NOTES ON THE METHODOLOGY OF URBAN TRANSPORTATION IMPACT ANALYSIS

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The introduction of a large-scale transportation improvement, such as the Bay Area Rapid Transit system, into the urban system may be viewed as an experiment to determine the effect of service characteristics on travel patterns, location of activities, and related variables. Examples of such service characteristics are travel time and cost, comfort, and convenience. If one considers the available scientific knowledge on these impliedrelationships, such experiments are indeed warranted.

If the improvement is viewed as an experiment, however, a major question arises that is all too familiar to social and other nonexperimental sciences. This methodological question is (a) how to provide for experimental control and (b) how to obtain replications of the experiment, that is, repeated observations on the effect of the experiment on randomly selected subjects. The detection of the effect of any experiment that is without control and replication is seriously in doubt. In these brief notes I discuss why previous impact studies have been inadequate from this point of view and present some ideas on how experimental control and replication can be obtained.

SHORTCOMINGS OF PREVIOUS STUDIES

Hundreds, perhaps thousands, of urban transportation impact studies have been undertaken to date. Most of these have been the before-and-after study variety. Despite these major efforts, our detailed scientific knowledge of the effects of an urban transportation facility is extremely limited. Few generalizations have resulted, and our ability to predict impact remains essentially undeveloped. There are many reasons for this finding, some of which are inherent in the before-and-after study approach. The procedure of such studies is (a) to record the values of pertinent variables, such as land value, construction value, travel volumes, and speeds, for a period of time prior to the introduction of the facility; (b) to record the values of the same variables for a comparable time period following the introduction of the facility; and (c) to compare the 2 sets of values.

Experimental Control

There are 2 major deficiencies of such an approach. First, the before-and-after values are not strictly comparable. Regardless of the introduction of the facility, the variables are likely to change during the elapsed time period. This deficiency is overcome by forecasting the values of the variables of interest without the improvement (Fig. 1) and comparing these forecasts with the observed values after the improvement. In other words, direct comparison of the values from the before studies with the after studies is invalid; the proper comparison to make is between forecast values based on no improvement and observed values resulting from the improvement.

Another method for achieving comparability in the experimental sciences is to introduce one or more control observations. A control is an observation on which no experiment is performed. The use of a control is shown in Figure 2. In this case the comparability of the observation on the improvement and the control observation is established in the before period. After the improvement is initiated, the 2 are again compared in the after period.

Replication

Even if comparability of the before-and-after period is achieved, a second question remains as to what constitutes a real difference or change. Are the values recorded

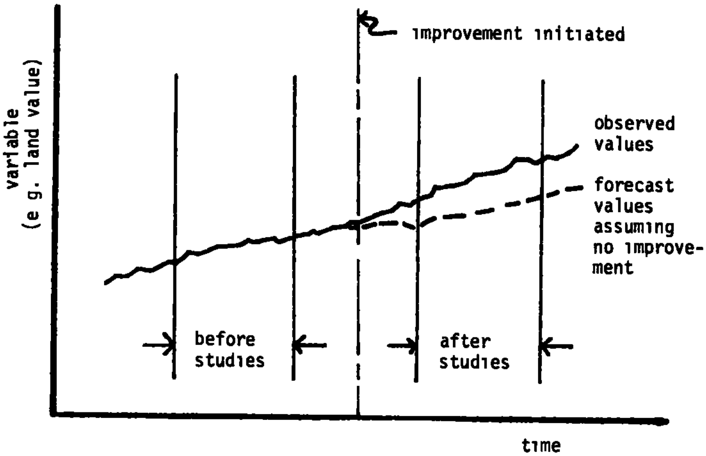

Figure 1. Comparison of outcome with forecast

in Figures 1 and 2 for the after period actually different? Based on the information typically given in such studies, the answer to this question can be made only in terms of one's judgment and experience. Such opinions are not particularly useful in predicting the effects of improvements in other situations.

In experimental situations the question of significant statistical differences between an experiment and its control is answered by replication of the experiment. The experiment is repeated several times, and the experimental error for the 2 types of observations is computed. The variance of the differences between the experiment and the control is then compared with the variance of the error within the 2 types. If the ratio of the variances associated with these between-and-within group differences is larger than that which could occur by chance, then it is concluded that the 2 types of observations are different. The procedure outlined briefly here belongs to a rich methodological area in statistics called analysis of variance (2, 3). An earlier paper by the author gives an example of an application to the analysis of travel patterns (1).

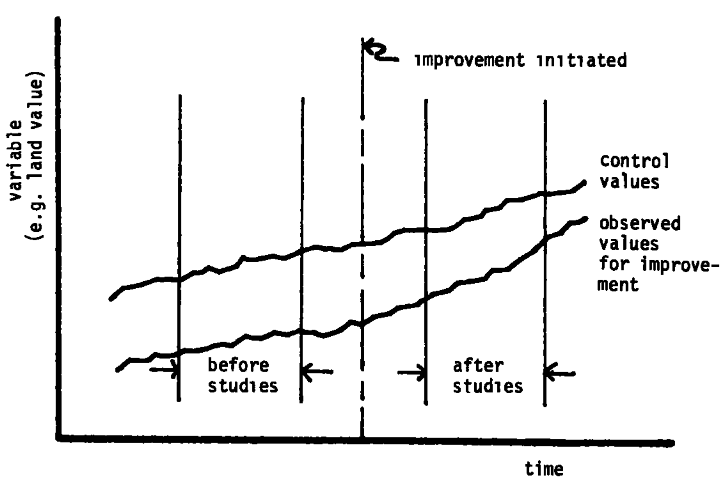

Figure 2 Comparison of outcome with control.

How can replications be achieved in a nonexperimental science? Clearly, if we regard the BART system as an experiment, it cannot be easily replicated from corridor to corridor or from city to city. However, by a careful definition of the problem, we may possibly view an urban transportation improvement as an experiment in the sense just discussed. A possible experimental design for such an experiment is as follows:

1. Let the impacts of interest be associated with points such as transit stations or freeway interchanges;
2. Consider each such point as a potential observation in an experiment;
3. Suppose that one or more corridors in which no improvement is made is identified, together with the hypothetical locations of the impact points (stations or interchanges);
4. To satisfy the randomization requirements of the analysis, draw a sample of points from both the experimental and control corridors; and
5. Compute statistics to test the hypothesis that the difference between the 2 corridors is significantly different from zero.

AN EXAMPLE: THE PHILADELPHIA-LINDENWOLD RAPID TRANSIT LINE

In order to illustrate the concepts of control and replication in a nonexperimental context, I have drawn an example from a proposed impact study of the Delaware River Port Authority's Lindenwold line; the technology of this facility is fully comparable with that of the BART system. Being unfamiliar with the details of the BART system, I am unable to assess whether the methodology is also applicable to that case; however, the concepts should be valid.

The Lindenwold line extends approximately 10 miles southeast from Philadelphia on a former commuter railroad right-of-way. It has 6 center city stations in Philadelphia and Camden and 6 suburban stations with parking for 5,700 cars. Ridership near the

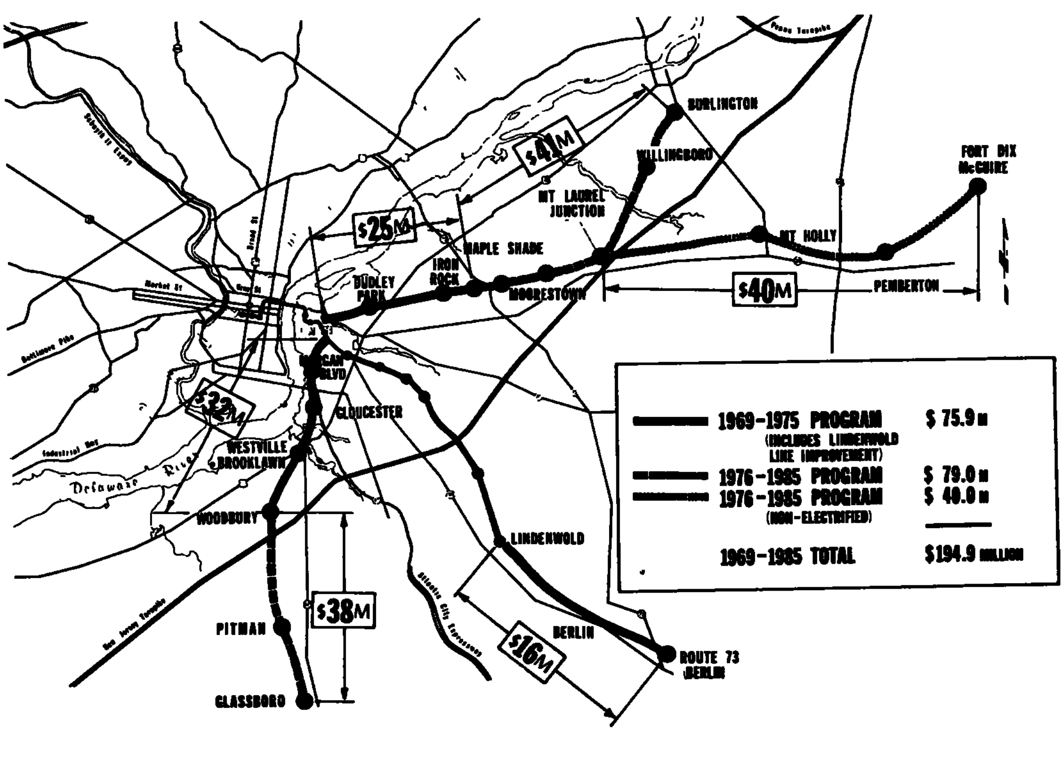

Figure 3. Southern New Jersey Regional Transit Improvement Program.

end of the first year of operation now exceeds 29,000 trips per day; average fare is about 47 cents with a range from 30 to 60 cents. Two additional lines, to Mt. Holly-Willingboro and to Glassboro, are planned for construction in the next 5 to 10 years, also largely on former railroad rights-of-way. All 3 branches of the 70-mile completed system will serve downtown Camden and center city Philadelphia and connect directly with the Philadelphia transit system. These lines are shown in Figure 3.

Now, as an example, consider the specific problem of determining the impact of the Lindenwold line on residential land value. Stations on the Lindenwold line constitute observations in the experiment. Proposed locations on the 2 planned lines provide for experimental control. Hypothetical station locations on 1 or 2 other abandoned commuter lines for which no rapid transit line is contemplated provide for control observations to account for land speculation on all 3 lines.

Stations could be divided into 2 types: (a) those serving residential areas developed prior to 1945 and (b) those serving residential areas developed after 1945. All corridors have at least 2 stations of each type. For each station a series of concentric zones of equal area could be defined. Within each zone the year-to-year changes in residential land value can be determined from sales value and assessment records.

These definitions of terms permit an experimental design to be stated. Let factor A be the corridor type with 3 levels or classes: Lindenwold corridor, planned transit corridor, and corridor with no transit. Let factor B be the distance from the station with levels consisting of areal rings extending 2 to 5 miles from each station. Let factor C be area type: prewar development and postwar development. Let 2 stations drawn at random from each category of corridor and station type constitute replications on the criterion variable, change in residential land value.

If a 3-way analysis of variance model is used, a large set of hypotheses, such as the following, can be tested:

1. Mean changes in land value are equal for all corridors;
2. Mean changes in land value are equal for all station types;
3. Mean changes in land value are equal for all areal rings
4. Mean changes in land value are additive for corridor type and station type (no interaction effect); and
5. Mean changes in land value are equal for prewar service areas for all corridors.

Many other tests could also be conducted, giving special attention to interaction effects and the assumptions of the model. Other variables could be studied such as changes in residential construction, employment, residential rents, land use conversion, and so forth.

CONCLUSIONS

This example serves to illustrate the application of control and replication concepts to the study of the impact of urban transportation facilities. The example also suggests that a rather large and rich methodology is available for studying such effects. These methods are extremely flexible, and are by no means limited to the simple example described. Their careful application should lead to useful results from impact studies of the Bay Area Rapid Transit system.

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