one (!; W 11) dealing with a concrete statistical problem.

His principal examples concern: *a*) (p. 13–15) a person going to make an omelet and considering whether or not to break a doubtful egg into a bowl containing five good eggs; *b*) the number of "utiles" a person should pay for tasting a few grapes which have probabilities $\frac{1}{4}$, $\frac{1}{2}$, $\frac{1}{4}$ of being of poor, fair and excellent quality, in order to determine whether he shall buy 1, 2 or 3 pounds, without taking account of the cost of his computation, and of the printing. (This example takes $2\frac{1}{2}$ from $4\frac{1}{2}$ pages about "What an observation is". Notwithstanding the author's claim on p. 62 mentioned above, this chapter contains nothing which is pertinent to this important question); *c*) choices between gambles, e.g. for obtaining $ 2.500.000, $ 500.000 and zero with probabilities 0,1; 0,89 and 0,01 respectively!! (p. 101–103); *d*) a bet about coins with rules like "If the coin is a penny, he must pay a tax of 10; if it is a dime, he receives a bonus of 20. If he chooses to observe a coin, he must pay an inspection fee of 1, etc."; *e*) a common bet by Peter and Paul about the result of a future vote when, first case, it is Peter's unequivocal opinion that 55% (precisely?!!) of the electorate is for and 45% against the issue, and, second case, when Peter attaches probability $1—10^{-10}$ to this issue. In both cases the only alternative considered, of which Paul is convinced, is obtained by interchanging the two percentages (p. 170–177). The author apparently overlooks the fact that no probability of a phenomenon of this kind can be defined so precisely as to make a distinction between 10^{-10} and 0 meaningful. A phenomenon having probability 10^{-10} corresponds, if an experiment is made each second, with one success on the average in 317 years!

Such examples, sooner than pointing to a consulting statistician having a large and varied experience, rather suggest a philosopher, trying — and failing — to imagine what statistics actually might be. In the same direction points his remark (p. 5) that from Ch. 8 onward he passes to a "shallower level". Although this chapter is called "Statistics proper", I did not find anything in this chapter (nor in any other one) which would be of great help to a statistician struggling with difficulties regarding the foundations of statistics proper. The whole book remains on the too shallow level of so-called economic decision theory, without much insight in the possibility that it might be far from "rational" to base decisions on sham accuracy together with the illusion of "optimality", instead of recognizing that ultimately all accuracy is so restricted that what is optimal in a simplified mathematical model may be rather remote from optimality in reality, so that *restricting* loss may be a far better strategy than *minimizing* losses.

10. What do ordinary statisticians do?

The author does not state clearly that and how what he calls "verbalistic statistics" could easily be fitted within his scheme, viz. by admitting 1^0 that the risk of loss a statistician incurs may e.g. be a "loss of face", and thereby proportional to the ratio of the number of statements he makes (predictions, rejections of hypotheses, interval estimations, etc.) which are *disproved* by later observations, to their total number, and 2^0 that trying to *minimize* this ratio may (by unavoidable small disagreements between model and reality) easily lead to self-deceit and therefore should rather be replaced by *restricting* it, e.g. to a fixed level. And this is exactly what ordinary statisticians do.

11. What do "personalistic" statisticians do?

An inexperienced student, after having read Savage's book, might easily retain the impression that a statistician is someone, running around, buttonholing anyone who will listen to him and offering him dollars in order to learn his order of preferences, betting today for pennies and dimes, and to morrow for millions of dollars, computing carefully the cost of observing whether a coin is a penny or a dime, and forgetting to compute the cost of the computation, shortly, a gentle idiot, with a strong craze for betting, but without any of the characteristics which *actually* have led to the great and just fame of "British American statistics".

12. Minimax regret

Savage has replaced [1]) Wald's minimax loss method by the so-called minimax regret method (we do not follow his terminology, in which loss is called "negative income" and regret "loss"). He criticizes the former by an example where the number of events as well as that of strategies ("acts") is 2, and where the losses under the two strategies (f_1, f_2) in the first event

[1]) Savage does not claim the authorship for this replacement but, modestly, ascribes it to Wald. In the light of the following there may be some doubt whether Wald's friends will really consider this as an *honour* to his memory.

(B_1) are (1, 10) and in the other one (B_2) (1, —1). His objection is that the person has to choose f_1 irrespective of whichever free of cost (!) information he may have, however relevant to the events (apart, of course, though unmentioned, of certainty that the first event will occur). This he considers as absurd. In his example, however, the losses are obtained by subtracting the minimum of 1 and 10, i.e. 1, from the former, and the minimum of 1 and —1, i.e. —1, from the latter pair of values, so that the table of regrets becomes

$$\begin{matrix} 0 & 9 \\ 2 & 0 \end{matrix}$$

(rows representing events, columns acts). The minimax regret method therefore leads to choosing the first act, exactly as the minimax loss method did, so that Savage refutes, if any, his own method together with Wald's one.

From his "personalistic" point of view the author should have said that there *are* persons behaving in this way, viz. trying by all means to avoid a very large danger as soon as they are aware of its possibility. He might have called their behaviour "overcautious", but by no means — personalistically spoken — "absurd".

There is, however, a far more serious argument against his own method, namely that the minimum of the losses in an event may be a completely irrelevant quantity, because it may occur under acts which are not chosen anyhow. I illustrate this by an example, also with 3 states (rows), whereas 4 acts (columns) are available. The losses ("negative incomes") and regrets ("losses") are given by the following table, where a denotes a large number in comparison with 1.

		losses				minimum loss	regrets			
		strategies					strategies			
		f_1	f_2	f_3	f_4		f_1	f_2	f_3	f_4
	S_1	0	$-a$	$-2a$	-1	$-2a$	$2a$	a	0	$2a-1$
states	S_2	0	$-a$	a	$a-1$	$-a$	a	0	$2a$	$2a-1$
	S_3	0	$2a$	a	$2a-1$	0	0	$2a$	a	$2a-1$

According to the minimax regret method one should choose the fourth act f_4, because any other one has a possibility of a larger regret $2a > 2a - 1$. Comparing, however, in the table of losses f_4 with f_1, one wonders why one should be obliged by the minimum regret method to run the large risks $a - 1$ and even $2a - 1$, just for the possibility of a small gain 1 if the first state is realized. This means firstly attributing a very large importance to the small gain 1 in comparison with the large losses $\approx a$ and $\approx 2a$, and secondly speculating heavily upon the realization of S_1. If, however, S_1 actually occurs, f_4 is by far not the best strategy, but f_3 is!! Actually f_4 is the *worst* choice one can make, because it entails in *any* case almost the highest possible loss.

Now let us apply Savage's own arguments, and assume that the 3 states have definite probabilities p, q, r respectively $(p + q + r = 1)$. Then the expectations of the losses under $f_1, \ldots, f_4$ are:

$$0, \; (2r - p - q)\, a, \quad (q + r - 2p)\, a, \quad (q + 2r)\, a - 1.$$

When is f_4 the best choice? Evidently if $(q + 2r)\, a - 1$ is the smallest of the four expectations. This requires the three inequalities:

$$q + 2r < \frac{1}{a} \;,$$

$$2p + r < \frac{1}{a} \;,$$

$$2q + p < \frac{1}{a} \;.$$

The first requires, a being large, that $q + 2r$, hence q and r separately must be small, and the last one, that $2q + p$, hence also p must be small, contradicting $p + q + r = 1$. In fact, by adding corresponding members of the three inequalities we get as a necessary condition $3\,(p + q + r) < \frac{3}{a}$, i.e. $a < 1$, contradicting $a \gg 1$.

Hence, *whatever* are the probabilities of the three states, f_4 is *never* the most favourable choice.

The following example shows clearly that the minimax *regret* method makes the choice of a strategy dependent on wholly irrelevant circumstances.

		losses					minimum loss	regrets				
		strategies						strategies				
		f_1	f_2	f_3	f_4	f_5		f_1	f_2	f_3	f_4	f_5
states	S_1	a	0	$a-1$	-1	a^3	-1	$a+1$	1	a	0	a^3+1
	S_2	a	$2a$	$2a-1$	a^3-1	$a-1$	$a-1$	1	$a+1$	a	a^3-a	0

Looking at the losses and assuming $a \gg 1$, we see that the real issue is between f_1 (to play on safety) and f_2 (to plunge on S_1); f_3 is decidedly worse than both, because the small difference with f_2 in S_2 is no compensation for the large difference in S_1, and vice versa in comparison with f_1. This holds unless either S_1 or S_2 is very improbable [1]), but then f_5 or f_4 respectively are

[1]) If S_1 and S_2 have definite probabilities, f_3 is, according to the loss expectation, better than f_1 if and only if $P\{S_2\} < a^{-1}$, and better than f_2 if and only if $P\{S_2\} > 1 - a^{-1}$, hence, if $a > 2$, never better than both.

by far preferable to f_3. Unless there is almost certainty of either S_1 or S_2, in which case there is no real decision problem, f_4 and f_5 are completely out of the question because of the enormous risks involved in the terms a^3.

The minimax regret principle, if accepted, would force us to choose f_3, which under no circumstances whatsoever is the best strategy, and this choice depends on the presence of the strategies f_4, f_5 which, unless one of the states is almost certain, will not be chosen anyhow. For, let us change in the f_4 and f_5 columns -1 and $a-1$ into $+1$ and $a-2$ respectively, then the minimax regret principle would lead to f_1, and if we change them into -2 and $a+1$ respectively it would lead to f_2.

Comparing the latter two games only, viz.:

a	0	$a-1$	1	a^3	with	a	0	$a-1$	-2	a^3
a	$2a$	$2a-1$	a^3	$a+2$		a	$2a$	$2a-1$	a^3-1	$a+1$

leading to the regret tables

a	0	$a-1$	1	a^3	$a+2$	2	$a+1$	0	a^3+2
2	$a+2$	$a+1$	a^3-a+1	0	0	a	$a-1$	a^3-a-1	1

we see that the minimax regret principle would force us to choose f_1 in the former, f_2 in the latter case.

Hence, leaving out of consideration the cases where either S_1 or S_2 is practically certain, so that there is no real decision problem, we see that the only relevant decision problem, viz. the choice between f_1 and f_2, i.e. whether to choose the safe way f_1 (minimax loss method) or (f_2) to gamble on S_1 for the amount a, is made dependent on the completely irrelevant small changes in the consequences of f_4- and f_5-strategies, which are not chosen anyhow.

As a last example I might mention a case where the minimax regret method has actually been considered during some time in an econometric decision

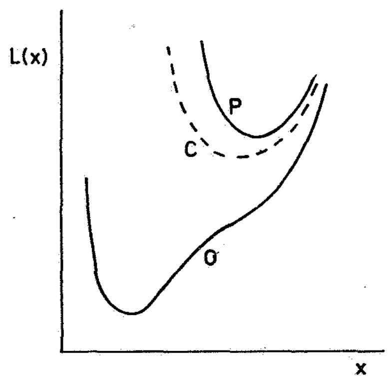

Fig. 1. Extreme estimates of the loss function

problem of great importance. Ultimately the method has been abandoned on the grounds to be mentioned.

The loss function $L(x)$ depends upon the abscissa x, which can be chosen freely, and upon a number of badly known parameters. In fig. 1 the curves O and P represent the loss functions under the most Optimistic and the most Pessimistic estimates of all parameters concerned. By choosing intermediate estimates in different ways curves like C are obtained.

We consider the two extreme estimates O en P only and drop the intermediate ones. These are therefore the possible "states of nature". Nothing being known about the relative trustworthiness of the two estimates, the Bayes-Laplace method would attribute probabilities $\frac{1}{2}$ to each of them, leading to a curve not differing very much from C. The minimax loss method leads uniquely to the abscissa $b = x_B$ of the minimum B of curve P (cf. fig. 2).

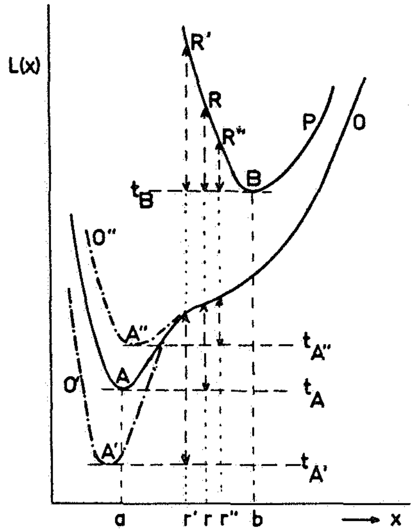

Fig. 2. Influence of irrelevant changes of the loss function on a minimax regret solution [1])

According to the minimax regret method we have to draw the tangents t_A and t_B in the minima A and B of the curves O and P respectively, and to find an abscissa $r = x_R$ such that the height above t_A (= regret) of O equals that of P above t_B, as for any other choice of x the regret either in state O or in state P is increased.

Now we remark 1^0 that intermediate curves like C in fig. 1 reduce the minimax loss only slightly, 2^0 that a choice of x near $a = x_A$ is quite out of the question, because the great steepness of the lefthand part of curve

[1]) The letter O should also be inserted near the left hand end of the lower *full drawn* curve.

P would lead to enormous losses if the gamble on "close to O" would ultimately appear to have been wrong.

Finally let us assume that the small-x-part of curve O can be reduced to O', or has to increase to O'' without any change in the large-x-part. Then $r = x_R$ would no longer be the minimax regret solution, but would have to be replaced by $r' = x_{R'}$ or $r'' = x_{R''}$ respectively. Now, whereas one may ask from the beginning why one should incur a loss as high as $R = L\ (r)$ if the unfavourable state P might obtain, if one need not risk more than $B = L\ (b)$, it is quite unreasonable, to increase it to $R' = L(r')$ because in the most favourable case O, a solution a which anyhow is not chosen because it is too risky, would be cheaper, and similarly for R''.

This, I repeat, is not a fictitious example, but one where actually large risks were on stake.

Examples of a similar type as the first ones given above have been given before, e.g. by H. Chernoff, and are mentioned by Savage. As a variant of his own Savage adds wittily (p. 206): "Fancy saying to the butcher: "Seeing that you have geese, I'll take a duck instead of a chicken or a ham." " Erroneously, however, he attributes the absurdity to the "objective" interpretation instead of to the use of regrets instead of losses, and seems to believe that the method would become reasonable if group decisions instead of individual decisions are concerned: "It would not be strange, for example, if a banquet committee about to agree to buy chicken should, on being informed that goose is also available, finally compromise on duck". It would not be strange, indeed, but Savage's question should have been whether it were "rational". And this would be hard to maintain.

These examples, I believe, are not only *objections* against the replacement of loss by regret, but are actually *fatal* to it and refute it finally. The method may lead, on completely irrelevant grounds, to unnecessarily large risks, and to strategies which under no circumstances are optimal, or nearly optimal, but which are under whichever circumstance almost as bad as the worst one.

I might add that, unlike loss, regret has no direct economical meaning. A loss is experienced; a "regret" is a relation between losses suffered and those which *would have been* suffered if acts *were* performed which are not. "Regret" may be considered as a *part* of the loss we suffer; *regret is the price we have to pay for lacking knowledge.*

Literature:

[1] Leonard J. Savage, The foundation of statistics, Wiley & Sons Inc. New York, Chapman & Hall Ltd London, 1954.

[2] D. van Dantzig, Utilité d'une distribution de probabilité, ou distribution des probabilités d'une utilité? Colloque d'Econométrie, Paris 1950.