Laboratory-Simulation Versus Revealed-Preference Methods for Estimating Travel Demand Models

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The results are reported of an empirical comparison of two different approaches to deriving models of travel-choice behavior: models based on revealed choices and models based on responses to controlled scenarios. In particular, interest centers on the results of a longitudinal study in which both methods were used to derive models of modal choice for a random sample of persons living in two Iowa cities over a five-month period during 1979. Models were compared on the basis of (a) predictive ability and (b) consistency of the parameter estimates over time and space. Specifically, the laboratory-derived models were shown to be equal to conventional models in terms of predictive ability for revealed-behavior data. Moreover, the parameter estimates of the laboratory-derived models were for the most part temporally and spatially stable and were consistent with the parameter estimates of revealed-choice models. Finally, the laboratory models provided a more cogent interpretation of the modal-choice process than did the revealed-choice models.

Choice models based on the revealed or observed choices of individuals have historically received the most attention in the area of research and application in travel-choice modeling (1-3). Recently, attention has been given to the possibility of deriving models based on individuals' responses to hypothetical situations that simulate variation in travel-choice attributes (4-6). Both methods have advantages and disadvantages. Revealed-behavior, or econometric, choice models have high face validity in that they are calibrated to real data; models based on scenario responses have lower face validity in that choices are made in hypothetical, not real, situations. Revealed-behavior models suffer from a lack of controls in that variables may have limited ranges, attributes may be highly correlated (e.g., times and costs), and some choice alternatives may not yet exist; laboratory-simulation models can be designed to cover broad ranges of choice attributes, can reduce or eliminate attribute intercorrelations, and can include choice alternatives that do not now exist. Revealed-behavior models must rely on assumed functional forms that, at best, can be tested only weakly and must cope with biases introduced by unobserved attributes and/or other misspecification problems; laboratory-simulation models, of course, can control for these potential sources of bias by appropriate design techniques before data are collected.

Clearly, therefore, the approaches are complementary: Each is strong where the other is weak, and vice versa. The intent of this paper is not to argue the virtues of one against the other but to compare aggregate model forms derived from the two methods to see whether an even closer relation exists than has previously been assumed. In particular, we will derive parallel models from two sets of data obtained from the same individuals. We wish to compare the coefficients derived from the two approaches both separately and globally. We shall assume throughout that the revealed-behavior data constitute the "true" state of the world, but, of course, there is no guarantee that this is correct.

Revealed-behavior data require an observation of what was chosen (or how often it was chosen) and what was rejected (or how often it was not chosen), plus actual measurements of associated travel attributes and interpersonal factors. Models are calibrated directly to these data, and statistical tests determine their "adequacy". The true validity of these models, however, lies in their ability to reproduce other choices not drawn from the calibration sample or an associated "hold-out" sample. Results in transferability tests have been mixed (7-9), and few would argue that overwhelming success has been achieved.

In contrast, laboratory-simulation methods do not require revealed-behavior data for calibration. They do, however, require real data for initialization. That is, given a set of initial conditions described by a vector of attribute measures and interpersonal measures, the laboratory-simulation models "forecast" the choice behavior of the individual. The difference is that the simulation method calibrates its models to the laboratory response data and not to real choice data. The validity issue for these models, therefore, is their ability to recover parallel real choice data as well as to transfer successfully. The first of these tests is the most obvious and the one that is examined in this paper; later papers will explore the issue of transferability.

The strongest argument for the comparability of the two methods is that they are the same in philosophy and theory and that there are only minor analytic differences; they differ in the type of data obtained. In fact, the theoretical similarities have recently led some researchers to propose that the above problems need not be inherent in modeling social behavior (5,10); that is, econometric models have problems because of data, not because of theory. In particular, the problems discussed earlier could be largely overcome if it were possible to conduct controlled social experiments in which individuals could be observed making repeated choices in a variety of situations that exist now and that might exist in the future. Such data, therefore, would permit both the estimation of models at the individual level and forecasts of likely responses to system changes over time.

Given that it is practically infeasible to conduct such experiments in the "real world", a second alternative is to design simulation experiments in which individuals are confronted with a number of hypothetical choice scenarios in which they are required to respond in the way they would be most likely to if they were placed in that situation. Individual choices (or other responses) can be analyzed given sufficient observations for each individual.

Despite consistent evidence amassed over the past five years (5,10-12) that models built on responses to hypothetical scenarios are accurate predictors of real behavior in analogous situations, little attention has been directed to this work. This situation prevails despite the continuing failure of econometric models to predict very well to any data other than those from which they are calibrated (7-9). Of course, the noneconometric approach contradicts the established dogma of "revealed preferences" being the only legitimate data for econometric analysis, and therein lies the crux of the matter. The counterarguments typically run as follows (13):

Yes, in principle the limitations of revealed preference methods could be circumvented, but what guarantee is there that the way people behave in hypothetical situations bears any resemblance to the way they behave in the "real world".

As mentioned earlier, however, this argument does not fit the facts because so-called "laboratory" models have enjoyed surprising, if unheralded, success (5,10,11). Hence, there seems to be strong evidence that models derived from responses to hypothetical situations can predict actual behavior very well. Previous research examples include modal choice, store selection, and residential location (4,12).

This evidence, however, does not imply that laboratory-simulation methods are better than more conventional revealed-behavior methods. It does suggest, however, that they deserve equal attention and that they can no longer be dismissed as unacceptable on "religious" and not scientific grounds. Although few comparisons with revealed-behavior methods have been undertaken to date, there is some evidence to suggest that both methods may be comparable from a purely predictive standpoint (5,11).

This paper attempts to provide a comparison based on data derived from a two-city, longitudinal study of modal-choice behavior. The study design is described in the sections that follow. The background and results of the investigation are described in detail, and the implications of the results for current research in travel-choice modeling are discussed.

METHOD OF APPROACH

Overview

Comparison of econometric and laboratory-type simulation methods requires parallel data-collection efforts in which identical data are obtained. As part of a longitudinal study of traveler mode preferences and choices in two cities in the state of Iowa--Iowa City (a university town of about 50 000 population) and Cedar Rapids (a city of about 100 000 population)--parallel data necessary for the conduct of such a test were obtained. The data collection was done in survey form and consisted of two main sections of interest: (a) an experimental design or simulation section, in which respondents were asked to indicate mode choices for each of a number of hypothetical scenarios, and (b) a current-behavior section, in which respondents were asked to provide information about their current travel habits.

The Simulation

The simulation section comprised a set of 30 hypothetical bus-automobile scenarios that consisted of different combinations of levels of 10 mode attributes: (a) automobile parking cost, (b) automobile travel time, (c) gasoline cost, (d) bus fare, (e) bus travel time, (f) walking distance from home to the closest bus stop, (g) walking distance from the closest bus stop to the work destination, (h) frequency of bus service, (i) bus crowding, and (j) season of the year (survey 1) or gasoline availability (survey 2). Each of these 10 variables was assigned three levels reflective of past, current, and likely future conditions. The attributes and their levels and typical resulting scenarios are given in Table 1.

There are 3^{10} possible combinations of these attribute levels in a complete factorial enumeration. From this total, a set of 405 combinations was selected that have the property of permitting the derivation of orthogonal estimates of all main and two-way interaction terms in a regression-type model. Fifteen different sets of 27 combinations each were created to produce survey designs that were manageable in size for respondents to complete. Each of the 15 sets of 27 combinations has the property of being a main-effects plan--i.e., permitting estimates of all main effects, assuming negligible interactions.

Three common treatment combinations were added to each of the sets of 27 to ensure that all respondents faced some common items. These combinations were (a) all attributes favoring bus, (b) all attributes favoring automobile, and (c) all attributes at middle levels. Hence, all respondents were required to evaluate 30 hypothetical bus-automobile scenarios.

The hypothetical modal-choice section was further divided into (a) a category ratings or judgment task, in which respondents estimated on a 1-20 scale the percentage of the time that they would use the automobile to travel to work in each scenario (1 = 0-5 percent, ..., 20 = 95-100 percent, and (b) a choice task, in which respondents were asked to indicate which of 11 possible modes (bus, automobile

Table 1. Attributes, attribute levels, and sample scenarios.

	Automobile Situation			Bus Situation						
Scenario	Parking Costs (¢/h)	Travel Time (min)	Gasoline Cost ($/gal)	Fare (¢)	Travel Time (min)	Walking Distance from Home to Bus Stop (blocks)	Frequency of Service (min)	Walking Distance from Work to Bus Stop (blocks)	Crowding	Gasoline Situation
1	Free	5	0.85	50	27	10	60	10	Standing room only	No waiting, no limit
2	10	15	1.25	30	17	4	30	4	Share seat	No waiting, 7-gal limit
3	25	25	1.75	10	7	1	15	1	Seat by yourself	Thirty-min wait, 7-gal limit
4	25	15	1.75	10	27	1	15	10	Share seat	No waiting, no limit
5	Free	5	1.75	50	27	10	60	1	Seat by yourself	No waiting, no limit
6	10	15	1.75	10	7	1	60	4	Seat by yourself	No waiting, 7-gal limit
7	Free	25	1.75	30	17	4	30	1	Share seat	No waiting, 7-gal limit
8	25	5	1.75	50	17	10	15	10	Standing room only	No waiting, 7-gal limit
9	Free	25	0.85	50	27	1	15	4	Share seat	No waiting, 7-gal limit
10	25	25	1.75	30	7	4	15	10	Seat by yourself	Thirty-min wait, 7-gal limit
11	Free	25	1.25	10	7	10	60	10	Share seat	No waiting, 7-gal limit
12	10	25	1.25	10	17	10	15	1	Standing room only	No waiting, no limit
13	25	15	1.25	50	17	4	30	4	Share seat	No waiting, no limit
14	Free	5	1.25	30	17	1	60	10	Seat by yourself	No waiting, no limit
15	Free	15	0.85	30	17	10	30	4	Standing room only	Thirty-min wait, 7-gal limit

alone, etc.) they would be most likely to use to travel to work or (for university students) to school in each scenario. Respondents were assigned to each of these tasks on an equal probability basis by random number assignment.

Current-Behavior Data

The second section of the survey requested individuals to indicate which mode they had used to travel to work or school that day and how many times (out of 40 possible) in the past month they had used the bus, the car, and other modes. They were then requested to supply information on each of the 10 scenario variables for a typical work or school trip. All respondents supplied this information regardless of the task they completed in the first section.

Administration of Surveys

Virtually identical surveys were administered during April 1979, prior to the large rise in gasoline prices, and in August-September 1979, after the price rise. September surveys were slightly changed in two respects:

1. Gasoline prices were \$0.18, \$0.22, and \$0.26/L (\$0.70, \$0.85, and \$1.00/gal) in survey 1. Because the prevailing actual level was \$0.28/L (\$1.05/gal) by the time survey 2 was administered, they were changed to \$0.22, \$0.33, and \$0.46/L (\$0.85, \$1.25, and \$1.75/gal).
2. Season of the year was found to have little systematic effect in survey 1 and was replaced with a description of the current gasoline situation: (a) no waiting and no limit, (b) no waiting and a 26-L (7-gal) limit, and (c) a 30-min wait and a 26-L limit. (The survey forms themselves expressed all levels in U.S. customary units.) These levels were representative of the range prevailing in Iowa at the time. No other changes were made.

In both surveys, respondents were initially contacted by telephone before they were mailed copies of the survey. In survey 1, of 800 persons contacted in the two cities, 263 usable questionnaires were returned. In survey 2, 1493 persons were contacted, and 516 usable questionnaires were returned.

ANALYSIS

Overview

Analytic interest centers on a comparison between the laboratory-simulation results based on the scenario responses and the revealed-behavior results based on the reported modal-choice behavior of the respondents. This paper focuses entirely on aggregate results; other reports will deal in detail with disaggregate results. The dependent variables of concern are (a) the scenario ratings data, or respondents' estimates of the likelihood of using the automobile; (b) the scenario choice data concerning choice between automobile and bus; and (c) respondents' reports of recent past automobile and bus choices.

All of the aforementioned dependent variables may be regarded as continuous for the purposes of this study. In particular, the ratings data can be converted to "probability" estimates by associating each category with the corresponding midpoint of the relative frequency or percentage-of-time range; thus, 1 = 0-5 percent = 0.025, ..., 20 = 95-100 percent = 0.975. These data refer to the percentage of time that automobile would be chosen. A second, different scale estimate can be obtained by tabulating the relative frequency of responses in categories less than 10, the midpoint of the category scale. In effect, this scale estimates the relative frequency of a response "likely to take other than automobile." We interpret this to mean bus, although we realize that there are some other choices involved. Likewise, the choice data can be separately analyzed by tabulating the relative frequency of choices of any of the automobile-related modes and bus. Thus, there are four dependent variables that can be analyzed in the aggregate for the scenario data--two to represent bus choice and two to represent automobile choice.

Collectively, the four dependent variables observed as the choice outcome of the scenarios have a corresponding dependent variable in the reported choice data. Attention in this study centers on the relative frequency of work or school trips reported by the respondents and the respondents' own reports of the real-world levels of the scenario variables. In fact, these are theoretically parallel sets of data because both dependent variables are assumed to be conditional on the values that individuals believe the attributes to have. In this instance, the relation, if any, with physically measurable attribute levels is inappropriate as a comparison, although it will be of interest in future research. Moreover, this relation can be directly assessed by using the data at hand. Nonetheless, it is important to realize that the scenarios describe the levels that individuals believe attributes to have; respondents must believe the attribute levels to be true because we as investigators tell them they are true. Thus, the appropriate attribute comparisons are with reported or believed attribute levels. Other research will examine the relation between physically observable attribute levels and reported levels (14).

Model Forms

The model forms to be estimated are logit-transformed multiple linear regression equations. It is important to note that each model incorporates attributes of competing modes. Thus, we estimate

$$\ln [R_j/(1 - R_j)] = b_{0j} + \sum_k b_{kj} X_k + \epsilon_j \qquad (1)$$

where

$\ln [R_j/(1 - R_j)]$ = logit of the scenario response transformed to the interval (0,1) for mode j;

b_{0j}, b_{kj} = regression coefficients for the 0th, 1st, ..., kth attributes, X_k; and

ϵ_j = a random disturbance, assumed to conform to the usual assumptions of classical, fixed-effects regression.

Equation 1 involves all linear and squared terms and all two-way interactions of the 10 attributes. The 450 observations divided into 15 subsets of 30 scenarios were specifically designed a priori to permit the independent estimation of all of these terms at the group level, assuming negligible higher-order effects. It is important to realize that it is always possible to know a priori exactly what effects can be estimated from given experimental designs; hence, one can design sets of scenarios to ensure that various terms of interest in the multiple linear regression or analysis-of-variance models can be estimated with known precision at known levels of power. This is, of course, fundamentally different from a typical econometrics analysis, which, practically speaking, can never know which terms are truly estimable, with what precision, or with what power. Thus, it must rely on potentially weak tests based on a priori assumptions about effects. Our approach permits the exact determination of what can and cannot be reliably estimated.

Thus, the number of terms to be estimated in the scenario models will probably seem overwhelming to analysts accustomed to econometric analyses. Yet all such terms are potentially estimable. Our approach, therefore, is to test all of the main and interaction effects noted above for each of the four dependent scenario variables. Our modeling criterion for acceptance of effects is that they are consistently significant; i.e., they are significant at least at the 0.10 level for both surveys for both dependent variables that correspond to a particular mode.

We first estimate models for the scenario data for each survey, for each city; then, based on these results, we estimate parallel models from the corresponding real-world bus and automobile choice data. Our criteria for testing the equality of the models derived in this manner are as follows:

1. Use the 0.05 and 0.01 levels for the standard errors of each coefficient estimated from the choice data reported by respondents (this is obviously better than using the standard errors of the scenario coefficients because the standard errors are all equal and very small by design; since no such precision can be achieved in the respondent-reported data, the appropriate criterion for the comparison should be based on the respondent-reported data estimates) and
2. Test whether the sum of squares for regression given by the model estimated from the reported choice data is significantly different from that for the model by using fixed regression weights derived from the simulation experiments [the test statistic is the F-value given by mean square (improvement in sum of squares) ÷ mean square (residual for reported choice data model); the degrees of freedom are equal to the difference in the number of parameters for improvement for the numerator and N, the number of coefficients estimated for residual].

Additional considerations that are important to note concern differences in the two sets of models caused by additional terms in the respondent-reported data that are not included in the scenario data. In particular, because the scenario data are aggregated over individuals, individual differences caused by factors such as income, automobile availability, and age are, in effect, averaged out. This is exactly true in the scenario results because each individual has a constant value for these factors within his or her 30 responses. Thus, there can be no correlation between scenario attributes and interpersonal factors, and we can legitimately ignore such factors in estimation. That is not to say that there are no effects attributable to these covariates but only that such effects cannot affect the estimation of aggregate coefficients in the controlled, experimental data.

However, in the case of the respondent-reported choice data, we cannot ignore the effects of interpersonal factors because they can have significant effects on the parameter estimates of the 10 attributes of interest. Thus, to minimize this source of potential bias, we include a number of interpersonal factors as terms in the respondent-reported choice models. This is accomplished in the following manner:

1. All main effects of attributes and interpersonal factors are tested in a multiple linear regression analysis. Nonsignificant interpersonal factors are dropped from further consideration.
2. All two-way interactions of the attributes and the remaining interpersonal factors are tested as follows. The main effects of attributes, significant attribute interactions uncovered in the controlled scenario data, and previously significant interpersonal factors are forced into a multiple linear regression equation, and the remaining two-way, attribute/interpersonal-factor interactions are tested by stepwise regression methods. The acceptance criterion is set at the 0.10 level.

The latter procedure will undoubtedly be objectionable to some, but it is strictly a matter of convenience. We are not interested in these estimates per se; rather, we wish to try and minimize as many sources of bias on the attribute parameters as possible. That is not to say that such effects are not important but only that interest in this analysis centers entirely on the 10 attributes, aggregated across respondents. Future analyses will examine interpersonal effects in the scenario data at the individual-respondent level. They are not of interest, however, in this paper.

RESULTS

Overview

There are a number of results of interest that involve a large number of parameters. In order to reduce the tabular material, standard errors and statistical tests are not reported for the scenario data. This is because the scenario conditions are controlled, which also fixes the standard errors. Virtually all t-values are significant in these data because of the power of the tests (450 df) and the precision of the estimates (complete details are available from the authors on request). The respondent-reported data, however, are tabulated with standard errors because of the test comparisons.

Simulation Results

Detailed Analyses

Table 2 gives the most detailed aggregate results available by survey (1 or 2), by city (Iowa City or Cedar Rapids), and by response measures for the scenario data. The response measures can be defined as follows:

RATE = 1-20 category ratings scale transformed to (0,1) interval,
R<10 = relative frequency of category ratings less than 10 on the 1-20 scale,
CHAUTO = relative frequency of choices of automobile in each scenario, and
CHBUS = relative frequency of choices of bus in each scenario.

The results given in Table 2 suggest the following.

RATE

The only significant ratings difference in the Iowa City data is in the crowding attribute. Survey 1 reveals a larger impact for crowding (-0.166) than survey 2 (-0.092). The timing of the surveys was such that crowding was considerably greater during survey 1. For Cedar Rapids, there are differences in the two walking-distance variables and crowding,

Table 2. Detailed model results for scenario data.

Variable	RATE		R < 10		CHAUTO		CHBUS	
	Survey 2	Survey 1	Survey 2	Survey 1	Survey 2	Survey 1	Survey 2	Survey 1
Iowa City								
Parking cost	-0.020 27	-0.023 70	0.031 05	0.037 36	-0.052 16	-0.075 73	0.027 97	0.034 57
Automobile travel time	-0.028 61	-0.018 70	0.039 66	0.041 223	-0.030 78	-0.011 41	0.055 23	0.047 06
Gasoline cost	-0.004 16	-0.003 15	0.006 69	-0.002 96	-0.009 57	-0.007 03	0.003 92	-0.007 67
Bus fare	0.005 86	0.006 64	-0.011 88	-0.011 00	0.003 35	0.005 98	-0.017 59	-0.019 33
Bus travel time	0.020 27	0.017 94	-0.029 39	-0.036 07	0.024 00	0.028 07	-0.033 08	-0.029 02
Walk from home	0.093 53	0.074 74	-0.143 22	-0.139 72	0.079 67	0.081 77	-0.205 12	-0.179 66
Bus frequency	0.008 51	0.007 93	-0.012 72	-0.009 63	0.001 83	0.009 45	-0.012 81	-0.025 62
Walk from work	0.088 28	0.065 95	-0.148 22	-0.103 07	0.006 50	0.095 33	-0.191 75	-0.183 68
Bus crowding	-0.092 12	-0.166 36	0.136 13	0.295 94	-0.053 66	-0.001 44	0.191 72	0.308 70
Season		-0.051 97		0.023 41		0.403 79		0.149 51
Gasoline situation	-0.172 56		0.220 80		-0.312 07		0.243 45	
Quadratic								
Parking cost	0.000 68	0.001 16	-0.000 05	-0.001 58	0.001 62	0.002 31	-0.000 84	0.000 24
Automobile travel time	0.000 48	0.000 05	0.001 00	0.001 22	-0.000 74	-0.000 36	0.000 68	0.000 94
Gasoline cost	0.000 07	0.000 10	-0.000 06	-0.000 01	0.000 06	-0.000 20	-0.000 03	0.000 61
Walk from home	0.001 77	0.003 77	-0.005 84	-0.001 27	-0.003 29	0.000 03	-0.005 03	0.000 18
Walk from work	0.006 59	0.005 88	-0.014 33	-0.002 93	-0.000 89	-0.002 65	-0.012 45	-0.004 74
Bus crowding	0.055 95	0.045 19	0.083 80	-0.016 35	-0.047 20	0.061 29	0.019 17	-0.072 62
Season		0.034 02		0.118 92		0.183 59		0.025 16
Gasoline situation	-0.101 02		0.131 96		-0.047 47		0.021 98	
Intercept	0.711 15	0.690 26	-1.391 31	-1.345 156	-1.512 474	-1.392 93	-1.503 388	-2.022 951
R^2	0.63	0.42	0.50	0.36	0.49	0.32	0.69	0.58
Cedar Rapids								
Parking cost	-0.014 84	-0.018 15	0.018 73	0.024 12	-0.043 93	-0.066 16	0.017 41	0.019 85
Automobile travel time	-0.021 228	-0.019 27	0.024 57	0.026 28	-0.024 33	-0.013 07	0.045 89	0.058 84
Gasoline cost	-0.005 118	-0.007 75	0.004 86	0.010 32	-0.013 47	-0.025 52	0.009 56	0.022 70
Bus fare	0.005 92	0.003 10	-0.010 69	-0.002 92	0.005 13	0.014 21	-0.008 05	-0.017 54
Bus travel time	0.018 91	0.018 29	-0.028 07	-0.031 91	0.013 61	0.031 83	-0.030 32	0.039 75
Walk from home	0.084 38	0.133 13	-0.137 06	-0.181 28	0.056 76	0.086 31	-0.182 46	0.162 43
Bus frequency	0.008 58	0.010 61	-0.013 71	-0.010 96	0.008 95	0.011 46	-0.023 15	-0.019 64
Walk from work	0.008 76	0.122 67	-0.135 79	-0.149 77	0.040 82	0.125 89	-0.158 09	-0.211 07
Bus crowding	-0.109 37	-0.143 76	0.198 86	0.177 29	-0.091 00	-0.122 97	0.090 75	0.394 01
Season		0.149 90		-0.134 74		-0.007 72		0.087 98
Gasoline situation	-0.159 40		0.191 75		-0.409 65		0.234 39	
Quadratic								
Parking cost	0.000 24	-0.000 27	-0.000 11	0.001 09	0.001 15	0.002 97	-0.000 45	-0.001 43
Automobile travel time	0.000 58	0.000 40	-0.001 39	-0.000 52	0.000 33	-0.003 39	-0.000 10	0.001 27
Gasoline cost	0.000 02	-0.000 06	-0.000 02	0.000 43	0.000 08	0.000 14	-0.000 07	0.000 51
Walk from home	0.005	0.004 12	-0.003 46	0.000 93	0.003 94	0.000 27	-0.007 61	-0.023 91
Walk from work	0.005 65	0.013 62	-0.009 70	-0.007 84	0.002 63	0.003 92	-0.008 59	-0.001 30
Bus crowding	0.027 83	0.123 79	0.028 38	-0.317 35	-0.176 91	0.131 92	-0.157 43	-0.211 58
Season		0.039 13		0.013 02		-0.002 43		0.063 21
Gasoline situation	-0.076 02		0.095 33		-0.154 32		0.114 32	
Intercept	0.723 168 8	0.997 03	-1.435 13	-1.705 44	-1.006 39	-0.947 684	-1.540 473	-1.930 946
R^2	0.74	0.54	0.56	0.40	0.58	0.33	0.70	0.53

all indicating the same thing--namely, less impact in survey 2. Again, the timing of the first survey was at the end of the winter peak season, whereas the second survey was near summer's end. There is a suggestion, therefore, that the weights change in response to seasonal differences.

There is an apparent difference in the intercepts between the two cities and the two surveys. The scenario attributes were all centered about their respective means; thus, the intercept can be directly interpreted as the likelihood of using the automobile given that all scenario variables are at their average level. The apparent difference is in the Cedar Rapids data between surveys 1 and 2. It appears that the likelihood of using the automobile dropped from survey 1 to survey 2. This drop coincides with the dramatic increase in gasoline prices. No such drop is evident in the Iowa City data, but levels of bus ridership were already quite high in that city.

It would be difficult to conclude that there are major coefficient differences between Iowa City and Cedar Rapids on the ratings-scale data.

CHAUTO

The automobile choice data reveal a pattern similar to that of the ratings data. Differences between cities and surveys are apparent in walking distances and crowding and in the intercepts. The interpretation of these differences would be similar to the interpretation for the ratings data.

R<10

The R<10 results largely parallel those of the previous response measures but with opposite signs. There are small differences in the walking-distance, crowding, and intercept terms between cities and larger differences between surveys within cities.

CHBUS

The CHBUS results are similar to the results for the preceding response measures. There are minor differences between cities and larger differences within cities between surveys with respect to the walking and crowding attributes and the intercept.

Aggregation Across Cities

The detailed scenario results lead us to conclude that the major differences between cities are in (a) the intercept term (on the average, Iowa City respondents are more likely to choose the bus than Cedar Rapids residents) and (b) gasoline availability and season (Iowa City residents are less sensitive to availability and respond differently to season). Differences between surveys are evident in the walking-distance and crowding attributes: The effect of these variables decreased from survey 1 to survey 2. There were also intercept differences, which indicates that, all things being equal, respondents became more positive toward bus and more negative toward the automobile between surveys 1 and 2.

The data were aggregated over cities because city differences were minor in comparison with survey differences. These aggregated results are given in Table 3, where the results of the different response measures are listed and the averages are over the bus (CHBUS and R<10) and automobile (CHAUTO and RATE) models, respectively. The season and gasoline-situation attributes are not comparable, except within a survey.

Table 3 reveals that within surveys there are major differences in the intercepts, which is to be expected because, for each response measure, the origin of each scale can be different, even if the units are comparable. Major differences in survey 1 within the two automobile responses (RATE and CHAUTO) are in the intercept, parking costs, and season. The ratings data yield a lower estimate for the effect of parking costs than the choice data; the seasonal effects are also totally different. The major differences in the bus responses (R<10 and CHBUS) are in the intercepts, walking-distance effects, and season; the choice data indicate larger effects for these attributes than do the ratings data.

In survey 2, there are major differences in the automobile response measures in the intercepts, parking cost, walking distance, crowding, and gasoline situation. The automobile choice data generally display a lower likelihood of choosing automobile, more sensitivity to automobile parking costs, less sensitivity to walking distance, less sensitivity to crowding, and considerably more sensitivity to the gasoline situation than do the ratings data.

The bus choice data show differences in intercepts, revealing lower average probabilities and more sensitivity to automobile travel times and walking distances. Hence, the bus responses are much more homogeneous than the automobile data. Nonetheless, it should be noted that, for the most part, only the intercepts exhibit dramatic differences, particularly when one considers that the attribute of gasoline situation and availability was constant in the real world during both surveys and that only crowding and gasoline costs actually changed between the two surveys. Of course, it is possible that perceptions of the onerousness of walking distances change from winter to summer in Iowa.

Examination of the average coefficients suggests that there may be less difference between surveys than previous, more disaggregate results have suggested. In particular, there does appear to be a consistent change in the intercept that reflects a definite change upward for bus and downward for automobile. Otherwise, only the crowding attribute appears to be very different, and only for bus, showing a lower effect in survey 2, when there is in fact less crowding. This result and the previous, more disaggregate results suggest that some of the coefficients may depend on the present situation of the respondent as well as on the levels of the experimental variables.

COMPARISON WITH ECONOMETRIC-TYPE RESULTS ON CHOICE DATA REPORTED BY RESPONDENTS

Disaggregate Results for Both Cities and Surveys

Table 4 gives the results for models fit to each choice variable, by city and survey, including only the nine scenario attributes (season and gasoline availability are constant in these data), the one quadratic term that consistently appeared in the scenario results, and three interactions that also consistently appeared: (a) gasoline cost and bus travel time, (b) walking distance to the bus stop from home and from the bus stop to work, and (c) walking distance from home to the bus stop and crowding.

The results reveal little in the way of consistency except in the case of bus travel time, which is significant in three of four tests in Iowa City, and the two walking-distance variables in Cedar Rapids. Of course, the intercepts are significant as well, revealing more positive bus probabilities and lower automobile probabilities in Iowa City, which is to be expected because of the high levels of transit patronage in Iowa City. The disturbing aspect of these results is that one would probably conclude that few of the attributes have any significant effects, but, as we shall see, the coefficients are fairly close to those estimated from the scenario data, almost all of which are highly significant. Of course, this could result from bias introduced by not including interpersonal factors in the model. To pursue this possibility, data for both cities were combined, and interpersonal factors were investigated.

Table 3. Model results for scenario data averaged over cities.

	Survey 1				Survey 2			
Variable	R < 10	RATE	CHAUTO	CHBUS	R < 10	RATE	CHAUTO	CHBUS
Intercept	-1.148 06	0.677 94	-0.939 03	-1.658 585	-1.251 12	0.711 998	-1.126 796 4	-1.419 782
Parking cost	0.028 70	-0.016 91	-0.057 65	0.030 38	0.021 28	-0.021 64	-0.044 31	0.025 90
Automobile travel time	0.028 55	-0.023 38	-0.014 95	0.054 54	0.029 968	-0.015 34	-0.024 17	0.046 51
Gasoline cost	0.005 52	-0.004 72	-0.012 68	0.006 95	0.005 94	-0.005 10	-0.011 24	0.005 78
Bus fare	-0.007 70	0.005 78	0.007 73	-0.015 75	-0.009 49	0.004 82	0.004 85	-0.011 59
Bus travel time	-0.025 35	0.018 90	0.028 97	-0.037 84	-0.026 27	0.015 17	0.015 99	-0.028 02
Walk from home	-0.128 85	0.082 80	0.082 92	-0.169 81	-0.124 09	0.087 44	0.062 40	-0.178 53
Bus frequency	-0.009 06	0.008 52	0.009 80	-0.022 28	-0.012 16	0.008 90	0.006 03	-0.016 46
Walk from work	-0.105 54	0.082 02	0.095 73	-0.181 71	-0.125 86	0.080 22	0.042 40	-0.151 80
Bus crowding	0.197 42	-0.099 61	-0.084 22	0.273 00	0.140 67	-0.145 63	-0.068 16	0.122 05
Season	-0.015 09	-0.158 85	0.160 57	0.085 74				
Gasoline situation					0.239 83	0.023 98	-0.338 92	0.268 67
R^2	0.55	0.79	0.62	0.71	0.67	0.66	0.72	0.79

Table 4. Model results for real-world data.

	Iowa City				Cedar Rapids			
	CHBUS		CHAUTO		CHBUS		CHAUTO	
Variable	Survey 1	Survey 2	Survey 1	Survey 2	Survey 1	Survey 2	Survey 1	Survey 2
Parking cost	0.024 87	0.065 42[a]	-0.054 72	-0.056 15[b]	0.008 70	0.081 37[a]	-0.065 55	-0.106 98[a]
Automobile travel time	0.005 11	0.030 14	0.045 86	-0.088 62[a]	-0.003 93	-0.006 22	-0.022 02	-0.010 56
Gasoline cost	0.012 34	-0.045 78	-0.056 00	-0.040 94	0.014 81	0.026 37	-0.031 773	-0.008 68
Bus fare	-0.000 06	0.033 54	-0.053 84	-0.016 60	0.004 29	-0.002 53	-0.008 46	-0.055 79[c]
Bus travel time	-0.000 40[a]	0.001 86	0.057 76[a]	0.053 67[a]	-0.006 69	-0.002 21	0.028 57[c]	0.010 29
Walk from home	-0.306 13	-0.173 52	0.258 59	0.125 90	-0.112 70[b]	-0.166 31[a]	0.131 05	0.182 69[b]
Bus frequency	-0.004 84[b]	-0.007 80	0.046 13[b]	0.009 81	0.008 21[c]	-0.004 74	-0.011 14	0.026 51[c]
Walk from work	-0.416 15	-0.450 97[a]	0.156 94	0.288 91	-0.150 59[c]	-0.136 31[a]	0.318 71[b]	0.350 64[a]
Bus crowding	-0.069 31	0.452 83[b]	-0.006 40	-0.740 35[c]	-0.051 84	0.016 19	-0.875 70[b]	-0.474 85
Quadratic (walk from work)	0.089 68	-0.074 87	0.047 02	0.089 20	-0.148 46[a]	-0.040 14	0.052 40	-0.025 68
Gasoline cost x bus travel time	-0.002 03	0.003 76	0.005 21	-0.004 87	-0.000 228	-0.001 27	0.001 96	0.004 74[c]
Walk from home x walk from work	0.139 095	-0.031 00	-0.161 74	-0.009 19	0.058 46[b]	0.037 35	0.138 09	-0.001 15
Walk from work x crowding	-0.109 92	-0.337 73	-0.492 32	-0.333 46	-0.103 15	-0.203 49[c]	-0.474 52	0.047 65
Intercept	-3.058 46[a]	-2.471 96[a]	-1.134 87[a]	-1.412 162[a]	-4.006 18	-3.773 63	1.534 817	0.930 82
R^2	0.13	0.20	0.28	0.18	0.20	0.21	0.16	0.14

[a]Significance level = 0.01. [b]Significance level = 0.10. [c]Significance level = 0.05.

Table 5. Model results for real-world data averaged over cities.

	CHBUS				CHAUTO			
	Survey 1		Survey 2		Survey 1		Survey 2	
Variable	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error	Coefficient	Standard Error
Parking cost	0.022 56[a]	0.016 0	0.068 254	0.014 09	-0.066 29[b]	0.028 17	-0.096 71[b]	0.022 82
Automobile travel time	-0.004 41	0.008 55	0.011 357	0.011 225	-0.000 475	0.015 08	-0.036 03[c]	0.018 18
Gasoline cost	0.008 19	0.019 04	0.007 24	0.023 91	-0.031 64	0.033 59	-0.027 66	0.038 73
Bus fare	-0.007 77	0.019 33	-0.004 83	0.013 74	0.001 29	0.034 10	-0.023 96	0.022 26
Bus travel time	-0.002 986	0.006 02	-0.003 27	0.005 13	0.032 01[c]	0.010 62	0.021 07[c]	0.008 30
Walk from home	-0.183 58[b]	0.068 45	-0.190 355[b]	0.052 597	0.195 51[a]	0.120 73	0.193 03[c]	0.085 20
Bus frequency	0.008 12	0.005 35	-0.005 22	0.003 31	-0.000 43	0.009 43	0.012 98[c]	0.005 36
Walk from work	-0.160 82	0.067 88	-0.227 30[b]	0.050 44	0.173 76	0.119 72	0.333 77[b]	0.081 70
Bus crowding	-0.123 81	0.176 72	0.175 19	0.141 92	-0.151 73	0.311 68	-0.491 74[c]	0.229 88
Quadratic (walk from work)	-0.007 26	0.056 61	-0.059 17	0.042 22	0.063 76	0.099 84	0.085 18	0.068 38
Gasoline cost x bus travel time	-0.000 48	0.001 02	-0.000 215	0.001 07	0.001 137	0.001 79	0.002 31	0.001 74
Walk from home x walk from work	0.095 00	0.041 01	0.033 40	0.030 36	0.011 727	0.072 34	0.027 709	0.049 17
Walk from home x crowding	-0.057 98	0.137 58	-0.210 84	0.094 83	-0.389 91[a]	0.242 66	-0.055 81	0.153 60
Vehicle availability	-0.835 34[b]	0.321 43	-1.217 78[b]	0.257 26	3.453 12[b]	0.566 91	2.291 88[b]	0.416 70
Residence	0.371 36	0.393 01	0.004 69	0.246 5	-0.605 86	0.693 17	0.495 97	0.399 23
Number of family members	0.049 90	0.101 09	0.061 80	0.096 10	-0.203 19	0.178 29	-0.183 18	0.155 7
Number of children								
Under age 6	-0.060 86	0.202 76	-0.147 60	0.181 69	0.497 13	0.357 61	0.540 31[a]	0.294 29
Aged 6-14	-0.108 95	0.170 00	-0.021 41	0.146 49	0.463 92	0.299 68	0.310 03	0.237 28
Number of workers	0.158 03	0.079 25	-0.088 46	0.097 20	0.101 55	0.139 78	-0.169 63	0.157 45
Number of students	-0.005 85	0.104 33	0.003 36	0.132 32	-0.071 11	0.184 00	-0.231 53	0.214 32
Sex	0.137 74	0.195 77	-0.231 47	0.166 94	-0.669 30[a]	0.345 28	-0.099 62	0.270 40
Age	0.005 50	0.007 92	-0.003 65	0.006 97	0.009 26	0.013 97	0.050 74[b]	0.011 29
Bus rating	0.125 38	0.094 84	0.167 25[c]	0.072 04	-0.132 82	0.167 28	-0.064 10	0.116 68
Education	0.141 42	0.091 77	0.205 08[c]	0.098 83	-0.239 40	0.161 85	-0.118 94	0.160 08
Income	-0.158 52[b]	0.049 85	-0.013 79	0.040 05	0.173 67[c]	0.087 9	0.076 60	0.064 87
Intercept	-3.630 918	0.119 92	-3.130 278 2	0.092 030 1	0.175 473 5	0.211 512	-0.140 783	0.149 067

[a]Significance level = 0.01. [b]Significance level = 0.10. [c]Significance level = 0.05.

These results, given in Table 5, are better but still disturbing. Parking costs emerge as significant in both surveys for both modes. Bus travel time is highly significant in automobile mode choice but very marginal in bus mode choice; walking distance from home to the bus stop is consistently significant; frequency of bus service is probably significant, although survey 1 results have the wrong sign for bus mode choice; and walking distance from the bus to work is also consistently significant. Similarly, the intercepts have the same change in favor of bus as previously observed. Among interpersonal factors, vehicle availability is consistently highly significant with the appropriate sign; of the remaining factors, only income displays any consistent trend and, if we are to believe the data, the results suggest that its effect declines from survey 1 to survey 2. If true, this suggests that the increase in gasoline price manifests itself, inter alia, in a shift in bus probabilities across all income groups and has more impact on upper-income than on lower-income earners. It might be speculated that lower-income groups already had fairly high (relative) probabilities of bus use and that the effect of the price increase was to force some individuals in the higher-income groups to give the bus serious consideration.

The respondent-reported choice-data results are still disturbing despite the emergence of a few more consistently significant terms. This is because one would hope that effects found to be significant in the scenario data would also emerge as significant in the reported choice data. A comparison of Table 5 with Table 3, however, reveals some interesting similarities and differences. In general, in survey 1, for the automobile choice model in which interpersonal factors are included, all of the scenario coefficients are within the 95 percent confidence level of the respondent-reported estimates! For survey 2, there are only two coefficients outside the 95 percent confidence band: parking cost and walking distance to work from the bus. These coefficients are only slightly outside the 99 percent limits--again, an encouraging result for the scenario data.

In the case of bus choice data for survey 1, only the coefficients for automobile travel time and frequency of bus service are outside the 95 percent confidence limits of the reported choice data. Both of these attributes, however, have the wrong sign in the reported choice data. In survey 2, several coefficients lie outside the 95 percent confidence interval: parking cost, automobile travel time, bus travel time, and frequency of bus service. Automobile travel time is within the 99 percent confidence band; the others are generally just outside the 0.99 interval.

On the basis of the results thus far, we cannot reject the hypothesis that the models based on the average scenario coefficients are the same as the models based on the respondent-reported choice data. Because the hypothetical choice tasks in the surveys most closely parallel the respondent-reported choice data, we also examine these results for comparability.

For automobile choice data from survey 2, only walking distance from the bus to work lies outside both the 95 and 99 percent bands; in addition, parking cost lies slightly outside the 95 percent level. The intercept term is dramatically different, however. In survey 1, no coefficients lie outside the 95 percent confidence band except the intercept term, which is again very different.

For bus choice data from survey 2, three attributes lie outside the 95 percent bounds: parking cost, automobile travel time, and frequency of bus service. Of course, the intercept is very different. For bus choice data from survey 1, two attributes--automobile travel time and frequency of service--lie outside both the 95 and 99 percent limits; crowding is within the 99 percent bounds.

Once again, we find the evidence insufficient to reject the hypothesis that the models are the same. It should be noted that all previous results included missing data in the respondent-reported data by replacing the missing values with their respective means. As a final investigation, we examine the effects of removing those respondents who could not estimate walking distances--a likely source of error throughout the data because these individuals are likely to be ignorant of other variables as well. We also combine data from both cities for both surveys in order to gain degrees of freedom. The models using respondent-reported choice data were estimated by forcing all attributes and interpersonal factors as main effects and stepping any other significant attribute-by-interpersonal-factor interactions into the model. The stepwise criterion was set at 0.10.

Let us consider the average coefficient results for the bus choice data first: Parking cost, bus travel time, and the two walking-distance attributes are beyond the 99 percent confidence limits, whereas remaining attributes are within the limits. If we use the average coefficients of the bus choice data from surveys 1 and 2, we find that the same four attributes lie outside the range. In the case of the automobile choice data, the averages all lie within the 99 percent confidence limits. Averaging the CHBUS coefficients yields the same result. It appears that the automobile choice model is very well estimated but the bus choice model is less so. One can speculate that this is because the bus data, except for the choice task in the surveys, are less well defined; for example, the ratings data are for automobile choice, not bus. We must assume the residual to be bus, although there are other modes in the data.

The second test examined the relative predictive abilities of simulation and real-world models. The simulation models were related to actual behavior through a regression equation that included (a) the simulation-derived utility argument, averaged over surveys and cities, and (b) the same socioeconomic covariates used in the final real-world model. The simulation-behavior model, therefore, had an additional slope parameter associated with the entire simulation-derived utility model.

Results, as expected, suggested similar predictive abilities. In particular, the R^2s (adjusted for degrees of freedom) for the laboratory and real-world bus models were 0.15 and 0.19, respectively. Likewise, the adjusted R^2s for the laboratory and real-world automobile models were 0.30 and 0.31, respectively. The F-test, described earlier, indicated that the simulation and revealed-behavior automobile choice models were not significantly different from each other (F-value of 1.56 with 9 and 529 df). Conversely, the bus choice models were found to be significantly different (F-value of 4.04 with 9 and 597 df).

It might be added that the above predictive levels, although low, are not out of line with those usually reported when predicting individual behavior from aggregate demand models (15,16). Increased predictive ability could have been achieved, however, through complete disaggregation, which is possible only with the scenario data.

DISCUSSION OF RESULTS

This paper reports the results of a comparison of two methods for modeling travel-choice behavior: laboratory-simulation and revealed-behavior modeling. The results provide additional evidence that laboratory-simulation models are a potentially valuable tool for understanding and predicting individual reactions to travel alternatives. In particular, the findings showed that laboratory-derived models performed about as well as models based on revealed behavior in terms of predictive ability and that much stronger inferences can be drawn from such models about the likely effects (precision of estimates) of changes in transportation system variables on mode choice.

The predictive ability and parameter temporal and spatial stability of laboratory and revealed-behavior models were compared. The results suggest that the laboratory-derived models are strongest in terms of the diagnosis of important explanatory variables. This strength, of course, is inherent in the approach: Not only does the analyst obtain multiple observations for a single individual, but these observations are also purposefully designed a priori so as to maximize orthogonality among variables and to ensure precision of their estimates. In contrast, models based on revealed behavior cannot achieve such conditions except by a most unlikely accident. Furthermore, with traditional econometric methods, the analyst usually has merely one observation per individual, which further limits the ability to generalize. Thus, much larger samples are required to achieve the inferential power of the laboratory methods. Such limitations, of course, are especially troublesome in trying to draw inferences regarding interpersonal differences. The laboratory methods achieve greater power from smaller samples because it is possible to observe distributions of coefficients over samples of individuals. Clearly, these coefficients can be related to interpersonal measures so as to ensure a much stronger test for individual differences (7,17).

The results of this study suggest a potential danger in basing models on revealed-behavior data: The effects of two major policy variables--bus fare and gasoline price--were found to be not significantly different from zero in both the automobile and bus models. Hence, a policymaker confronted with these results might conclude that any changes in either bus fares or gasoline prices, or both, would be likely to have little effect on travel-choice behavior. Yet, in reality, this suggestion would be grossly misleading. The laboratory models produced coefficients that were virtually the same as those estimated by the revealed-behavior models; moreover, the laboratory results clearly revealed that the effects of fare and gasoline price were highly significant. In the revealed-behavior data, there was an insufficient range of variation in the observations on fare and price values to permit reliable inferences to be drawn. Thus, the results of the revealed-behavior models would completely mislead a policymaker regarding the underlying determinants of modal-choice behavior. It is therefore conceivable that such models might also lead to the formation of incorrect transportation policy decisions. Such problems could be avoided, of course, if laboratory-simulation methods were made an integral part of the analyst's bag of tools.

In terms of the spatial and temporal transferability of models, both methods appeared to be similarly robust. Indeed, the fact that both types of models were reasonably stable in relation to time was an important result. This implies that travel demand models are not necessarily purely descriptive. For example, the results suggest that reasonable forecasts of choice behavior after the major 1979 gasoline price rise could have been made based on the pre-price-rise models.

Despite this optimism, however, there were major changes between surveys: (a) a more favorable disposition toward bus (as inferred by changes in the intercepts) and (b) a uniform decrease in the effect of "bus crowding".

The more favorable disposition toward the bus between surveys is to be expected. Specifically, this might be traced to the fact that there was a gasoline price increase of some 7¢/L (25¢/gal) between survey 1 and survey 2 in the real world. One would expect this change to be associated with an increase in the mean probability of taking the bus. The decrease in the effect of crowding observed between surveys in the bus choice models is probably attributable to seasonal changes in actual bus conditions. Crowding peaked in the winter months, when survey 1 was conducted, and reached a low point during summer, when survey 2 was conducted. This implies that individuals' reactions to system attributes may depend on their context at the time. This suggestion would manifest itself in different coefficients during different seasons. This result should hold for both econometric and laboratory simulation methods. If true, it suggests that greater attention needs to be paid to contextual differences as they affect choices. This would require much more attention to joint longitudinal/cross-sectional studies, especially those involving multiple study sites.

The final point of comparison between models based on revealed behavior and those based on laboratory simulation was overall predictive ability. In this regard, the methods were comparable. It is important to note, however, that considerably improved predictive ability could be achieved by using the totally disaggregate, individual equations--that is, by using separate modal-choice models for each individual in the sample. Such total disaggregation, of course, would be impossible with the revealed-behavior approach. Given revealed-behavior data, the most an analyst can do is to include socioeconomic variables in the model in the hope that some (of the many) individual differences can be inferred.

CONCLUSIONS

Transportation planners increasingly have to develop policies regarding transportation system scenarios that often have no recent precedent in the real world. The need for accurate forecasting models to include unprecedented conditions is obviously important. But, despite some 20 years of active research, our modeling technology still fails to adequately meet this need. Although econometric models have become increasingly complex, they still cannot deal adequately with new technology or futures very different from the historic past.

Laboratory-simulation methods have been proposed to help overcome some of the limitations of current econometric models. In particular, laboratory simulation would appear to offer an efficient means by which an analyst could explicitly model behavior under a wide range of present and future transportation scenarios and do so at a completely disaggregate level.

We regard it as unfortunate that, despite five years of highly successful validity tests, simulation methods remain generally unaccepted and are forced to take a back seat to more traditional econometric methods. Although paradigms are slow to change (18), it is hard to understand the resistance to methods that have a good record in numerous validity tests over an extended period of time. Simulation models are at least as accurate as revealed-behavior models, offer greater flexibility in both data collection and analysis, and allow stronger model tests.

This paper has reported the results of a study in which laboratory-derived models of modal choice were shown to yield parameters and predictions comparable to those derived by using more conventional revealed-preference methods. The generality of these results can only be assessed through replication, but it is hoped that the results and the discussion will serve to attract more attention to laboratory-simulation methods as a complementary (and alternative) approach to existing methods of travel-choice analysis.

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Evaluation by Individuals of Their Travel Time to Work

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Modelers of transportation-related decisions have often drawn the distinction between "objective" measures of attributes used to describe the transportation system and individuals' perception and evaluation of these attributes. Only a few studies have been made, however, of the relation between these objective and subjective assessments. Individuals' satisfaction with the length of the work trip is examined, primarily with the aim of establishing the nature of the relation and its stability across different groups of travelers. The study is based on data collected in a home interview survey of residential location choice conducted in outer suburban Melbourne during 1978 and 1979. A number of broader issues are addressed, including implications for modeling and policy.

The ease with which people can participate in activities is influenced by the transportation system. A good transportation system may entice people to partake in certain activities, whereas a poor system may discourage such involvement. However, to ascertain what is a good or bad transportation system, it is necessary to investigate both objective and subjective measures of effectiveness. It may be that one individual views the separation between two activities in a much different light than another. Handicapped people, for example, are likely to view a trip to the corner shop as much more onerous than a neighbor who can walk without difficulty.

Transportation planners have often developed models of transportation choice or measures of accessibility that have assumed that individuals view the transportation system in the same manner. Car drivers are assumed to have the same satisfaction with a travel time of 10 min as those traveling by public transportation. Males and females are similarly assumed to have similar satisfactions with travel time. Yet these people experience quite different conditions and constraints. Moreover, most such models are calibrated by using data on existing travel patterns. This approach suffers from a major flaw--that all people clearly do not have the same sets of choices. Alternative choices must be built into the analytic procedure for evaluating spatial patterns before we can state firmly the nature of the relation (i.e., the shape of the curve) between satisfaction and journey length.

This paper explores individuals' perceived satisfaction with the length of the work trip. The primary aim is to establish the nature of the relation and its stability across different groups of travelers.

ATTRIBUTE EVALUATION

Evaluating attribute levels entails a number of steps (see Figure 1) (1):

1. Individuals must first have some estimate of the magnitude of the attribute in question (in this case, the length of the work trip). The relation between the actual length of journeys and travelers'