Perception & Psychophysics
1978, Vol. 23 (6), 542-545

Notes and Comment

Kubovy on "A possible basis for conservatism in signal detection and probabilistic categorization tasks": Some comments

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Kubovy (1977) argues that subjects fail to maximize expected payoff in detection tasks because they misrepresent the posterior probabilities. However, the paper contains some mistakes and omissions.

Here is the background. Stimuli s_1, s_2 are presented with probability $(1 - \gamma),\gamma$; the subject responds r_1 or r_2; event s_ir_j implies monetary outcome o_{ij}. The s_i are represented internally by distributions $f_i(x)$, and the subject is supposed to choose a criterion, c, on X and respond r_1, r_2 as $x < c$ or $x > c$. The likelihood ratio is given by $l(x) = f_2(x)/f_1(x)$. $\beta = l(c)$ is the likelihood ratio at criterion. β^* is the likelihood ratio at c^*, the value of c which maximizes expected payoff. By a well-known argument

$$\beta^* = l(c^*) = [(1-\gamma)/\gamma][(o_{11}-o_{12})/(o_{22}-o_{21})]. \quad (1)$$

"Conservative" criterion placement is $\beta \lesssim \beta^*$ as $\beta^* \gtrless 1$.

(1) Kubovy writes: "Consider an observer who is ideal in all respects but one: his or her knowledge of the distributions is deficient. Suppose that such a deficient observer were placed in a signal detection situation with an a priori probability of signal plus noise of 0.25, and a symmetric payoff matrix. Since the calculation of β^* does not require any knowledge of the distributions, our deficient observer will do so correctly, choosing a criterion $\beta^* = 3$. Because the posterior probability of observation x is equal to $l(x)/[1 + l(x)]$, where $l(x)$ is the likelihood ratio of x, the criterion $\beta^* = 3$ corresponds to a posterior probability of 0.75."

This is clearly wrong. By Bayes' theorem the posterior probability of s_2 conditional on x is given by

$$\pi(x) = \gamma f_2(x)/[\gamma f_2(x) + (1-\gamma)f_1(x)]$$

$$= \{[\gamma/(1-\gamma)]l(x)\}/\{[\gamma/(1-\gamma)]l(x)+1\}. \quad (2)$$

$\pi(x) = l(x)/[l(x) + 1]$ only if $\gamma = 0.5$, which case Kubovy is explicitly *not* considering. In fact, for any symmetric payoff scheme $[(o_{11} - o_{12}) = (o_{22} - o_{21})]$ $\pi(c^*) = 0.5$.

Thus, Kubovy's Figure 1 is mislabeled, at least as far as the $\gamma \neq 0.5$ conditions are concerned. The figure, in fact, plots $l(c^*)/[1 + l(c^*)]$ against $l(c)/[1 + l(c)]$.

(2) Kubovy writes of the Green and Swets (1966, p. 90) data, in which an isosensitivity curve was traced out (1) by varying payoff, $\gamma = 0.5$, and (2) varying γ, payoff fixed and symmetric: "Both conditions..... show a pattern of radical probability judgment. We note a discrepancy, however, between the two conditions: the values-variable condition produces the greatest degree of conservatism, and hence implies more radical posterior probability judgments. If the data for the two conditions had been collected from the same observer at approximately the same time, his misconception of the distributions would be expected to be the same in both conditions. In such a case, a discrepancy would suggest that the hypothesis about the misconception of the distributions is incorrect, or at least that some additional factors counteract the effect of radical judgment in the probabilities-variable condition. We do not know, however, when the data were collected from this observer, and therefore need not speculate further on the source of the discrepancy."

Unfortunately, Kubovy appears to have reversed the two sets of data; in Green and Swets (1966, p. 90), it is the *probabilities-variable* condition which produces greater conservatism. Figure 1 embodies the error as well as the text.

(3) In the last quotation, Kubovy ignores the very relevant data of Galanter and Holman (1967, Experiment 1). Like Green and Swets, they compared the effects of varying payoff ($\gamma = 0.5$) and varying γ (payoff symmetric); but they ran two subjects rather than one and are quite explicit about the temporal contiguity of the two conditions. The data appear in Table 1[1]: for one of the two subjects (S1), as in the Green and Swets data, $l(c)$ and $l(c^*)$ appear to be related by different functions for the two conditions, though this time with the *values-variable* condition producing the greater conservatism. Thus, this

Table 1
Data Taken from Galanter and Holman (1967), Experiment 1

						l(c)/(l(c)+1)	
					$l(c^*)/$	Subject	
γ	o_{11}	o_{12}	o_{21}	o_{22}	$(l(c^*)+1)$	1	2
.1	2.0	−2.0	−2.0	2.0	.9	.705	.660
.3	2.0	−2.0	−2.0	2.0	.7	.602	.560
.5	2.0	−2.0	−2.0	2.0	.5		.506
.7	2.0	−2.0	−2.0	2.0	.3	.384	.368
.9	2.0	−2.0	−2.0	2.0	.1	.308	.293
.5	2.5	−2.5	− .1	.1	.962	.729	.741
.5	1.5	−1.5	− .1	.1	.938	.650	.686
.5	2.0	−2.0	−2.0	2.0	.5	.470	.506
.5	.1	− .1	−1.5	1.5	.062	.392	.325
.5	.1	− .1	−2.5	2.5	.038	.280	.211

difficulty *does* need to be taken seriously, and any explanation in terms of "some additional factors" has to allow either condition to produce greater conservatism.

(4) Kubovy's nonformal presentation of his model lumps together several related but nonequivalent models which need to be distinguished. One possibility (model A) is that the subject bases his choice of criterion on subjective likelihood ratio, l_S; that is:

A (i) there is a bijection $l_S(x) = s[l(x)]$

A (ii) the subject chooses c such that $l_S(c) = \beta^*$.

Another possibility (model B) is that the subject's choice is based on subjective posterior probability, or equivalently and rather more simply on subjective posterior odds: that is, if $\Omega(x) = [\gamma/(1 - \gamma)]l(x)$ is the objective posterior odds,

B (i) there is a bijection $\Omega_S(x) = t[\Omega(x)]$

B (ii) the subject chooses c such that $\Omega_S(c) = \Omega(c^*)$.

The models are certainly not equivalent. Model A, for example, implies

$$l(c) = s^{-1}(\beta^*), \tag{3}$$

whereas B implies

$$l(c) = [(1-\gamma)/\gamma]t^{-1}\{[\gamma/(1-\gamma)]\beta^*\}, \tag{4}$$

which is equivalent to Equation 3 iff t is a similarity transform, $t{:}\Omega(x) \rightarrow \alpha\Omega(x)$. Similarly, model A implies

$$\Omega(c) = [\gamma/(1-\gamma)]s^{-1}(\beta^*), \tag{5}$$

whereas model B implies

$$\Omega(c) = t^{-1}\{[\gamma/(1-\gamma)]\beta^*\}. \tag{6}$$

In fact, Kubovy's argument quoted in section 2 above is valid only for model A. However, the same data can be used to check model B, since it implies that all the symmetric payoff conditions should share the same $\Omega(c) = t^{-1}(1)$ up to error variance. This is clearly false (Table 2): $\Omega(c)$ shows a large and perfectly systematic variation with γ.

This argument aside, both model A and model B share an important prediction which Kubovy does not discuss. If an isobias curve is traced out by keeping bias conditions (including γ and payoff) fixed and varying discriminability, then $l(c) = \beta$ should stay constant across discriminability (see Equations 3 and 4). Evidence on this point is far from adequate (see Dusoir, 1975, for a review), but probably not so inadequate that it can be ignored. Creelman and Donaldson's (1968) data, at least, show a very clear dependence of β on discriminability, which Kubovy needs to explain. Also, Dusoir (Note 1) reports 12 12-point isobias plots obtained in a signal detection task with feedback, of which six show a Spearman ϱ corrlation between d' and β of greater than 0.5; this underestimates the failure of subjects to keep β constant, of course, since deviation from prediction can be systematic though nonmonotonic, for example, if subjects keep false alarm rate $p(r_2 \mid s_1)$ constant instead. Thus the data from isobias plots, like that from isosensitivity plots discussed above, suggests that something is wrong with Kubovy's idea as embodied in models A and B.

Of course, one way out of the isobias predictions is (model C):

C (i) there is a function $l_S(x) = u[f_1(x), f_2(x)]$

C (ii) the subject chooses c such that $l_S(c) = \beta^*$.

In this case, equal objective likelihood ratios at different discriminability levels need not have one and the same subjective representation and β need not stay constant. Evasiveness is not the sole motivation for model C, which arises naturally from the following kind of assumption. We have density function f(x) and, for any discriminability level α, $f_{1\alpha}(x) = f(x-\mu_\alpha)$, $f_{2\alpha}(x) = f_{1\alpha}(x-d_\alpha)$, so that all the distributions are identical up to x-shift. The subject represents f(x) by subjective density function $h(x) = g[f(x)]$. Clearly, $l_S(x,d_\alpha)$ is functionally related to $l(x,d_\alpha)$ if

$$\frac{f(x-d_\alpha)}{f(x)} = \frac{f(x'-d_{\alpha'})}{f(x')} \rightarrow \frac{g[f(x-d_\alpha)]}{g[f(x)]} = \frac{g[f(x'-d_{\alpha'})]}{g[f(x')]} \tag{7}$$

that is, if g is a similarity transform, which contradicts the assumption that h(x) is a density function, unless $h(x) = f(x)$. Consequently, A (i) must be false, and we are led to C (i). Though model C removes the problem of the isobias data, however, it does nothing to remove the problem of the isosensitivity

Table 2
$\Omega(c)$ for Galanter and Holman's (1967, Experiment 1) Subjects (S1 and S2) and for Green and Swet's (1966, p. 90) Subject (S3)

	γ				
	.1	.3	.5	.7	.9
S1	.265	.649	.887	1.454	4.014
S2	.216	.546	1.025 1.025	1.360	3.726
S3	.244	.600	.880 .980	1.727	4.14

data discussed in sections 2 and 3 above, since, in that case, f_1, f_2 stay fixed and their being equal variance Gaussian distributions implies a bijection $l(x) = \phi[f_1(x),f_2(x)]$, so that we have model A again with $u = s \circ \phi$. This leaves the possibility (model D):

D (i) there is a function $\Omega_S(x) = v[\gamma,f_1(x),f_2(x)]$

D (ii) the subject chooses c such that $\Omega_S(c) = \Omega(c^*)$.

Neither the isosensitivity nor the isobias data falsify model D. On the other hand, it is not easy to test model D at all.

(5) Kubovy reports an experiment in which subjects made verbal estimates of posterior probabilities. A general problem with any such experiment in relation to models A to D is that some further function seems to be required to link $l_S(x)$ to the verbal responses: only if that function is the identity function do the verbal responses cast direct light on $l_S(x)$.

However that may be, there is a more specific objection to Kubovy's experiment. Kubovy is at pains to argue that what happened on the "influence" trials was not such as to lead to overestimation of high posterior probabilities and underestimation of low ones, which is the pattern predicted and found. He implies that what matters is that the "influence" responses did not themselves systematically show this pattern. This seems naive: it seems quite possible that subjects in this sort of situation react *against* the bias present in the previous trials' "influence" responses, rather than towards it; so that what matters is whether the bias in the "influence" responses was *systematically related to* the predicted pattern. Now for Group 2, Sessions 11 and 12, such a systematic relation was present: that is, the "influence" responses were systematically opposite to the predicted pattern. If this relationship were confined to Group 2, Sessions 11 and 12, then the problem for Kubovy would be slight: the predicted pattern seems to have been found everywhere else in the data. The problem is that during all the other posterior probability estimation sessions (except Sessions 11 and 12 for Group 1) the same systematic relationship would seem to have held, though this is not made clear by Kubovy.

Kubovy writes: "The influence responses during the first four posterior probability estimation sessions were selected as follows: for each observation x, an observation y was drawn from the same distribution. If the difference in posterior probability of y and x did not exceed a predetermined value, d, the number $100 \times p(b \mid y)$, rounded to an integer, was used as influence response. Otherwise, y was sampled again. For one group of subjects (Group 1), d = .35 on Sessions 7 and 10 and d = .20 on Sessions 8 and 9. For the other (Group 2), d = .20 on Sessions 7 and 10 and d = .35 on Sessions 8 and 9. No bias was involved: The percentage of influence responses which constitute overestimates was between 49 and 51."

Now the case under discussion involves $f_2(x+1.0) = f_1(x) = N[0,1]$ so that

$$l(x) = \frac{(1/\sqrt{2\pi})\exp[-\tfrac{1}{2}(x-1)^2]}{(1/\sqrt{2\pi})\exp(-\tfrac{1}{2}x^2)} = \exp[x-0.5] \quad (8)$$

and the posterior probability, since $\gamma = 0.5$, is

$$\pi(x) = \exp(x-0.5)/[1+\exp(x-0.5)]. \quad (9)$$

What Kubovy did amounts to defining two functions:

$$u(x) = \pi^{-1}[\pi(x)-d]$$
$$v(x) = \pi^{-1}[\pi(x)+d], \quad (10)$$

so that

$$q(x) = p(y < x) = [1-\pi(x)]\{\phi(x)-\phi[u(x)]\}/\{\phi[v(x)] - \phi[u(x)]\} + \pi(x)\{\phi(x-1)-\phi[u(x) - 1]\}/\{\phi[v(x)-1] - \phi[u(x)-1]\}. \quad (11)$$

Table 3 shows some values of x, q(x) for $x \geqslant .5$, and by symmetry $q(x) = 1-q(1-x)$, $x < .5$. Clearly, $p(y < x) \gtreqless .5$ as $\pi(x) \gtreqless .5$, which justifies the claim above: Influence responses are systematically related to the predicted pattern which makes Kubovy's experiment difficult to interpret.

Table 3
x,q(x) for Some Positive Values of x (see text)

x	q(x),d=.2	q(x),d=.35
.5	.5	.5
1.0	.54	.58
1.5	.59	.70
2.0	.72	.81
2.5	.84	.91

REFERENCE NOTE

1. Dusoir, A. E. *Isobias curves in detection and recognition tasks*. Paper delivered at the 5th Annual Conference of the European Mathematical Psychology Group, Regensburg, 1974.

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NOTE

1. The values given are approximate: unit slope straight lines were fitted by eye to Galanter and Holman's double probability plots, and the values in the table were calculated from the nearest points on the fitted line to the original data points.

(Received for publication February 14, 1978;
accepted April 3, 1978.)