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Evaluating the attractiveness of a new light rail extension: Testing simple change and displacement change hypotheses

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ABSTRACT

Many communities in the United States have been adding new light rail to bus-predominant public transit systems. However, there is disagreement as to whether opening light rail lines attracts new ridership or merely draws ridership from existing transit users. We study a new light rail line in Salt Lake City, Utah, USA, which is part of a complete street redevelopment. We utilize a pre-test post-test control group quasi-experimental design to test two different measures of ridership change. The first measure is calculated from stops along the light rail route; the second assumes that nearby bus stops might be displaced by the rail and calculates ridership change with those stops included as baseline. Both the simple measure (transit use changes on the complete street light rail corridor) and the "displacement" measure (transit use changes in the one-quarter mile catchment areas around new light rail stops) showed significant ($p < .01$) and substantial (677%) increases in transit passengers compared to pre-light rail bus users. In particular, the displacement analysis discredits a common challenge that when a new light rail line opens, most passengers are simply former bus riders whose routes were canceled in favor of light rail. The study suggests that light rail services can attract additional ridership to public transit systems. In addition, although pre-post control-group designs require time and effort, this project underscores the benefits of such quasi-experimental designs in terms of the strength of the inferences that can be drawn about the impacts of new transit infrastructure and services.

1. Introduction

Assessing transit ridership changes after the construction of light rail is integral to transit policies but has been infrequently evaluated in a comprehensive manner. The present study is located in Salt Lake City, UT, USA, and utilizes a novel set of comparisons to evaluate whether transit use increased when a new light rail line opened in a neighborhood previously served only by local bus routes. Although many prior studies have addressed the ridership impacts of new rail lines, they all have methodological constraints that prompted our use of new measurement, a new quasi-experimental design, and new data gathering techniques.

First, we propose two new measures of increased ridership, one a "simple change" measure and the second a "displacement" measure. The "simple change measure" shows whether ridership increases along the new rail corridor in comparison to baseline bus ridership in that same corridor (prior to rail construction). The "displacement measure" evaluates a common challenge that new light rail lines do not attract new ridership but simply attract ridership from bus routes that were canceled when light rail opened (Rubin et al., 1999; Baum-Snow and Kahn, 2000; Cox, 2000). To address this "displaced bus riders" challenge, we count baseline bus use in the entire quarter mile catchment area (not only along the rail corridor), and ask whether light rail use exceeds the baseline catchment area bus use.

Second we employ a pre-post, treatment-control, quasi-experimental design to better infer a causal relation between the new line and increased ridership. The pre-post design provides an estimate of changes in transit use that might occur without any

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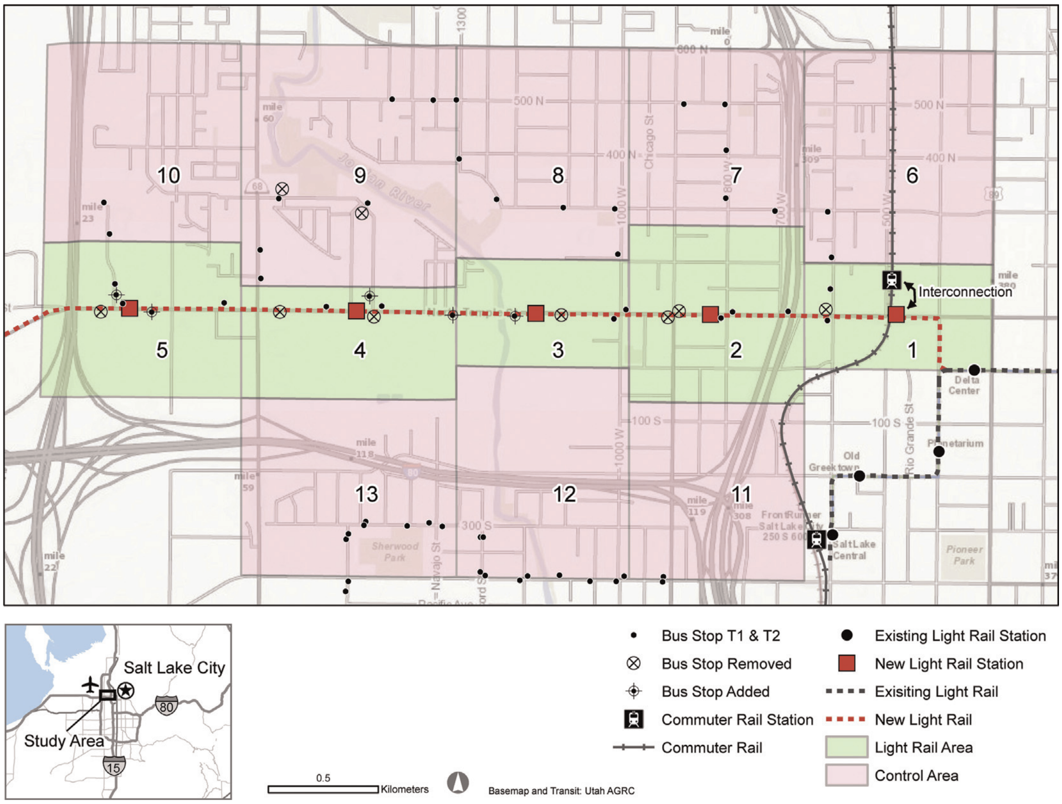

Fig. 1. Overview of study area showing the one-fourth mile street network walking distance above and below the intervention corridor and control areas one-fourth mile beyond that.

intervention. For example, transit use might increase because of increased employment, growth in population, or seasonal changes in travel due to holidays or school and university schedules. These events would affect both the control and light rail areas similarly; therefore, differences between them in ridership would most likely be due to the attractiveness of light rail.

Quasi-experimental designs were developed for settings where “true experiments” are impractical but researchers want to evaluate whether a causal relationship might exist (Campbell and Stanley, 1963). True experiments are the “gold standard” for demonstrating causal relationships – in the present case, that the new light rail line is responsible for increased ridership. True experiments comprise three features: manipulation of a treatment; use of both treatment and control groups; and random assignment to groups. If the outcome measure is significantly different between the treatment and control groups, a true experiment supports the idea that the treatment was effective. In this study, the treatment is the new light rail line and the control groups are bus routes in catchment areas between one-fourth and one-half mile of the light rail catchment area (see Fig. 1). The one-fourth mile buffer distance was used because it is a typical distance bus riders will walk to access bus stops (O'Sullivan, Morrall, 1996: Furth and Rahbee, 2000: Murray and Wu, 2003). The combined intervention and control catchment areas extended one-half mile above and below the intervention area.

Random assignment is essential because it assures that the groups are equal prior to the intervention, making the intervention the most likely source of differences after the intervention. However random assignment is impossible in most studies of transit use, so logic and the quasi-experimental design are used to rule out rival explanations for treatment effects. In the present research, the control neighborhoods are adjacent to and similar in size and configuration to the intervention neighborhood. They comprise neighborhoods just beyond the quarter mile catchment area surrounding the new rail line. People living in the control and treatment areas are in similar census tracts and are similar in demographics (age, income, employment status, ethnicity, education, etc.). These physical and demographic similarities, coupled with the pre-post comparisons, reduce the likelihood that neighborhood differences account for differences in ridership.

Third, the analyses are based on on-site counts of bus and light rail passengers. We made the decision to use our own counts in part because the passenger counting systems at the local agency were undergoing changes [similar to the dynamic changes seen with counting systems nationwide (Boyle, 1998, 2008)]. Therefore, in order to assure that a similar system would be available for counts in both years, trained observers made the counts. On-site counts also have advantages over passenger surveys which can be limited by response rates, non-representative samples, and self-report recall biases and other errors. In addition, although we do not share their concern, critics of light rail often distrust ridership figures provided by transit agencies (O'Toole, 2010). Thus, there are several reasons for using on-site passenger counts with proven inter-rater reliability.

Results provided statistically significant support for the following three research questions.

1. Simple hypothesis, focusing on the light rail corridor: light rail ridership on the corridor was substantially higher than baseline bus use on the corridor; it was also substantially higher than pre and post bus ridership in the adjacent control catchment areas. This pattern provides strong evidence that ridership increased for light rail.

2. Displacement hypothesis, accounting for former bus ridership within one-quarter mile of the new rail stops: light rail ridership in the intervention one-quarter mile catchment area was significantly higher than Time 1 bus ridership in that area. Furthermore, rail ridership in the intervention catchment area was significantly higher than Time 2 bus ridership in the adjacent