FRANK M. ANDREWS AND AUBREY C. McKENNELL

RESPONSE TO GUTTMAN & LEVY'S ARTICLE 'ON THE DEFINITION AND VARIETIES OF ATTITUDE AND WELLBEING'

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ABSTRACT. Guttman and Levy have prepared an extravagant critique focused mainly on the 1980 Andrews–McKennell article in this journal. The clearly stated purpose of that article was to report a "series of explorations into the affective and cognitive components of some of the more widely used measures of perceived well-being". Guttman and Levy ignore this. They proceed on the mistaken impression that we were (or perhaps should have been) embarking upon a definitional exercise to relate the concepts of attitude and wellbeing. Yet the reason we did not cite their article on that topic was precisely because it did not address in a direct or focused way the topic that concerned us. Their critique consists of an entirely irrelevant reanalysis of some attitudinal data by Ostrom, together with a tissue of recondite definitional and methodological issues of little consequence either for the objectives or the conclusions of our research. Their dismissal of our work as 'scientific retrogression' rests on an a priori definition of science that fits their own methodological style but excludes that of many other prominent researchers. Their comments reflect an attempt at methodological imperialism. We defend our independence – and that of other investigators – to use promising new methodologies other than the particular approach advocated by Guttman and Levy. (Their denunciation of the new methods of structural equation modeling is not shared even by the authoritative reviewer they themselves quote.) In addition to Guttman and Levy's specific criticisms, our Response addresses general methodological issues such as the status of structural modeling and the testing of structural models. In a concluding section we identify areas that merit further research.

The Guttman–Levy paper 'On the Definition and Varieties of Attitudes and Wellbeing' (1981) is largely devoted to a many-sided critique in extravagant terms of a previous article by us in this journal (Andrews and McKennell, 1980), with some additional mention of another article (McKennell and Andrews, 1980). (A third article not mentioned by Guttman and Levy is part of this series – McKennell, 1978.) Although we have high regard for much of Guttman and Levy's research, we believe this particular article falls far short of their usual high standards. Did we not feel compelled to respond to its unreasonable comments about our own work, we would give it scant attention.

We believe the criticisms Guttman and Levy make about our work reflect a misunderstanding about our purpose – though our article was not ambiguous about this – and an idiosyncratic view about what constitutes useful ways to proceed in social science that is not shared by recent authoritative reviewers

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or ourselves. Guttman and Levy's comments reflect an attempt at methodological imperialism that we find surprising, and we defend our independence – and that of other investigators – to use promising new methodologies other than the particular approach advocated by Guttman and Levy.

This Response is divided into three main sections. First, we comment on some of the criticisms made by Guttman and Levy that are specific to our article; these matters do not lead far but need to be addressed early. Then we turn to several wider-ranging topics dealing with structural modeling – issues of long range and general importance. And, third, we briefly consider a couple of future research activities suggested by the preceding discussions.

1. RESPONSES TO SOME CRITICISMS SPECIFIC TO OUR ARTICLE

A. *Non-citation of the 1975 Levy-Guttman Article*

One of the most repeated charges in the Guttman-Levy article is that we did not cite the article by Levy and Guttman 'On the Multivariate Structure of Wellbeing' that appeared in this journal in 1975. This criticism appears in various forms at least five times!

The reason we did not cite their article – which we *do* believe to be an interesting and scholarly contribution – is simply that it has little relevance to what we were investigating. The purpose of our article was clearly stated in its Introduction: to report "a series of explorations into the affective and cognitive components of some of the more widely used measures of perceived well-being" (p. 127). The Levy–Guttman article was not primarily addressed to this topic; it presented no detailed hypotheses with regard to this matter; its discussion of findings devoted just one sentence to this topic – and noted that their results did not distinguish between affect and cognition. (This did not surprise us, because we believe the data Levy and Guttman analyzed were not particularly well-suited for finding such differences – though the data were good for other purposes – and because the facet/Smallest-Space-Analysis methodology Levy and Guttman used is not designed to apportion variances of observed measures into components reflecting distinct sources of influence.)

In short, the Levy–Guttman article simply did not address in a direct and focused way the topic that concerned us.

B. The Purpose of Our Article

Despite the clear statement about the purpose of our article (quoted above), Guttman and Levy seem to be under the mistaken impression that we were (or perhaps should have been) embarked on a definitional exercise to relate the concepts of attitude and well-being. In fact, whether perceptions of well-being might be 'attitudes' was not an issue for us. (For years, we had been doing 'attitude surveys' to measure perceptions of well-being!) However, lest there were any doubt about our stance, we did quote a Fishbein–Ajzen (1975) definition of 'attitude' and then said: "It seems *obvious* that people's responses to questions about perceived well-being ...meet the above definitions and hence that knowledge about the nature of attitudes may ...contribute to our understanding of self-reports of well-being" (p. 130, emphasis added).

Actually, our starting point was the considerations and some empirical results relating to the role of cognition and affect in the perception of well-being set out in an earlier article in this journal by McKennell (1978). The brief section titled 'Connections to Some Previous Research on the Nature of Attitudes' in the Andrews–McKennell article was an incidental part of what we were about and served simply to point out that the concepts 'affect' and 'cognition' have a long history and figure prominently in attitude research.

It is ironic to observe that had Guttman and Levy attended to the purpose we stated for our article, they apparently would have evaluated it differently. Referring to our article, their penultimate sentence says: "Were this a part of a *process of exploration* in order to arrive at actual testable hypotheses, then it might have some scientific value" (emphasis added). As quoted above, the stated purpose of our article *was* to report a 'series of explorations'!

C. Critiques of Our Analysis and Interpretation

Guttman and Levy express distaste for a component-oriented analysis, including the structural modeling approach we used, and we present our comments on this matter later. In addition, however, their article includes a generous sprinkling of specific criticisms about the analyses and interpretations in our report, and some of these deserve attention here. Particularly noteworthy are criticisms by Guttman and Levy that simply *disregard* clear reports of how we proceeded or what we found.

Expectations about the global well-being measures. Guttman and Levy observe (correctly) that we presented some expectations regarding the relative impacts of affective and cognitive components on the measures of perceived well-being that we were analyzing; Guttman and Levy then go on to say (incorrectly) that our classification of the measures influenced the modeling results reported in our Exhibits 3 and 4. Furthermore, in a surprising lapse of good scholarship, they add *their own* (incorrect) words to a passage they quote from page 140 of our article, and then suggest that *we* said the initial classification would affect the results! Actually, anyone reasonably familiar with structural modeling could see from the model presented in our Exhibit 2 that the global well-being measures shown there were free to be assigned whatever balance of affects and cognition that would best fit the data. (If there were any doubt about this, one could examine the results in our Exhibits 3 and 4 and see that these results did not agree perfectly with the initial expectations. The fact that the results came close to those theoretical expectations, however, was one of the fundamental and innovative contributions of the paper). Guttman and Levy are *incorrect* when they write: "Revision of their classifications of the items will also change their empirical results".

Robustness of conclusions. Guttman and Levy rail at us for having questioned the degree of overlap that might exist between the affect and cognition factors. They seem not to have noticed that our article explicitly said: "the degree of presumed overlap between affect and cognition actually has little effect on the general conclusions that will be drawn about the nature of self-reports of well-being" (p. 131), and (contrary to what Guttman and Levy say) we did present the logical evidence for this – in Section 5.3 of our article.

Our article reported results computed under the assumption that all factors were statistically independent of one another. Now, spurred by the expression of doubt in the Guttman–Levy article about the robustness of our conclusions, we have actually run a model for the American data using an alternative assumption that Cognition related +0.5 to Positive affect and –0.5 to Negative affect. As expected on the basis of the logical evidence presented in our article, this new run ranks the well-being measures in nearly the same order as did the original results in our Exhibit 3: The correlation (rho) between the order of the measures when ranked according to their sensitivity to Positive affect (a) under the assumption of substantial relationships between affect and cognition (i.e., the results from this new run) and (b) under the alternative assumption of independence of affect and cognition (i.e., the results reported in Exhibit 3 of our article) is 0.93; the comparable rhos are 0.90 for Negative affect and 1.00 for Cognition. Furthermore, changing the assumption about the relationships between the factors has *no effect at all* on the estimates of common methods variance and unique variance that we presented. Guttman and Levy are simply wrong in suggesting that changing the assumptions of a model will always change one's conclusions.

Section 5.3 of our article also described a second series of explorations designed to see what impact changing assumptions about the relationship between two other factors (Positive affect and Negative affect) would have on the conclusions, and it reported that here also the findings were highly robust.

Checking the robustness of conclusions under different sets of modeling assumptions has been called 'sensitivity analysis' (Land and Felson, 1978) and has been widely applied in our work. We are amazed that Guttman and Levy simply disregard the evidence we presented about the robustness of our conclusions.

Maximum-likelihood estimation. Guttman and Levy claim our use of maximum-likelihood methods was inappropriate, though the reasoning behind their claim in unclear and we believe it to be unfounded. Furthermore, Guttman and Levy mislead their readers when they report that we 'justified' our choice of the method we used on the basis of its producing maximum-likelihood estimates. The parameter estimates on which our results were based were produced by the Joreskog-Sorbom LISREL program, which is an analytically powerful and highly regarded technique that was readily available to us. (Guttman and Levy themselves note that this program is 'popular'.) LISREL does produce maximum-likelihood estimates, and we noted that in our article, but this characteristic was of little importance in our choosing LISREL. What *was* important was that the parameter estimates produced by LISREL be appropriate and useful. We have no reason to suspect the estimates we obtained are incorrect; Guttman and Levy present no such evidence; and current statistical literature finds such estimates to be good. Lee and Jennrich (1979), for example, in a study of algorithms for covariance structure analysis with latent variables (which is the type of analysis we were conducting) found that maximum-likelihood estimates from the modified Fletcher–Powell algorithm (which is the algorithm incorporated in LISREL and that we used) "converged nicely to the same weighted least squares and maximum likelihood estimates that were obtained from other algorithms" (p. 111). Another recent study (Raj, 1980) found that maximum-likelihood estimates for simultaneous equations with either normal or nonnormal disturbances (which covers the kind of analysis we were doing) agreed with the estimates obtained from the alternative approaches of least squares, two-stage least squares and three-stage least squares. In short, there is good reason to believe that our use of a maximum-likelihood technique to produce parameter estimates has led to correct results.

Now it is the case that doubts exist about the appropriateness of the *standard errors* of these maximum-likelihood parameter estimates (Lee and Jennrich, 1979), and – as we noted in our article – the usefulness of the chi-square-based probability tests of model fit has also been questioned. It is important to note, however, (and Guttman and Levy seem to have overlooked it) that neither of these types of statistics are used in our analysis.

A peculiarity of the Guttman–Levy discussion of maximum-likelihood methods is that it seems to have been stimulated by a false impression that we confused sampling errors with 'observation' (i.e., measurement) errors. That this was not the case should have been obvious from our reporting actual estimates of the influence of correlated and random measurement errors. (The ability to explicitly take account of, and generate estimates of, various kinds of observation errors is one of the important advantages of the new structural modeling technology we used, and is one of the ways our results go beyond most of what has been available in the literature previously.) When Guttman and Levy miss this aspect of our results, we wonder whether they have an adequate understanding of what we did or of the methodology we employed.

Components and varieties. Guttman and Levy suggest we confounded the terminology of 'components' and 'varieties'. On the contrary, we were clear and consistent in our usage, and we *intentionally* talked about 'components'. The notion of 'varieties' does not appear in our article.

Components of components. Guttman and Levy claim we were in effect talking about 'components of components'. Neither the phrase nor the idea occurred in our article.

D. *The Discussion of Ostrom's Work*

A lengthy section of the Guttman–Levy critique is given over to a reanalysis of some data by Ostrom. The only clue Guttman and Levy give as to why they consider this reanalysis to be relevant to our work is their claim that we 'leaned heavily' on Ostrom. But this is incorrect. We devoted little more than a paragraph to Ostrom's work and said that we cited it simply as an example of the attitude component trilogy that has been investigated by social psychologists for the past several decades. We did not ourselves analyze Ostrom's data or refer to it in any detail. Almost any author who has written about the classical trilogy of components (McGuire, 1969, p. 155, lists nine) would have sufficed as an alternative example. Guttman and Levy's lengthy discussion of Ostrom's work is an irrelevant diversion as far as the critique of our article is concerned.

We will not burden this Response with further rebuttals to the captious comments made by Guttman and Levy. Cries of you-didn't-cite-our-article, generalizations that are not dependably true, erroneous quotations, and disregard for investigators' stated purposes, methods, and results are not the stuff of which productive science is made.

2. SOME BASIC METHODOLOGICAL ISSUES OF GENERAL IMPORTANCE

A. *On Structural Modeling with Latent Variables*

Among the criticisms of our work by Guttman and Levy, the most sweeping is their attempt to discredit our entire methodology of data analysis – the new technology of structural modeling with latent variables. (Guttman and Levy refer to it as a "peculiar bootstrap-type of estimation"!) That methods of this complexity should be subject to some criticism is not surprising, especially when they are still in the process of development. Guttman and Levy, however, denounce the development as a whole. Moreover, they go out of their way, quite wrongly, to persuade the reader that their views are widely shared by other authorities. According to them, Bentler's recent review (1980) of this type of analysis "gives a most disheartening assessment of the contribution to science made by these approaches". In language similar to that in which they dismiss our own work as "a good example of scientific retrogression", and presumably for the same reason, they add: "The multivariate component approach seems to be one of those that have a bright future behind them".

Well, however much Guttman and Levy might believe this, and for whatever reasons they may wish it so, it is simply not the case. This is not the place for detailing the contribution the new methods have already made and stand to make to psychology and sociology, but we are confident that any open-minded reader of Bentler's review will find it optimistic, not disheartening. The best authority here is Bentler himself. What he actually said about the new developments is that he believes they hold "the greatest promise for furthering psychological science" (p. 420). That Guttman and Levy could overlook this and arrive at the opposite conclusion from the same material is astonishing.

B. On Testing Structural Models

The statements in the Guttman–Levy article about the untestability of structural models need attention because they seem to us to be clearly wrong, and to indicate misunderstanding. As indicated above, we are in agreement with Guttman and Levy that the formal tests of model fit presently available from the LISREL computer program are of limited usefulness. However, it does not follow that models cannot be rejected on other grounds, and we indicated the specific kinds of evidence to be considered in Section 4.3 of the Andrews–McKennell article. (Our brief treatment there was intended to be suitable for the general readership of this journal; readers who want more detailed discussions may wish to consult Bentler and Bonett, 1980; Fornell and Larcker, 1981; and/or Maruyama and McGarvey, 1980).

Guttman and Levy may have been mislead because, in the interests of brevity, the Andrews–McKennell article did not describe the extended explorations that led to the model used to develop the estimates of affective, cognitive and other components of the well-being measures. The article did, however, report the evidence showing that the model fitted the data well, and the McKennell–Andrews article presents much of the developmental sequence that suggested we should reject the simpler 'life-as-a-whole' models and adopt the 'affect-and-cognition' model.

The role of formal hypothesis testing in social research (and here we regard formal significance tests of a model's fit as a special case of hypothesis testing) has been a subject of considerable discussion. (A useful summary appears in Morrison and Henkel, 1970). It is widely agreed that one can reject as 'false' those hypotheses that do not fit the data, but that one can never be sure that a hypothesis that does fit the data is the 'true' explanation. This is elementary philosphy of science, and it applies to 'model testing' as much as to any other form of hypothesis testing in science. The Andrews–McKennell article recognized this point – that one can never expect to prove, in an ultimate sense, that a model is correct – and it then went on to observe that the model being used seemed reasonable in that it fitted the data well, was in accord with extant theory, and gave reasonable parameter values in the light of other knowledge about the limits within which those values should fall. We believe that Guttman and Levy are incorrect when they suggest that the 'testing' of structural models is logically a weaker enterprise than 'testing' other formulations of hypotheses. In all of empirical science, the quest is to account for observed regularities with a parsimonious set of concepts that fit together reasonably in the light of available theory and give useful and consistent results. Structural modeling is simply one attractive way among many for trying to achieve this.

3. SOME FUTURE RESEARCH ACTIVITIES

Preparing this Response has stimulated us to consider a number of research activities that seem relevant to topics addressed above and/or in the Guttman–Levy paper. We briefly record these ideas here as items for a future research agenda.

A. The Meaning of the Cognitive Factor

One of the aspects of the work reported in the Andrews–McKennell paper that concerns us (as we acknowledged in the original article) is the fact that in neither of the data sets analyzed there, nor in any other data set, were we able to identify a reasonably 'pure' indicator of the cognitive component of well-being. As was explicit in the analyses we reported, the cognitive factor was defined residually: It was what the global well-being measures had in common after taking account of Positive affect, Negative affect, and correlated measurement error. In view of extensive discussions about attitudes incorporating affective and cognitive components, it seemed reasonable to call this residual factor 'cognition'. This also fit in nicely with the idea that self-reports of well-being might reflect two kinds of reactions: 'emotional' (i.e., affective) responses, and 'rational' (i.e., cognitive) evaluations against one or more criteria. (These ideas are more fully discussed in McKennell, 1978, McKennell and Andrews, 1980, and in Andrews, 1981.) Furthermore, this view of cognition as involving a judgement of how the self stands with respect to one or more criteria provides a theoretically appealing linkage between two recent sets of findings: (a) the findings reported in the Andrews–McKennell article that satisfaction measures tended to be more 'cognitively' oriented than happiness measures, and (b) the findings reported by Michalos (1980) that satisfaction measures reflected a 'gap' between achievements (i.e., the nature of oneself or one's life) and aspirations (i.e., the criteria).

Some of the comments by Guttman and Levy about our being 'confused' with respect to the cognitive character of the satisfaction measures may stem from their not recognizing the residual character of the cognitive factor in our analyses. How the cognitive factor in satisfaction measures relates to previous treatments of 'cognition' in attitude research would be worth further theoretical analyses and empirical investigation. (In a current project we are experimenting with some new items intended to provide purer and more direct measurements of the cognitive factor that we obtained only by residualization in our previous work; results from this new undertaking will be reported as they become available.)

B. Measures of Fit for Structural Models

A previous section of this Response mentions the kinds of evidence that would lead one to reject a structural model, and Guttman and Levy also raise this issue. While we are much more sanguine than they about the rejectability of inappropriate models, we do believe that further methodological work in this area would be helpful. Users of structural modeling algorithms would benefit from measures of model fit that are more sensitive than calculations of mean (absolute) covariance residuals and that are free from the strict probability/statistical-test orientation of the presently available chi-square indicators. Work recently reported by Bentler and Bonett (1980) and by Fornell and Larcker (1981) addresses this general need but – in our view –

has not yet arrived at a fully satisfactory solution. Hopefully, further developments will be forthcoming.

University of Michigan
University of Southampton

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