Comments on Causal Decision Theory

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1. Introduction

Two of the symposiasts here today acknowledge a specific and deep rooted problem in normative expected utility theory. They concur, the deficiency is exposed in decisions which emphasize the difference between probabilistic dependence and causal dependence across act-state pairs. Despite this shared agreement, my co-commentator and the principal speaker offer different prognoses for expected utility theory. They agree on the symptoms but not on the cure.

Professor Harper and his co-worker Gibbard despair of saving expected utility. With commendable forthrightness, they give us their newly designed (1978) causal decision theory to replace a moribund expected utility. With the fine skills of genetic engineers they have created causal decision theory by introducing causal conditionals into the algebra of probability, just as mutant virus proteins have been implanted into host DNA.

By contrast, Prof. Eells refuses to sign a postdated death certificate identifying Newcomb et al. as executioners. Though he agrees with the advocates of causal decision theory that standard accounts of expected utility deliver the wrong choices in the test-problems, he holds out for a complicated corrective planned to rejuvinate the old program so that it falls in line with the recommendations of the new "causal" approach. Eells prescribes a regimen of tickles and metatickles to shock expected utility into causal consciousness (1982, 1985).

I must beg your indulgence to complicate the debate with a third opinion. Though I agree expected utility theory is normatively deficient, the family of decision problems related to Newcomb's indicate no new flaw, no new shortcoming which I see. Expected utility can be challenged, successfully, for its conflation of risk with uncertainty--as in Ellsberg's (1961) work. Expected utility fails to admit a concern with "risk" beyond that captured in the curvature of a utility function--as Allais noted some thirty years ago (1953). And,

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because of the norms involving "closure", expected utility theory is hard put to extend the concept of "evidence" to include the results of (mere) calculation--so that there is precious little to be found about how subjective probability affords a sense of "evidence" in mathematics or logic. [See (Glymour 1980, chap. 3) for discussion of how this problem affects "confirmation" in science. Of course, the issue is not news. See (Savage 1967) and (Hacking 1967), for instance.] But I do not understand the alleged shortcomings exposed in the case of Newcomb. I do not see where expected utility is at fault because of confusions over causal independence. Where are the dire consequences stemming from a straightforward use of conditional expected utility theory? To be blunt, what's the beef?

2. Variations and Special Cases of Newcomb's Problem

In defense of expected utility, let me rehearse some small variations on the Newcomb theme. We begin with the canonical Newcomb problem. There is a choice between two options: $option_1$-- to take the contents of an opaque box (one-box); and $option_2$--to take the contents of the opaque box plus the $1,000 in the transparent box (two-boxes). The opaque box contains either $1,000,000 or nothing, depending upon a demon's earlier prediction that the choice is for one-box or for two-boxes. The familiar decision-matrix summarizes the choice problem and reveals a <u>dominance</u> in dollar payoffs of $option_2$ over $option_1$.

	$event_1$: demon predicts $option_1$ chosen	$event_2$: demon predicts $option_2$ chosen
$option_1$: take one-box	$1,000,000.	$0.
$option_2$: take two-boxes.	$1,001,000.	$1,000.

We assume the agent who is to choose in the Newcomb problem takes no interest in the consequences of the choice apart from the monetary outcomes of the matrix, where more is better. That is, the agent has no outside interest in, say, frustrating the demon's prediction apart from the preference for an additional $1,000 in case the demon predicts a choice of $option_1$. For simplicity, let us take a utility function linear in dollars, so that the dollar figures also are utiles. Hence, the dominance of $option_2$ over $option_1$ is truly a dominance by preference, not merely a dominance by dollar amounts. The twist, of course, is to add a constraint that the chooser admits the demon is a very accurate predictor of such choices--where the accuracy is interpreted as a high conditional probability given the agent's choice: $\text{prob.}(event_1 \mid option_1) \simeq \text{prob.}(event_2 \mid option_2) \simeq 1-\varepsilon$. [See (Levi 1975) for an explanation of why this interpretation is necessary for preserving a conflict between expected utility and dominance considerations.] Then, the conditional expected utility of $option_1$ exceeds that of $option_2$--against the recommendation afforded by appeal to dominance.

Causal decision theorists (and Prof. Eells, too) think(s) two-boxes are better than one, contrary to this analysis by conditional expected utility. [As I understand Eells, he hopes for added information that overrides the evaluation based on (just) the evidence of which option is chosen.] In one way or another causal decision-theorists argue that, since what is in the opaque box is "fixed and determined" when the agent chooses, attention to the causal efficacy of the choice makes the dominating option right and the one-box option wrong.

Let us trace out the effects of preferring the two-box option in cases of the Newcomb problem. First, consider an infallible demon. [This issue was raised by Levi (1975). I repeat it as I know of no reply from the causal decision theory camp.] Then, by the criteria of Savage's (1954) or Jeffrey's (1965) versions of expected utility theory, the problem is one of choice under certainty--there is no risk involved. Taking option_1 and getting \$0 , or taking option_2 and getting \$1,001,000 constitutes a null-event. It is certain that the demon predicts correctly; hence, the decision matrix is

	<u>demon predicts correctly</u>
option_1	\$1,000,000.
option_2	\$1,000.

where other outcomes are null. The causal relationships between the choice and the "fixed and determined" state of the opaque box are unchanged by this special-case version of Newcomb. Do the causal decision theorists advocate option_2 when it is certain to yield \$1,000 whereas option_1 is certain to yield \$1,000,000? To recommend two-boxes in this case is to contravene more than subjective expected utility theory--it is to overturn what is non-controversial by all standard accounts of choice, from expected utility to minimax theory to the very principle of dominance so dear to the hearts of causal decision theorists. Here option_1 dominates![1]

As a second case, consider the version of Newcomb with an infallible demon where a friend, not you, faces the choice but where you and your friend agree on the formulation of the decision problem. How do you evaluate the worth of your friend's act on the hypothesis it is made but before the opaque box is opened and its contents awarded? What are your betting odds that conditional on option_1 your friend becomes a millionaire? What are your betting odds that conditional on option_2 your friend receives only \$1,000? By the weakest of principles of conditionalization, where a conditional gamble is equated to a called-off bet (see (Shimony 1955) and for more recent discussion (Levi 1981)), these are sure-things; hence, you are prepared to offer arbitrarily high odds on their occurring. Last, what is your fair-market price for your friend's choice? That is, given which option it is that your friend chooses, what is the fair market value for the proceeds it brings? Given your friend chooses one-box, is there any reason to pass up the bargain to buy the proceeds for, say, \$10,000? Given your friend chooses two-boxes, is there any reason to offer more than \$1,000 to get the rewards that await? I see none.

These questions and answers erode the causal-decision-theoretic arguments pointing towards the two-box solution when the demon is infallible (either in first- or in third-person version). Likewise, they stymie Eells' strategy for underwriting the two-box option. No matter what sort of additional information Eells tries to capture under the headings of "tickles" or "metatickles", so long as we are not accepting what is practically impossible, the conditional probability of the demon predicting accurately remains 1. That is, for an infallible demon, given any non-null evidence, the problem remains one of choice-under-certainty. What is the point to "tickles" and "metatickles" here?

Perhaps the causal decision theorists, and Prof. Eells too, treat the infallible demon as an exception to the rule? I don't find good reason to switch principles of rationality when ε changes from a minute quantity to zero. Nor am I impressed by the argument that the case of the infallible demon is one where the agent lacks a free choice. I am sufficiently a "compatibilist" not to fear for my freedom even when my choices are perfectly predictable--in the sense of a predictor like Newcomb's demon. (Helpful here is Dennett's recent discussion (1984, chapter 5).) I think the arguments that tell against the "two-box" solution with the infallible demon also have bite when the demon is fallible.

Thus, it seems important to bother with the case where the demon is good, but not infallible. Suppose, prob.(demon predict option$_i$ | option$_i$) = $1-\varepsilon < 1$ $(i=1,2)$. The convenience of making these conditional probabilities equal is that then there is a simple Savage-styled formulation of the decision problem, where states are probabilistically act-independent (though, I gather, not causally act-independent in the sense required by causal-decision theory). The decision problem admits a Savage-styled matrix:

	demon predicts correctly	demon predicts incorrectly
option$_1$	\$1,000,000.	\$0.
option$_2$	\$1,000.	\$1,001,000.

where the veracity of the demon's prediction affords a binary partition of "states".

Let us inquire about the third-person version of this case of Newcomb's problem. Again, how do you evaluate the worth of a friend's choice on the hypothesis of which option he chooses but before the contents of the opaque box are revealed? What are your betting odds that conditional upon option$_1$ your friend becomes a millionaire? What are your betting odds that conditional upon option$_2$ your friend receives no more than \$1,000? Once again, by the weakest of principles of conditionalization, your conditional betting odds are $1-\varepsilon : \varepsilon$. What is your fair-market price for your friend's choice? Is there any reason to pass up the opportunity to own the proceeds of your friend's choice if offered at less than the expected value according to these conditional odds?

The conditional probabilities fix conditional betting rates on the payoffs to your friend. But they also guide your actions in the simple marketplace where options on Newcomb choices are bought and sold. Do the causal decision theorists recommend your friend choose option_2 and sell it at its fair-market value of about $1,000 when, had your friend chosen option_1 his fortune would be worth, by the market value, nearly that of a millionaire? Or are the causal decision theorists prepared to deny that betting rates on your friend's prospects are fixed by the conditional probabilities which are part of the constraints of the problem? If the conditional probabilities do not fix these betting rates, what betting rates do they fix? What is the content of the assumption that the prob.(demon predicts $\text{option}_i \mid \text{option}_i$) = $1-\varepsilon$ (i=1,2) if not to fix the conditional odds that your friend gets rich?

The same issues arise with a first-person version of Newcomb's problem for the fallible demon. I shall not bother to repeat for the third time each of the questions about conditional betting odds and about the fair-market value of your choice. It suffices to say I see no difference here between the third- and first-person versions.

3. Metatickles and Computation as Evidence

How does Prof. Eells propose to get a recommendation for "two-boxes" according to the dictates of expected utility? His plan develops a suggestion due to Nozick (1969, fn. 11, pp. 144-145). Perhaps the agent who is to make the choice can recognize extra, relevant information that makes the "fixed and determined" states conditionally act independent given the new evidence. Suppose the agent perceives that T obtains and that

$$\text{prob.(opaque box contains \$0} \mid \text{T \& option}_i) \quad (i=1,2)$$

is constant. Then, by the total evidence principle and conditionalization, if the agent knows T obtains the decision problem is not controversial: dominance is valid under expected utility--"two-boxes" are better than "one box".

Where shall the agent look for the elusive T-state which is to resolve the controversy? That depends upon what tickles your fancy. In some of the Newcomb-type problems, the agent is supposed to recognize a "common cause" of both the choice and the pertinent state. For instance were one's predilection to smoke due to a sensible "tickle" in the esophagus, which "tickle" is the by-product of the same hypothetical <u>genetic</u> cause for lung-cancer, then feeling the "tickle" brings the bad news about one's genetic make-up but also makes the event of getting lung-cancer probabilistically independent of whether or not one smokes. (See (Levi 1985) for a serious discussion of this stilted example.)

3.1. Aside on the Smoking/Cancer example

I find objectionable the use of R. A. Fisher's name in connection with this variant of Newcomb. It misrepresents the points Fisher argued for in the smoking/cancer controversy. He was concerned with the statistical interpretation of the Doll & Hill observational studies. Specifically, Fisher argued they were insufficient data to conclude smoking causes lung cancer (1959, pp. 7-10). Basing public policy on the unwarranted causal claim struck him as an abuse of statistics.

It should be noted Fisher bothered to include an essay on the meaning of "probability" in his short monograph on the smoking controversy (1959, essay 3). My understanding is that Fisher objected to the Doll & Hill studies also because they failed to provide the right sort of data to support a probability relation between smoking and lung cancer, quite apart from the more difficult matter of establishing a causal relation between these. More technically, the question Fisher raises is whether, because the data were collected in an observational study using only minimal controls, i.e., the study lacked important controls through blocking, do the data satisfy the requirement that "no relevant sub-set can be recognized" (1959, p. 28)? To the extent that the broad categories of non-smokers, cigarette smokers, pipe smokers, cigar smokers, etc. are genetically different, the Doll and Hill data fail to satisfy the requirement of being an epistemically homogenous reference class. The relative frequencies across these classes do <u>not</u> support inter-class comparisons of how probable it is <u>you</u> will have lung cancer given you smoke a cigarette rather than a cigar since <u>you</u> cannot have both of the distinct genetic types posited of these different classes. See Fisher's remarks in (1959, pp. 22-23).

Thus, I find it a burlesque of Fisher's analysis to use his conjecture about a common cause for smoking and lung cancer as a reason for a Newcomb-like presentation of the controversy. To repeat: Fisher was concerned with an abuse of statistics to arrive at a public policy on smoking in light of an unwarranted causal claim. If his words carry any import for individual decisions about smoking, it is that the Hill and Doll data also are inadequate for grounding probability relations connecting smoking with lung cancer.

3.2. A Problem Created When Computations Are Evidence

Eells agrees that such sensible "tickles" may be absent (1985, p.180). The bulk of his essay presented in this symposium deals with ideas for finding metatickles that, formally, play the same role as tickles. They are called "metatickles", I surmise, because they are reports about how the agent evaluates the (first-order) evidence available.

Perhaps I am handicapped, for I lack a meta-funny bone and can't grasp the sense of a metatickle. It is not just the complexity of Newcomb problems that perplexes me when I am asked to use as evidence my introspective account of how I evaluate options. I don't understand how that is to be done in the most mundane decision problems.

The problem I have with metatickles is a special case of the general problem of how to use the output of a mere calculation as new evidence. Under the normative constraints of expected utility, an agent carries a coherent set of probabilities and utilities which fix preference (and choice) by expected utility, i.e., preference and choice are in accord with the ranking by expected utility. But the business of calculating an expected utility, given the agent's probability and utility functions, is no more newsworthy to the agent than is, say, the calculation of a conditional probability given the two unconditional probabilities of which it is the ratio.

Of course, this analysis rests upon a non-occurrent sense of belief. See (Levi and Morgenbesser 1964, p. 221, fns. 3 and 4). On an occurrent sense of belief, I expand by beliefs when, after a calculation, I see that the decimal expansion of $\sqrt{2}$ is 1.4142... in place of 1.414.... . But if the output of the calculation is to serve as new evidence, in the sense of evidence appropriate to conditionalization, I must have some non-trivial personal probability distribution for the fifth digit in the decimal expansion of $\sqrt{2}$.

The basic problem in an approach which tries to use this calculation like an experiment, to produce new evidence, is that it robs the criterion of coherence (de Finetti 1970) of its normative force. The Dutch Book argument, associated with incoherence, uses an algebra of possibility in which both mathematical and logical consequences are respected. Any personal probability that assigns other than probability 1 to the "event": fifth digit in the decimal expansion of $\sqrt{2}$ is 2, is incoherent. This is appropriate for a normative theory of rational degrees of non-occurrent beliefs. The norms of subjective expected utility, like the norms imposed by elementary logic on rational belief, are not directed at the body of occurrent beliefs. For example, the requirement that one's knowledge be deductively closed makes little sense for occurrent beliefs.

Let me clarify the objection. I am not suggesting there is some conceptual difficulty incorporating, say, the cost associated with a calculation in the evaluation of an experimental design. (Though one must not ignore the obvious potential regress in worrying about the cost of estimating the cost-effectiveness of particular computational procedures.) What I do find too vaguely described to understand is the proposal that one may take the output of mathematical calculation as new evidence in the same sense in which new observations are taken as evidence for Bayesian conditionalization. No doubt this is a hard problem. Until it is addressed I am at a loss to comprehend what evidence is added by a metatickle that reports the computations in deliberation (in calculating an expected utility).

That is, the investigation into the "pre-choice dynamics of evidential deliberation" (Eells 1985, p. 185) jumps the gun on what is, at least, an open controversy in the debate over what may count as evidence for Bayesian conditionalization. Hence, I reject the assertion, "We have just seen that the results of calculating the evidential expectations of the available acts is [sic] itself evidence (a metatickle) that can sometimes require reassessments of the evidential relevance of the acts to the outcomes." (Eells 1985, p. 191).

Let me take a moment to emphasize the complexity of the challenge facing those planning to extend the concept of "evidence" to include computational outcomes. Suppose I plead ignorance of the decimal expansion of $\sqrt{2}$ beyond 1.414... . Can I treat $\sqrt{2}$ as some unknown constant in the interval [1.414, 1.415)? Suppose I hold some non-trivial distribution for $\sqrt{2}$ whose support is the set of irrational numbers in this narrow interval. [I know $\sqrt{2}$ is irrational by a familiar, indirect argument which fails to specify its fifth decimal digit. See, e.g., (Hardy 1967, p. 6).]

Now, it is a simple task to specify a problem in probability theory, which, by elementary geometry, is solved with the solution that the probability for the event in question is $\sqrt{2}/2$. That is, without knowing the decimal expansion of $\sqrt{2}$, I can show that my personal probability for a specific event is $\sqrt{2}/2$. What does this mean if I am to treat $\sqrt{2}$ as an unknown constant with a distribution supported by irrationals in $[1.414, 1.415)$? It won't do to "solve" for my personal probability by using the distribution for $\sqrt{2}$ over the interval $[1.414, 1.415)$ and "integrating out" the unknown quantity. That doesn't work since my expectation for $\sqrt{2}$ may be a rational number (as when I assign $\sqrt{2}$ a "uniform" distribution over irrationals in $[1.414, 1.415)$), which makes my "estimate" of $\sqrt{2}/2$ a rational quantity as well--contrary to my knowledge that $\sqrt{2}/2$ is irrational. The point is that, since I can identify my personal probability by terms which, in an <u>occurent</u> sense of belief, fail to identify the decimal representation of that probability, it appears that the consequence of treating computations as evidence is the conclusion that my (occurent) personal probability may be indeterminate--in conflict with the canons of subjectivism. If my personal probability is indeterminate, then I cannot compare events to decide which is subjectively more probable. Trying to capture this indeterminacy in a "second-order" distribution leads us to the contradiction above.

Perhaps, before rushing to assume there is a way to use the computations of expected utility themselves as evidence in Newcomb-problems, one should heed Savage's words on the dangers lurking that way. "Is it possible to improve the theory in this respect, making allowances within it for the cost of thinking, or would that entail paradox, as I am inclined to believe but unable to demonstrate?" (1967, p. 308)

4. Metatickles, Self-Knowledge, and a Problem of Free Will

The plan to use information about one's own deliberations as metatickles raises the spectre of an ancient problem about free will. Can choice be reconciled with thorough <u>self</u>-knowledge about how one deliberates? How much self-knowledge is compatible with free-will? Frederic Schick (1979) gives a careful accounting why contemporary decision theorists, ranging from the economist G. L. S. Shackle to Richard Jeffrey, have been spooked by the question. Also, see D. Dennett's discussion (1984, p. 112).

With Prof. Eells prepared to use Jeffrey's (1965) account of expected utility, it is appropriate to rehearse the dilemma as it arises within Jeffrey's theory. [See Jeffrey (1977) and Schick (1979). Jeffrey (1965, § 11.9) discusses a related question.] Agent <u>A</u> faces a choice among options he ranks in accord with the theory of <u>desirability</u>. The agent <u>A</u> believes his choice is effective. That is, <u>A</u> has the ability to make-true that which he chooses and he knows this. However, if <u>A</u> knows he will choose the most <u>desirable</u> option available, then (as it is a mere calculation away which option is most desirable) this option carries a personal probability of 1 (to <u>A</u>) of occurring. By Jeffrey's theory, this best option must be ranked (by _A_) as indifferent with any tautological proposition. If options are exclusive, the other options must be judged "null", i.e., _A_ assigns them probability 0 and they are identified with a contradictory proposition. In short, knowing what he will choose and knowing his choices are effective robs _A_ of his decision.

A variant of this dilemma troubles P. C. Fishburn in his review of normative theories of subjective expected utility (1981, p. 187). If o_1 and o_2 are distinct options with o_1 strictly more desirable than o_2, then the proposition corresponding to the disjunction "o_1 or o_2" must be ranked between o_1 and o_2. However, argues Fishburn, if one can make o_1 happen, then why wouldn't one rank "o_1 or o_2" indifferent with o_1 as one knows o_1 will be chosen from the two, i.e., prob.(o_1 | "o_1 or o_2")=1. Fishburn (1981, p. 194) suggests the fault is due to the unified domain in Jeffrey's theory: both "states" and "acts" are assigned subjective probabilities. An alternative to Fishburn's reason is to blame the problem on the dilemma about self-knowledge discussed above.

I do not know of an easy way out of this dilemma. Denying effectiveness of choices strikes me as only sidestepping the problem. It requires the _ad hockery_ also of denying propositional status to the modified option, "trying to do ...". Jeffrey (1965, p. 167) suggests this solution then withdraws it (1977). I understand Schick to opt for some introspective opacity. Agent _A_ doesn't know enough about himself to get into trouble. Levi (198x, chap. 4) proposes that while deliberating agent _A_ give up the claim he knows he maximizes expected utility.

The point of my worry is that there will be no way to incorporate the self-knowledge metatickles are intended to provide without overstepping the bounds of self-ignorance necessary for avoiding this puzzle about free will. How does Prof. Eells solve this free will problem? How does he guard against decision-theoretic paralysis from an information overdose with metatickles of dubious therapeutic value?

Notes

[1] Since the event, "demon predicts correctly", is (by assumption) certain to occur, we have that preference given this event coincides with unconditional preference (Savage 1954, theorem 2.1.4). Then, if causal decision theory is to recommend "two-boxes" when the demon is infallible and if Savage's axioms are "ambiguous" (Gibbard 1984) between the two rival interpretations, it follows that on a causal-decision-theoretic reading of this version of Newcomb's problem the proposition that the demon predicts correctly fails to denote an event governed by Savage's postulates P1 and P2. In other words, on pain of contradicting theorem 2.1.4, how can a causal-decision-theoretic reading of Savage's postulates recommend "two-boxes" yet admit the assumption that the demon is infallible? Does the sense of subjective probability in the claim that the demon is infallible escape reduction to Savage's postulates when these are given a causal-decision-theoretic interpretation? If so have we not

found grounds for disputing the thesis that the axioms are "ambiguous" between the rival interpretations? For, on the interpretation defended in this commentary there is no parallel restriction on theorem 2.1.4, i.e., supersets of a "sure event" are "sure events"--personal probability may be completed (as a measure).

References

Allais, M. (1953). "Le comportement de l'homme rationnel devant le risque: Critique des postulats et axioms de l'école Americaine." Econometrica 21: 503-546.

de Finetti, B. (1970). Teoria della probabilità. Volume 1. Torino: G. Einaudi. (As reprinted as Theory of Probability. Volume 1. (trans.) A. Machi and A. Smith. London: John Wiley & Sons, 1974.)

Dennett, D. (1984). Elbow Room: The Varieties of Free Will Worth Wanting. Cambridge, Massachusetts: The MIT Press.

Eells, E. (1982). Rational Decision and Causality. Cambridge: Cambridge University Press.

--------. (1985). "Causal Decision Theory." In PSA 1984. Volume 2. Edited by P.D. Asquith and P. Kitcher. East Lansing: Philosophy of Science Association. Pages 177-200.

Ellsberg, D. (1961). "Risk, Ambiguity and the Savage Axioms." The Quarterly Journal of Economics 75: 643-669.

Fishburn, P.C. (1981). "Subjective Expected Utility: A Review of Normative Theories." Theory and Decision 13: 139-199.

Fisher, R.A. (1959). Smoking: The Cancer Controversy. London: Oliver and Boyd.

Gibbard, A. and Harper, W. (1978). "Counterfactuals and Two Kinds of Expected Utility." In Foundations and Applications of Decision Theory. Volume 1. (University of Western Ontario Series in the Philosophy of Science. Volume 13.) Edited by C.A. Hooker, J. Leach, and E. McLennen. Dordrecht: Reidel. Pages 125-162.

----------. (1984). Decision Matrices and Instrumental Expected Utility." Unpublished manuscript. University of Michigan.

Glymour, C. (1980). Theory and Evidence. Princeton: Princeton University Press.

Hacking, I. (1967). "Slightly More Realistic Personal Probability." Philosophy of Science 34: 311-332.

Hardy, G.H. (1967). A Course of Pure Mathematics. 10th ed. Cambridge: Cambridge University Press.

Jeffrey, R. (1965). The Logic of Decision. New York: McGraw Hill.

----------. (1977). "A Note on the Kinematics of Preference." Erkenntnis 11: 135-141.

Levi, I. and Morgenbesser, S. (1964). "Belief and Disposition." American Philosophical Quarterly 1: 221-232.

-------. (1975). "Newcomb's Many Problems." Theory and Decision 6: 161-175.

-------. (1981). "Direct Inference and Confirmational Conditionalization." Philosophy of Science 48: 532-552.

-------. (1985). "Common Causes, Smoking and Lung Cancer." In Paradoxes of Rationality and Cooperation: Prisoner's Dilemma and Newcomb's Problem. Edited by R. Campbell and L. Sowden. Vancouver: University of British Columbia Press. Pages 234-247.

-------. (198x). Moral Struggle. Unpublished manuscript.

Nozick, R. (1969). "Newcomb's Problem and Two Principles of Choice." In Essays in Honor of Carl G. Hempel. Edited by Rescher et al. Dordrecht: Reidel. Pages 114-146.

Savage, L.J. (1954). The Foundations of Statistics. New York: Wiley.

-----------. (1967). "Difficulties in the Theory of Personal Probability." Philosophy of Science 34: 305-310.

Schick, F. (1979). "Self-Knowledge, Uncertainty, and Choice." The British Journal for the Philosophy of Science 30: 235-252.

Shimony, A. (1955). "Coherence and the Axioms of Confirmation." Journal of Symbolic Logic 20: 1-28.

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References

Le Comportement de l'Homme Rationnel devant le Risque: Critique des Postulats et Axiomes de l'Ecole Americaine

M. Allais

Econometrica, Vol. 21, No. 4. (Oct., 1953), pp. 503-546.

Stable URL:

http://links.jstor.org/sici?sici=0012-9682%28195310%2921%3A4%3C503%3ALCDLRD%3E2.0.CO%3B2-Z

Risk, Ambiguity, and the Savage Axioms

Daniel Ellsberg

The Quarterly Journal of Economics, Vol. 75, No. 4. (Nov., 1961), pp. 643-669.

Stable URL:

http://links.jstor.org/sici?sici=0033-5533%28196111%2975%3A4%3C643%3ARAATSA%3E2.0.CO%3B2-E

Slightly More Realistic Personal Probability

Ian Hacking

Philosophy of Science, Vol. 34, No. 4. (Dec., 1967), pp. 311-325.

Stable URL:

http://links.jstor.org/sici?sici=0031-8248%28196712%2934%3A4%3C311%3ASMRPP%3E2.0.CO%3B2-B

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Isaac Levi
Philosophy of Science, Vol. 48, No. 4. (Dec., 1981), pp. 532-552.
Stable URL:
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Leonard J. Savage
Philosophy of Science, Vol. 34, No. 4. (Dec., 1967), pp. 305-310.
Stable URL:
http://links.jstor.org/sici?sici=0031-8248%28196712%2934%3A4%3C305%3ADITTOP%3E2.0.CO%3B2-F

Self-Knowledge, Uncertainty, and Choice
Frederic Schick
The British Journal for the Philosophy of Science, Vol. 30, No. 3. (Sep., 1979), pp. 235-252.
Stable URL:
http://links.jstor.org/sici?sici=0007-0882%28197909%2930%3A3%3C235%3ASUAC%3E2.0.CO%3B2-Q

Coherence and the Axioms of Confirmation
Abner Shimony
The Journal of Symbolic Logic, Vol. 20, No. 1. (Mar., 1955), pp. 1-28.
Stable URL:
http://links.jstor.org/sici?sici=0022-4812%28195503%2920%3A1%3C1%3ACATAOC%3E2.0.CO%3B2-T