

**INDUSTRIAL PURCHASING BEHAVIOR:
SOME FINAL COMMENTS**

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We thank Professor Levitt for the kind tone of his response. Moreover, he and Professor Vatter have generously provided further insight into his study by the use of regression analysis and its Bayesian interpretation. Nonetheless, we believe that some comments on his reply are in order.

Professor Levitt's use of the data base to perform regression analysis is superior to either binomial or chi-square tests. Our objection to the anonymous company has been resolved by its omission from the regression analysis, which reduces much of the original ambiguity. Moreover, the results for the audience and presentation effects are largely in agreement with our own findings although Professor Levitt provides no additional information on the sleeper effect. Apart from objections due to the difficulty of generalizing these results beyond their laboratory setting, the single remaining issue therefore seems to be that of the source effect.

Indeed, the regression coefficients for this effect are exceedingly small: .019 and .050. With an informationless prior, the posterior probability of the correct sign for the regression coefficient is analogous to the complement of the alpha risk. Thus, a classical statistician would probably be unwilling to reject the null hypothesis of no source effect in both the low risk and high risk cases.

Of course, in a decision-making context it is more useful to employ posterior probabilities than to be concerned with classical alpha risks. In this case, however, the decision analysis should include relevant alternatives, their prior probabilities, and their payoffs, not merely directional effects. On the basis of the data presented here, given the choice of 'betting' on the source effect or on another marketing variable such as quality of presentation, most managers would surely decline the source effect alternative.

In conclusion, we believe that this exchange has been productive in defining more precisely the conclusions and limitations of a pioneering marketing study. It is our contention, however, that our original assertions have been substantiated and that the evidence for the source effect is, at best, very weak.

**INDUSTRIAL PURCHASING BEHAVIOR:
A BAYESIAN REANALYSIS**
Theodore Levitt*

The heart of the critics' paper is its conclusion that when one forgets about the responses of the questionable anonymous company, and then runs a chi-square test of the questionnaire results, the findings are different from those asserted in the research in which I employed the binomial sign test of significance. The critics' findings were based on a new set of raw data which were correctly calculated from my book.

I have no quarrel with their assertions in behalf of the chi-square test, nor do I think it incorrect to look at the results without the use of the anonymous source. There are a few minor differences between what is said and what I believe are more appropriate conclusions from my research. But the major point is, whose major findings are correct, the critics' or mine?

If one wants to question the adequacy of the evidence and be serious about the meaning of my research outcome, perhaps the most appropriate methodology is to construct a multiple linear regression model to test the results. This has now been done by my colleague Professor Paul A. Vatter, this time according to the critics' suggestion that the anonymous company data not be used.

The method was to set up a series of dummy variables, as follows:

For the low-risk decision — that is, whether to refer the salesman onward in the organization:

Variable 1. Code "1" if the response was positive, "O" if negative.

Variable 2. Code "1" if Monsanto was the source, "O" if Denver was the source.

Variable 3. Code "1" if a Purchasing Agent was the audience, "O" if otherwise.

Variable 4. Code "1" if a Chemist was the audience, "O" if otherwise.

Variable 5. Code "1" if the good presentation was made, "O" if a bad presentation.

The identical model and coding was set up for the high-risk question which required the respondent to indicate whether he would make a purchasing

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decision right then and there. With variable 1 as the dependent variable and variables 2, 3, 4, and 5 as the independent variables, the results are as follows:

Variable	Low Risk		High Risk	
	Regression Coefficient	Posterior Probability of Correct Sign	Regression Coefficient	Posterior Probability of Correct Sign
1	.878	1.000	.233	1.000
2	.019	.693	.050	.812
3	.069	.929	.120	.957
4	.128	.997	.099	.927
5	.125	.999	.271	1.000

If we look at the regression coefficients, we see that each has the sign I argued it should have in my book. For both the low-risk and high-risk situation, the coefficients associated with the source effect were positive, indicating a greater likelihood of a favorable response when Monsanto was the source than when Denver was the source, with the other variables (audience and quality of message) taken into account. The negative signs associated with variables 3 and 4 show the difference in reaction of purchasing agents and chemists relative to the reactions of students (the category eliminated in the dummy variable system). It indicates that each was less likely to have a favorable response than the students. As indicated in my book, the purchasing agents were more likely to take affirmative action than the chemists in the low-risk situation and the opposite was true (chemists more likely to take affirmative action) in the high-risk situation. The strong positive coefficient for variable 5 (quality of the message), demonstrates the obvious effect of the quality of the message – the good quality message elicits a higher number of positive responses than the poor quality when all other factors are held constant.

To evaluate the strength of the experimental evidence, we have calculated the posterior probability that the sign of the coefficient is correct starting from an informationless prior. One can see that for variables 3, 4, and 5 the posterior probabilities are extremely high. Thus the strength of the evidence on the audience effect and the quality of message effect is very great, even when we eliminate data from the ambiguous source. The evidence on the source effect (Monsanto vs. Denver, Variable 2) is not as conclusive as for the other variables, but is still considerable. One would be willing to bet at better than 4 to 1, based on this evidence alone, that there is a positive effect from the more creditable source in the high-risk situation, and at about 7 to 3 in the low-risk case. If one started with any prior belief in my hypothesis one's posterior betting odds would be even higher.

It seems to me this analysis says exactly what I argued in my book. Even if one eliminates the anonymous source from the data, the data are consistent with my book's findings that:

- 1) There is a source effect and it is somewhat greater in the high than the low-risk situation.
- 2) There is an audience effect where both the purchasing agents and chemists are

less likely to respond favorably than the students. In the high-risk situation, the chemists are more likely to respond than the purchasing agents, the opposite is true of the low-risk situation.

3) There is a strong effect from the quality of the message (i.e., a good presentation is more likely to result in a positive response than a poor presentation). This effect is stronger in the high than the low-risk situation.

In all cases one would be willing to wager at pretty substantial odds, based on this evidence alone, that the direction of the effects noted is the true direction of these effects.

Finally, unrelated to this minor tempest, I cannot help but observe that I am pleased and impressed with the effort that went into the paper's analysis of my book. It obviously reflects enormous scholarly dedication and thorough workmanship and should not go unnoticed for its excellence.