A year or two ago a colleague introduced me to a lawyer at the FTC as an “expert on the duration of advertising effects.” The lawyer’s direct and utterly justifiable response was, “Oh, how interesting. How long do they last?” To my chagrin, I realized that a few years of involvement in cumulative advertising studies had given me no basis upon which to answer that most important question. My attempt to obtain a substantive, broadly based answer to that question began the research which led to the survey published in the *JMR* in November 1976.

I think if one understands the genesis of the study, then some choices I made become more easily understood and some motives attributed to me by commentators are recognized as inappropriate. Because I began the survey with the intention of answering a reasonable “real world” question, I was forced to set some priorities. First, I wanted a number for an answer to the question rather than a philosophical skirting of the point. Second, to be useful in a real-world sense, that numerical answer had to be based on as broad an experience base as possible. Third, my interest in the question was primarily substantive and not methodological. As a result of these priorities I was forced to take the most broadly used model of cumulative advertising effect that I could find.

Anyone who tries to accomplish goals similar to these would be forced to choose the Koyck model exactly as I was.

**MISATTRIBUTION**

Because I was only analyzing results of previous studies and attempting to find consistency in them, it really does not seem appropriate for critics to claim that I “chose” the Koyck model in the sense that it is the model I prefer or “advocate,” as Peles puts it. I do not happen to like it very much at all as a model of advertising effect, but it best suited the survey’s purposes and let me compare a great number of studies.

Peles’ comment illustrates this misattribution, even though in general I find his points interesting and helpful. For example, I do not think it accurately reflects the research process involved in the survey to introduce his equation 1 as “The single-variable [sic] exponentially distributed lag model (used by Clarke) . . . .” I think his remarks would be more consistent with the path of my research if he would say within the parentheses, “(used by 70 authors and thus chosen for the survey by Clarke) . . . .” This statement would not detract from his point, but would reflect more accurately the historical viewpoint of the survey article. I react similarly when Peles says, “Clarke’s failure to distinguish between single- and two-variable models . . . .” A more accurate statement would be, “Clarke examined studies which failed to distinguish between single- and two-variable models . . . .”

This point may appear to be minor, but I think it is important. As I read the marketing research literature I find a distressing lack of attempts to draw generally applicable knowledge from individual studies. If this situation is to be remedied, researchers must become sensitive to the compromises that must be made between reflecting each author’s specific analysis and model preferences on the one hand and drawing comparable results from many studies on the other. This is not a trivial problem area and deserves more attention than I could give it in the survey, and further study in the future. It has been a source of concern to me that almost all the comments I have received on the survey relate to methodology in a narrow econometric sense and that so few readers have addressed the substantive findings or the problems of result comparison across studies.

In the context of Peles’ comment (which I think
has interesting methodological and survey points), let me illustrate the situation.

THE PELES MODEL AND THE KOYCK MODEL

Peles' comment is concerned with the problem of omitted variables, in this case competitive advertising. In his article he computed both the Koyck model (which I used in the survey) and another model (the subject of his comment). He states that he will discuss the omitted variable problem “on the basis of (his) article simply on the grounds of greater familiarity” (emphasis mine.) This statement is understandable, but not totally true. His comment must be based solely on his study because the same model does not appear elsewhere in the literature. One cannot determine (1) how generally applicable his results are; (2) how often the model gives believable results; or (3) whether the results vary systematically across applications because no one else has used his model. Thus, for practical reasons, I could not have used his model for the survey. I hope it is clear from this point alone that when Peles points out that he rejected the Koyck model in favor of another model, his choice does not preclude my finding the Koyck model more appropriate for the survey, nor does it imply that I do not agree with his choice of model in the context of his own article. If his preferred model had been used in 69 or 70 other papers I would gladly have considered using it (or any other such widely used model).

Before discussing Peles' specific model, I must make one other point in order to draw as much useful information together as is available on the question of the omission of competitive advertising effects. Peles states that “Clarke's article analyzes distributed lag models in which firms' sales are affected only by their own advertising and not by their competitors'. The Peles study constitutes an exception to this rule.”

It is important to note two problems with this statement.

1. Although not of the same specification as Peles' model, there are other models in the survey in which competitive advertising is at least an implicit part.
2. The sales of the firms are affected both by their own and competitors' advertising even if the models used to study this phenomenon do not include competitive advertising explicitly.

The studies by Beckwith (1972) and Clarke (1973) are both applications of the “seemingly unrelated regressions” method to systems of competing brands. The use of this method was motivated partially by the authors' convictions that the advertising of one brand affected the sales of the other brands. In each of these studies the implied duration intervals were much longer than the average duration intervals of studies done on similar length data intervals, and these findings tend to support Peles' contention that the computed duration of advertising effect should be longer if the effects of competitive advertising can be at least partially removed.

Koyck models in which market share is related with advertising share, although not including competitive advertising in as explicit a formulation as Peles' model, also include information about competitive advertising activity. (See Telser 1962 and Clarke 1973 for means of computing sales-advertising elasticities from share equations.) In an earlier version of the Clarke survey (1975), Table I demonstrated that there was no statistically significant difference between the λ's computed in share Koyck models and those from sales Koyck models. Peles' intuitively appealing conceptualization appears to receive less than total support as being empirically dependable in contrast to theoretically demonstrable.

Let me now examine the difference between the model Peles chose for his study and the Koyck results I drew from his study. Peles states the problem this way.

Unfortunately, Clarke's interpretation of the two-variable model may have been affected by a slip in presenting the Peles figures. For example, the figure for λ which he quotes . . . is not λ as interpreted by Clarke, but (γ + β) . . .

I hope this adequately expresses the error as he sees it.

His contention is that I chose λ in his equation 1 when I should have chosen γ (in 3), which Peles says is the “true” parameter of equation 4.

I hope this clarifies the situation (as well as accurately states it). Peles himself puts “true” in quotation marks, so he does not really mean true in an absolute sense. What does he mean? It seems to me that duration intervals derived from γ in equations 3 and 4 should be called “the duration of advertising effect in a competitive advertising vacuum.” That would lead us to call the duration intervals derived from λ in equation 2 “the duration of advertising in a competitive advertising situation.” I can see that academics and philosophers could be interested in the “duration of advertising effect in a competitive vacuum,” but is there any reason to call one type of interval “true” and the other “biased”?

My interest was in getting an operational estimate of the duration of advertising effect for managers and public policy decision makers. The theoretical value of Peles is much less useful to them than to know how long advertising's effects last when there is competition. If a manager wanted to allocate advertising dollars in a competitive market he would not want to use Peles' “true” duration interval because it is “biased” upward for his purposes: it eliminates the shortening of the duration of advertising effects due to competitive advertising interference because it takes
I found his comments in marked contrast to the frequent use of the survey by authors who completely ignore its findings and recommendations but nevertheless cite it to relieve themselves of the necessity of providing a reasonable literature review.

AN ERROR CORRECTION

An error has been found in the derivation of the implied duration interval in the survey article. It was pointed out that in obtaining equation 5 (Clarke 1976, p. 348), because \( 0 < \lambda \leq 1 \), \( \log \lambda \) is less than zero and thus the inequality should be reversed (Houston 1977). That is certainly true and was an error on my part. Had the reversed inequality stood as unavoidable it would have compromised somewhat the conclusions of the survey. Fortunately the error does not have such major implications.

The development of an estimate of the duration interval begins with choosing \( \lambda^* \) as the least integer such that \( \beta / \beta < p \). Why the inequality, anyway? Because there may not be an integer \( k \) such that \( \beta_k / \beta = p \). Because we chose \( \lambda^* \) to be the least such integer we would expect \( \lambda^* \) to be a fairly close lower bound on the duration interval. Indeed, \( \lambda^* + 1 \) could have been the least integer upper bound instead of \( \lambda^* \) in Table 1 of the survey. This is not necessary, however. Suppose we were to start over again in the development and look for \( k^* \) such that \( k^* \) is the greatest integer such that \( \beta_k / \beta \leq p \). Going through the same steps we did between 4 and 5 in the survey, we discover that \( k^* = \lambda^* + 1 \), which is not surprising once one thinks about it. So now \( \lambda^* \) is a lower bound for the duration interval and \( \lambda^* + 1 \) is an upper bound for it. If noninteger values are allowed, as was computed for each of the studies in Table 1, the continuous analogue is not a bound at all but rather an equality.

<table>
<thead>
<tr>
<th>Table 1</th>
<th>EFFECT OF THE DEPENDENT VARIABLE ON THE IMPLIED DURATION INTERVAL (Raw Data)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable</td>
<td>Coefficient of lagged dependent variable (( \lambda ))</td>
</tr>
<tr>
<td>Share</td>
<td>.588</td>
</tr>
<tr>
<td>(0.038)</td>
<td></td>
</tr>
<tr>
<td>Sales</td>
<td>.596</td>
</tr>
<tr>
<td>(0.040)</td>
<td></td>
</tr>
</tbody>
</table>

None of the differences is statistically significant at the \( \alpha = 0.05 \) level. Source: Clarke (1975).
REFERENCES


