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

Carol M. Werner^a, Barbara B. Brown^b, Calvin P. Tribby^c, Doug Tharp^d, Kristi Flick^d, Harvey J. Miller^e, Ken R. Smith^b, and Wyatt Jensen^f

^aDepartment of Psychology, University of Utah, 380 S 1530 E, Salt Lake City 84112, UT, USA

^bDepartment of Family & Consumer Studies and Cancer Control & Population Sciences, Huntsman Cancer Institute, University of Utah, 225 S 1400 E RM 228, Salt Lake City 84112, UT, USA

^cDepartment of Geography, Ohio State University, 1036 Derby Hall, Columbus 43210, OH, USA

^dDepartment of Family & Consumer Studies and Cancer Control & Population Sciences, University of Utah, 225 S 1400 E RM 228, Salt Lake City 84112, UT, USA

^eDepartment of Geography, Ohio State University, 1036 Derby Hall, Columbus 43210, OH, USA

^fDepartment of Family & Consumer Studies, University of Utah, 225S 1400 E RM 228, Salt Lake City 84112, UT, USA

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.

Keywords

Light rail; Bus; Ridership; Complete streets

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 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 rider-ship 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 control catchment area. Thus, the pattern of results was consistent with results for the simple hypothesis and did not support the hypothesis that light rail ridership simply comprised former bus riders whose routes were canceled in favor of light rail.
3. Local neighborhood without commuter rail. The easternmost stop in the study area was a light rail/commuter rail stop combination. In order to focus on the local community as the source of ridership, we tested the same research questions without this easternmost stop. Both the simple and displacement tests were supported, although the magnitude of the effects was smaller. Note that the light rail has a more local ridership but the commuter rail serves more distant suburbs.

Analyses with and without the commuter rail stop provide data for two different ridership markets and show that both increased.

In the next section of the manuscript we provide a literature review and rationale that puts our project in the context of previous similar research. The subsequent section is a description of methodology and the following section provides detailed results. The final section contains a discussion of this project and prospects for future research. We suggest that our methodological and measurement procedures, along with other strong quasi-experimental approaches (Cao and Schoner, 2014), can provide data relevant to understanding changes in transit ridership and to addressing particular charges of overestimated ridership counts by critics of transit policies.

2. Literature review and rationale

Many scholars have expressed concerns about the role of automobile use in societal problems, including low physical activity and associated health problems, such as obesity, cardiovascular disease, diabetes, and some cancers (Yang and French, 2013, Handy et al., 2002, Warburton et al., 2006). Additional concerns are raised about carbon emissions and poor air quality, especially under the specter of possible global climate change (Cervero and Murakami, 2010, Delucchi, 2000, Dulal et al., 2011, Newman and Kenworthy, 1999, Shoup, 1997). A critical question is whether high quality public transport can attract ridership, a question which has limited evidence from researchers outside of transit agencies. The present study asks whether there is an increase in transit use when light rail replaces local bus routes.

A common way to demonstrate changes in ridership levels is to count ridership on new rail lines (e.g., Soundtransit, 2013). For example, a review of published reports and inquiries with transit officials in 11 European and US communities showed dramatic increases in use for most new light rail lines (Hass-Klau et al., 2003). However, the pre-post design of these comparisons is inherently limited. Transit use might have increased for any number of reasons, not necessarily the provision of a new rail line. For example, the areas around the lines may have experienced increased population or employment, or new highly attractive destinations may have been constructed. Furthermore, ridership may be subject to seasonal variations that may differ from pre- to post-test (Moore, 1993). The presence of such rival hypotheses makes it difficult to infer a causal relationship between new light rail lines and increased ridership (Campbell and Stanley, 1963); control groups are needed in addition to pre-post designs to eliminate many common rival hypotheses.

Some cross-sectional studies have adopted control groups, but as reviewed by Cao and Schoner (2014), it is difficult to identify an ideal control group. Ridership on new lines has been compared to control groups that include countywide, citywide, or regional ridership (Cao and Schoner, 2014) or projected ridership (Moore, 1993). As Cao and Schoner pointed out, comparing one line's ridership to area-wide ridership is imprecise. For example, when comparing ridership on a new line in the middle of a city with ridership county-wide, the type of people who are compared are likely to be quite different, especially in terms of their willingness to use any transit at all (Schwanen and Mokhtarian, 2005). In addition, area-

wide control groups likely have lower, less attractive levels of transit service. Such selection confounds and comparisons undermine claims of ridership increases.

To counter such selection threats, Cao and Schoner carefully chose their control and new rail neighborhoods and then used propensity score modeling to simulate random assignment to these groups. Propensity score models are statistical models that use a large number of variables to match existing groups on relevant attitudes (e.g., pro-transit) or characteristics (e.g., affluence) that might compete with transit availability as explanations for different rates of transit use. Their results showed that there was a 50–80% higher level of ridership among rail- vs. bus-served residents. Although this supports the idea that rail is attractive, the increase was less than the 300–500% increases reported in studies they reviewed that did not employ rigorous controls, raising concerns about the procedures used to claim increased ridership on new rail lines (Cao and Schoner, 2014). Thus Cao and Schoner's work has challenged the field to be more careful about procedures used to assess ridership increases.

Even more persuasive than the addition of control groups in cross-sectional research is the use of pre-post designs with control groups, although these are rare. In Manchester, England, where a heavy rail was converted to a light rail, a comparison of residents in rail corridors and non-rail corridors showed a trend toward increased ridership of light rail among nearby residents and a significant decrease in bus rides (Senior, 2009). Amongst the non-rail corridor controls, bus ridership remained stable. Thus, the new light rail appeared to support greater transit ridership compared to controls, however there are limitations to the method. The panel sample selected to test longitudinal relationships was limited to 211 residents from a larger pool of about 1000 residents; wisely, the panel was limited to those who had no changes in reasons for travel, such as employment; however, controls were different people in the before and after periods. Data were based on self-reported frequencies of past week ridership, and it was not possible to differentiate new ridership on the new light rail line from rides on existing lines. Although these methodological difficulties weaken causal inferences, the study enabled tests of relationships over time and supported a claim of increased ridership for the new rail, relative to controls. As another example, an evaluation of the new Exposition rail line in Los Angeles also found that those receiving a new rail line reported increased rail rider-ship and stable bus ridership, with no differences seen among controls (Boarnet et al., 2013). Such pre-post studies, while promising, are infrequently conducted, time-consuming, costly (Mokhtarian and Cao, 2008), and incomplete.

In the present study, a different issue is tested with a second hypothesis, the “displacement hypothesis,” which assesses changes that might have been forced by the discontinuation of bus service when the light rail service began. When rail line construction is debated within a community, it is common to hear detractors claim that most of the rail riders will be individuals who rode buses prior to rail construction but who do not have that option post-rail construction because all of the former bus stops were eliminated (Ennis, 2010). Thus, critics charge that the counts of new rail ridership are artificially inflated by nearby bus riders who were forced to use light rail because their bus service had been discontinued (Cox, 2000). In fact, transit agencies do eliminate bus routes to avoid duplication of service. Weyrich and Lind (2001) assessed these charges and concluded that between 15% and 50% of new rail riders had been bus riders, a conclusion supported mostly by survey data.

Another study concluded that rail riders were likely former bus riders, but the study was based on simulations of likely time costs of alternative travel modes, not actual data from riders (Baum-snow and Kahn, 2005). Therefore, past studies have not fully addressed a central concern from critics that transit agencies eliminate bus service that would compete with the new rail line (Moore, 1993), setting up a situation in which displaced bus riders inflate the counts of new light rail ridership. Furthermore, these studies did not directly account for passengers using bus stops close to new rail stops, the stops most likely to be closed with the arrival of new light rail.

In sum, this project examined three questions: (1) the simple question of whether light rail attracted new riders, indicated by ridership on the corridor exceeding pre-light rail bus use; (2) the displacement challenge, that new light rail does not attract new riders but simply absorbs baseline bus riders whose routes have been canceled. There could be two patterns to the data. If eliminated bus stops account for all the ridership of the new rail system (i.e., there is no increase in total ridership), light rail would absorb all displaced Time 1 bus riders, and with no new ridership, the catchment area counts would not change pre- and post-light rail. In contrast, if light rail attracts new ridership, then the Time 2 rail counts will be larger than all the catchment area bus counts from Time 1. Thus, in order to claim that light rail attracts new transit users, the Time 2 light rail counts would need to be significantly higher than the Time 1 passenger counts in a reasonable walking distance catchment area along the new route. (3) The third question is whether there were significant ridership increases within the neighborhood, independent of riders transferring to the new line from heavy rail arriving from the suburbs. The simple and displacement hypotheses are retested, omitting the commuter rail stop.

Finally, two control areas above and below the rail corridor add additional information about transit choices. If the Time 1 bus riders in the control areas abandon their buses in favor of light rail or if their bus is discontinued, at Time 2 we should see a decrease in bus passengers in the control areas and a corresponding increase in light rail ridership.

3. Methodology

3.1. Light rail background and context

The new light rail extension connected two important primary destinations, the international airport at the west end and the city's downtown to the east and south, approximately 6.5 miles apart (10.5 km). The airport is a terminus, but a stop at the east end of the study area connects passengers to new station on an 87-mile north-south commuter (heavy) rail line between Ogden and Provo. In the downtown area, connections are available to 2 other light rail lines; the airport line continues south to an outlying community.

With respect to justifying the new line and its potential for financial success, the history of light rail in the County was initial public opposition followed by enthusiasm and tax support as popularity and use of light rail grew. The airport extension has been discussed for many years. Indeed, a light rail line between the airport and downtown had been included in the proposal for a new line to the University of Utah for the 2002 Olympics. At that time, funding was only obtained for the university line, and in the years since then, developing

light rail to the airport was set aside in favor of extending or adding routes to communities to the south and west of downtown. In reintroducing the airport line, transit authority publicity emphasized traffic to/from the airport, with some acknowledgment of other major destinations on North Temple that might attract employees and customers (three state office buildings, the headquarters of the local electric company, the State Fairpark as well as a number of small businesses). Transit authority and local community leaders were also interested in potential neighborhood use in the study area. In the decision making phase, there were public meetings for discussions about the new line's stop locations, décor, and safety, among other issues (<http://www.rideuta.com/files/12AirportLRESRCHAPTER5.pdf>).

During the first three weeks of the new line's operation (but excluding the grand opening day) the Utah Transit Authority reported that the airport attracted 6,692 weekly boardings and the downtown stop (stop 1 in Fig. 1) averaged 5,026, with the intermediate stops averaging somewhere in between (in Fig. 1 stop 5 had 3,714 boardings; stop 4 had 1,824; stop 3 had 586; and stop 2 had 2,685 boardings); the new stops are part of the larger green line that averaged 27,242 riders per week during those three weeks (Lee, 2013).

The current study is part of a larger assessment of the effects on nearby residents of a complete street renovation of North Temple Street in Salt Lake City (<http://bikeslc.com/GetInvolved/MasterPlansandPolicies/PDF/CompleteStreetsOrdinance.pdf>) (Authors, 2014) (see Fig. 1 for overview). In this renovation, the street along the light rail corridor was redesigned to appeal to non-automotive modes of travel as well as automobile users. Among other changes, the street received the new rail line, 10-ft wide sidewalks along much of the route, safer bike paths, reduced traffic speed and fewer lanes, and new esthetics (attractive light posts, landscaping, colorful brick sidewalks and crosswalks, etc.). These improvements were only along the light rail corridor and did not extend into the neighborhood. It is unclear if improving only the corridor would be enough to attract local residents to light rail. The neighborhood is socioeconomically and ethnically mixed and the rail corridor traverses an area of mixed industrial, commercial, and business use.

3.2. Study approach and research design

The study is designed to evaluate whether a new light rail line attracts new users or simply absorbs existing bus riders. A first, simple analysis, compares baseline bus use with light rail ridership on the corridor and asks if there is an increase. A second analysis, the catchment-area analysis, is based on the idea that transit agencies cancel closeby bus routes, thereby forcing former bus riders to shift to light rail. The catchment-area analysis accounts for such displaced bus riders by including them in the pretest passenger counts. That is, to adjust for the possibility that new rail ridership includes bus riders whose bus stops were eliminated when light rail service began, we expand our Time 1 counting beyond the simple corridor counts of the first hypothesis to include the larger catchment areas. Thus, the geography of the pretest counts is expanded to include all bus stops within a one half-mile zone around the planned rail line (that is, 1/4 mile walking distance to the catchment area boundaries above and below LR). This is conservative because one-quarter mile is the walking distance

typically considered to be the catchment area for bus stops (O'Sullivan and Morrall, 1996; Furth and Rahbee, 2000; Murray and Wu, 2003).

Only two bus routes were canceled when the airport light rail opened. One was a route between a multi-modal transit stop (Salt Lake Central Station) and the airport; this bus primarily used the freeway but was on the light rail corridor between Redwood Road (1700 West) and the west edge of the study area. The other canceled route traveled between a northwest neighborhood and downtown. The bus traveled north–south within the neighborhood and also traveled east–west on the light rail corridor the entire width of the study area. Although the replacement route continued to weave through the neighborhood, it did not cover the east–west portion of the original route. Both are examples of routes whose east–west passengers would have been displaced by the route change. Passengers would need to switch to a different east–west bus route or take light rail.

The study used a basic 2 Time (Before/After Light Rail) by 2 Areas (Control Areas/ Intervention Areas) between and within factorial design. The control areas (numbered 6 through 13 in Fig. 1) comprised a “bus only” condition; the light rail intervention was a “bus then bus plus light rail” condition (areas numbered 1–5 in Fig. 1). This design was used with two variations. First, there were two different dependent measures. One tested the simple hypothesis by focusing only on ridership on the light rail corridor; the other tested the displacement hypothesis by including riders in the entire catchment area along the light rail route. The second variation focused on the neighborhood by limiting analyses of these two dependent measures to the neighborhood stops, excluding the commuter rail stop.

3.3. Defining catchment and control areas

In order to have identical catchment areas before and after the complete streets and light rail intervention, each catchment area was centered on a light rail stop and similar-sized areas were drawn around them. The boundary between adjacent stops was located equidistant between the two stops in an east–west direction. Thus, each rail stop defined the center of its catchment area; the north and south control areas were centered directly above and below these stops. Bus stops within one quarter mile on the street network (typical walking distance) were considered to be in that light rail stop area; bus stops further away were considered to be in the control (no rail) area. In Fig. 1, the uneven north south distances along the light rail corridor reflect the differences in street network access. Note in particular that the neighborhood below the light rail corridor has limited access to light rail stops, compared to the area north of the corridor. As shown in Fig. 1, the upper sections of Areas 12 and 13 are roadless and provide no direct access to the light rail corridor (these are closed industrial areas, including a railroad yard and power station). Depending on the proximity of their dwellings, residents in the lower areas have access to the rail corridor via several unobstructed north–south streets shown on the maps (600 W, 800 W, 900 W, 1000 W and Redwood Road). In sum, there were five light rail areas and eight no rail controls of approximately equal size. At Time 2, the control areas contained only their original bus stops while each light rail area contained a mix of bus stops and that area's single light rail stop.

Except for the simple analysis of stops directly on the light rail intervention corridor, the unit of analysis was the one-fourth mile catchment area so as to include passengers within walking distance of the bus and light rail stops. This strategy had the additional benefit of accommodating changes in stop locations that occurred before and after the intervention. There were different numbers of stops at Times 1 and 2 because the light rail stops did not exist at Time 1, because some of the bus stops were eliminated between the two observation periods, and because some stops were shifted during observation due to construction activity. As noted, data from individual stops were combined into a single sum for each catchment Area for Time 1 and Time 2; this allowed the focus to remain on the total number of passengers in each area despite the stop changes.

3.4. Data collection

Passenger counts were collected weekday mornings during October 2012 and 2013 on similar dates so that daily temperatures would be similar and activity patterns would be comparable from one year to the next. We counted passengers waiting at each stop on study-area routes on five separate week days at both Times 1 and 2. Because we were interested in passengers originating in the study area, we counted only people waiting at a bus or light rail stop; we did not count people already on or exiting from transit. To focus on local residents and those using commuter rail during the highest use period, data collection covered a two-hour period during the morning commute (7:00 am until 9:00 am); two hours was enough time to make five counts of each route. At Time 1, the control areas contained 43 bus stops and the light rail corridor contained 19 bus stops. At Time 2, the control areas contained 43 bus stops and the light rail corridor contained 17 bus and 5 new light rail stops. Although the number of stops in the light rail corridor differed by only 2 between Times 1 and 2, seven bus stops had been discontinued and five others had been added as part of the redesign on the corridor (minor shifts in stop locations are not included). For the combined intervention and control areas, there were 1086 observations at Time 1 and 1236 observations at Time 2.

Observers in vehicles drove along the bus and light rail routes and counted passengers waiting for a bus or light rail. People were considered to be waiting for a bus if they stood next to or within 10 feet of a bus stop as observers drove past. For light rail, individuals were considered to be waiting for light rail if they were on the platform or were at curbside of the mid-block crosswalk waiting for a “walk” signal to access the platform. Individuals seen running towards either a bus or light rail stop were only counted if the rater could see them actually reach the stop and wait for transport.

The goal of the observations was to count all people who met the above criteria of “waiting for transit.” Because of traffic conditions, we met this goal differently in the control and planned light rail areas. In the control areas, there were three unique bus routes, two north and one south of the intervention corridor. For counting passengers on these routes, individual observers drove along the route ahead of the bus and recorded the number of passengers waiting at each bus stop. At this time of day (7:00–9:00 am) there was very little traffic in the control areas and it was easy to stay ahead of the bus and count waiting passengers. Because the observers could usually see all passengers as they entered the bus, the control stop counts are considered to be comprehensive.

For the intervention corridor, passenger counts were conducted in a similar manner, except to assure safe driving, an observer accompanied the driver and counted passengers at all bus stops plus, at Time 2, the five light rail stops. On the intervention corridor, there were 11 different bus routes (9 routes at Time 2) all using the same bus stops, but each on a different schedule. The most efficient way to count passengers was to drive along bus routes and count people waiting at each stop (regardless of which particular bus route they needed). Waiting to count until all passengers had arrived was not possible because of traffic, so counts included all waiting passengers visible during the brief drive-by time. We consider this to be a time-based sample rather than a complete count. Driving at a steady rate (rather than waiting for passengers to assemble) allowed us to maintain similar route schedules thereby achieving comparable counts in the intervention area across days as well as between Times 1 and 2. Thus our method likely underestimated passenger counts along the rail corridor. However, by using the same methodology and driver, this underestimate was consistent at Time 1 and Time 2 and occurred with both bus and light rail passengers in the intervention corridor. Because the control area counts were focused and complete, the intervention area counts contained a conservative bias that was present at both pre- and post-test for both bus and light rail passengers.

3.5. Inter-rater reliability

Inter-rater reliability (IRR) data were collected at both Times 1 and 2 for bus and light rail routes. Two experienced observers rode in the count vehicle and collected data silently and independently. Inter-rater influence was reduced by positioning observers in the front and back seats of the vehicle. For the control areas, Time 1 IRR, $r(77) = .96$ and Time 2 IRR, $r(38) = 1.00$, where .80 is acceptable and 1.00 is the highest possible agreement score. For the light rail corridor, Time 1 IRR, $r(146) = .93$ and Time 2 IRR, $r(178) = .98$. Thus, inter-rater agreement was very high.

3.6. Data and analyses

Data were the counts of people waiting at bus or light rail stops within each numbered map area; counts were summed across five observation days for Time 1 and again for Time 2, and these sums were used in analyses. For control area bus stops, counts were summed for each entire catchment area. For the Intervention area (i.e., complete street routes) data were summed differently for the simple and displacement (catchment area) hypotheses. For the simple hypothesis, data were summed only for stops directly on the light rail intervention corridor in each area. For the displacement hypothesis, data were summed for all stops in each catchment area along the intervention corridor. The major analyses included all five stops in the study neighborhood and the control areas; remaining analyses omitted the Commuter Rail stop to focus more specifically on the neighborhood as the source of displaced riders.

The simple and displacement hypotheses are each tested twice, once with all the areas and second omitting the commuter rail transfer station to focus on neighborhood usage. These four hypotheses are tested with planned contrasts that address the central question of whether the new light rail increased ridership. The planned contrast compares the Time 2 light rail corridor counts to the sum of the counts in other three cells in the design (i.e., Time

1 rail corridor, Time 1 control, and Time 2 control). If the contrast yields a significant coefficient, it indicates that the Time 2 light rail counts are higher than the sum of the others. This is evidence that Time 2 light rail – and only that group – experienced an increase in ridership. Bonferroni adjustments for multiple tests were used to protect the study wide alpha level at .05; the Bonferroni-adjusted alpha level for the 5 contrasts is $p = .01$ (i.e., .05/5 tests). Preliminary analyses included population density and morning temperature as covariates to control for their effects on ridership. However, these covariates did not account for significant variance (p 's $> .50$) so they were omitted from analyses. Data were analyzed using fixed effects repeated measures regression (Proc Mixed using SAS v 9.2, Cary, NC) on the summed counts for the corridor and control areas at Times 1 and 2. Although it is appropriate to present the planned comparison tests to address our specific hypotheses, results of the 4 regression analyses for the full 2 x 2 design are available from the first author; in all cases, the interactive effect from the 2 x 2 analysis is similar in magnitude to the result of the planned contrast. A final test compared the Time 1 and Time 2 control area counts; a significant drop in control area passengers might suggest some shifting from bus to light rail.

4. Results

4.1. Simple hypothesis

Changes in transit use on stops along the intervention corridor vs. control catchment areas. After the new light rail line opened, transit ridership increased significantly along the corridor but did not change in the adjacent control catchment areas. As shown in the first row of Table 1, on the light rail corridor, the passenger counts collected at bus and/or light rail stops increased by 677% between Times 1 and 2. Passenger counts did not increase in the control areas and in fact decreased by 29%, a nonsignificant change, $t(8) = 2.70$, $p < .027$, where the Bonferroni criterion is $p < .01$. The strength of the increase and the drop in the control area contributed to a significant planned contrast of the Time 2 light rail corridor versus the other three cells in the design, $B = 32.23$, $SE = 8.19$, $t(23.4) = 3.93$, $p < .0006$.

4.2. Displacement hypothesis

Changes in Transit Use in Corridor Catchment Areas versus Control Catchment Areas: The pattern of results was very similar for the displacement hypothesis. This analysis includes the intervention and control catchment area bus riders at Time 1 to address directly the challenge that increased ridership when light rail opens is primarily due to existing bus riders being forced to use light rail when their bus routes are canceled. Contrary to this claim, after the new light rail line opened, transit ridership increased by almost 700% in the corridor catchment areas (Table 1, row 2) but did not change in the adjacent control catchment areas; planned contrast of Time 2 Light rail vs. the three other cells, $B = 31.63$, $SE = 7.99$, $t(23.4) = 3.96$, $p < .0006$. As described in the simple analysis, Time 1 vs. Time 2 ridership in the control catchment areas declined slightly though not significantly, $p < .027$, n.s. using the Bonferroni criterion. The overall differences in passengers for the displacement results are shown in Fig. 2. The dotted portion of the Time 2 intervention catchment area shows that approximately 90% of the passengers were waiting for light rail rather than buses, indicating that the Time 2 increase was due to the light rail ridership increase. To complement this, Fig. 3 shows the change in use rates area by area. The

numbers on each catchment area underscore that there is little change in passenger counts in the control areas but clear increases along the light rail corridor.

4.3. Focus on neighborhood passengers

To show the robustness of the increased ridership within the neighborhood, the lower part of Table 1 shows passenger counts for analyses without the Commuter Rail station and its high number of suburban passengers. The simple hypothesis, row 3, compares the 4 stops on the light rail corridor against the control catchment areas. Although the increase in ridership is lower without the commuter rail stop, the increase is still considerable (293%), and the planned contrast is significant, $B = 18.10$, $SE = 3.68$, $t(15.6) = 4.92$, $p < .0002$. These neighborhood-focused results are similar for the row 4 displacement hypothesis, which compares the light rail catchment areas against the control catchment areas, omitting the commuter rail stop, $B = 18.06$, $SE = 3.65$, $t(15.6) = 4.95$, $p < .0002$.

5. Discussion

This study used improved methodologies to test two hypotheses related to the attractiveness of light rail compared to local buses. Both hypotheses asked whether light rail was a sufficiently attractive transit mode that it could draw additional ridership above the Time 1 observations. For both hypotheses, the observations were clustered around five planned light rail stops and compared riders along that corridor to riders in the nearby control areas. The simple hypothesis measured transit use for stops only on the road hosting the light rail corridor. The displacement hypothesis evaluated the criticism that light rail does not attract new ridership but simply accommodates people who had already been riding buses in the catchment area. To do this, we measured transit use in the catchment areas (one-quarter mile street network buffers) above and below each planned light rail stop. Both the simple and displacement analyses showed that transit use increased substantially along the light rail corridor and dropped by a nonsignificant 29% in the control catchment areas. The increase for the simple hypotheses was 677% and for the displacement hypothesis, 682%. Omitting the commuter rail stop, these increases were 293% and 322%, respectively. Most important, as indicated by Fig. 2, almost all of the Time 2 passengers observed on the corridor were using light rail, with just a few using local buses.

By using a quasi-experimental pre-post control group design and counting passengers during the same discrete time periods one year apart, this study provides strong evidence of increased ridership along the light rail corridor. This is consistent with surveys showing that transit users prefer rail over bus when rail offers higher quality service, as in the present case (faster, more frequent, and more reliable than local buses) (Benakiva and Morikawa, 2002; Brown et al., 2003). Including the adjacent control areas is particularly important methodologically because ridership in both control and intervention areas would be affected by factors that might increase transit use in general (holiday shopping, increased employment, school schedules, etc.). The substantially greater rail ridership at Time 2 for both the simple and displacement analyses underscores the potential for light rail to attract new transit ridership. An intercept survey would be needed to determine whether the increased ridership is achieved by attracting new riders and/or by supporting more trips from

pre-existing riders. However, given the magnitude of effects it is likely that new rail riders were attracted to the new rail line.

In order to enhance the generalizability of the findings of ridership increases, we distinguished between transit users in the neighborhood and those who transferred from the north–south commuter rail line. Both simple and displacement analyses showed significant ridership increases when light rail opened. Fig. 3 shows that there are considerably more people waiting at the commuter rail stop than the other stops in the study area, supporting the value of the transit authority's decision to link commuter rail with light rail. A similar though less dramatic increase obtained in the analyses that omitted the commuter rail stop. Even without that stop, there were substantial and significant increases in passengers in the light rail corridor at Time 2, with light rail ridership accounting for most of the growth. Note that omitting the commuter rail stop results in an underestimate of neighborhood light rail use. Observers often saw local residents walking toward the light rail platform at the commuter rail transfer station but were not able to keep separate counts of neighborhood vs. commuter rail passengers. It was clear to observers that the predominant source at the commuter rail transfer stop were people offloading from commuter rail and waiting for light rail (access to departing commuter rail was at a different location).

One difference between this study and others is that data were collected for 5 light rail stops and adjacent controls for 10 hours of observation per area, a small sample compared to some research. We targeted the morning commute period to be able to assess use by local residents. This amount of data is adequate because the comparison of the control and light rail areas shows a clear difference between bus and light rail use; the strong effects make it unlikely these differences occurred by chance. Indeed, Fig. 2 shows an almost 7-fold increase in passengers in the light rail corridor. Although the absolute numbers were small in comparison to other reports (e.g., Hass-Klau et al., 2003), it is essential to keep in mind that other data typically describes full days of passenger use for several weeks and even months.

Future studies could rely on automated passenger counts for assessing larger systems or for more detailed assessments of the effects of time of day, day of week, or particular stops. Although relying on observed passengers is labor-intensive, it has the value of a similar methodology across Times 1 and 2 and does not rely on agency data. As automated counting systems become more standardized, studies similar to the present one might be done on new rail systems for less difficulty and cost (Boyle, 2008). To establish comparability between the methods, it may be important to compare automated counts with on-site counts, at least for a sampling of the observations. Given the fact that many transportation policy officials face the same criticisms of diverting bus riders to rail lines, it will be important to replicate the tests in the current study so that there are multiple sources of data that can address criticisms of claims that displacement accounts for rail ridership increases. In addition to the counting system, it is important that researchers use designs that eliminate rival hypotheses such as the present pre-post control group design that increases confidence that the new rail line was the impetus for increased ridership.

For purposes of a larger complete street study, we were especially interested in the impact on local residents of providing light rail and therefore focused only on the five neighborhood

stops. We made counts only during the morning commute when we expected to find mostly local residents waiting for transit. This strategy would undercount local residents who traveled later in the day or on weekends. We thought it was better to be conservative than to mistakenly include visitors who were taking transit to leave the area. Our counts also did not include passengers who boarded light rail at the airport, and we did not have passenger counts for those who boarded east of the commuter rail stop to come into the neighborhood. Thus, we pursued our intention of evaluating the specific question of neighborhood transit use during the morning commute and our passenger counts should only be used for that purpose.

With respect to policy implications beyond enhanced ridership, there are other benefits that likely accrue from this light rail intervention. Due to the complete street development, the additional ridership had a more attractive setting for approaching transit. In a synergistic way, the increased ridership may have created a setting that felt safer due to increased surveillance and activity—"eyes on the street" (Jacobs, 1961)—potentially magnifying additional use of the area. Additional research is needed to evaluate the role of the Complete Streets intervention in the observed ridership increases. Another strength is that this line connected to other existing lines with multiple and distant destinations. This greatly expanded the reach of the line within the county and – through the commuter rail line – to stops along the 87-mile corridor north and south of the downtown area. This has the potential to open these distant areas to employment and other opportunities for residents. Simultaneously, this made the neighborhood businesses accessible to visitors, which brings the potential for economic growth. Rail access also benefitted people who work in the target neighborhood and the adjacent downtown, which may reduce traffic congestion, reduce the need for more roads, freeways, and parking lots, and may help to improve air quality (Litman, 2007). Other potential benefits for rail include enhanced local business development, greater satisfaction with life among users, increased active travel with its associated health benefits (Lachapelle and Noland, 2012), satisfactions for residents with preferences for transit near home (Lund, 2006; Baum-snow and Kahn, 2005), enhanced real estate values (Petheram et al., 2013), and compared to bus rapid transit, reduced emissions (Puchalsky, 2005) and noise (Stutsman, 2002).

An additional strength is that the intervention made high quality transit service available to a predominantly low-and middle-income neighborhood. While this is tangible support of a much-needed service, it may limit generalizability of the findings to similar neighborhoods in the U.S.; it would not be appropriate to conclude that adding light rail into any neighborhood would be popular. Future tests of new rail corridors with other socio-demographic qualities and land use mixes are encouraged. As more communities develop and expand high quality transit, the popularity of transit and associated development is increasing, both in major US metropolitan areas (Washington DC, Portland OR, New York City, etc.) and in other countries (Hass-Klau et al., 2003). Especially as individuals find ways of making transit time productive for themselves (e.g., working, texting, reading, studying, and sleeping), popularity could increase (Páez and Whalen, 2010; Brown et al., 2003).

In summary, this study found substantial increases in ridership for a new light rail line. In particular the displacement analyses challenged the claim that light rail does not attract new ridership; the present analyses showed that light rail attracted more rider-ship than had the former bus stops in the catchment area. Few studies of the past have addressed this common challenge to light rail. This does not mean that all communities would benefit from potential ridership increases or other potential benefits from light rail. As others have noted, which type of transit to offer needs to take into account multiple factors, including carrying capacity, operating and capital costs, minimum densities (Zhang, 2009), differential accessibility of transit stops across different transit modes (Murray and Wu, 2003), and land use practices that encourage transit (Thompson and Brown, 2012). This study does suggest that transit mode development decisions can be better informed by addressing criticisms of light rail policy more directly and by assessing ridership changes over time in ways that take advantage of stronger research designs and actual observations of ridership.

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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.

Fig. 2. Displacement analysis: Bus and rail ridership over time for control and new North Temple rail areas. Percent change is shown in Table 2.

Fig. 3.
Ridership count locations and ridership changes.

Table 1

Rider counts by Time (1 vs. 2) and Group: Tests of Time 2 rail area vs. other three cells.

Control Area		Rail Area				
A. Complete test; includes commuter rail stop						
	Time 1	Time 2	% Change	Time 1	Time 2	% Change
Simple (corridor only)	334	238	-29	100	777**	+677
Displacement (catchment area)	334	238	-29	102	798**	+682
B. Neighborhood focus; omits commuter rail stop						
	Time 1	Time 2	% Change	Time 1	Time 2	% Change
Simple (corridor only)	334	238	29	93	386**	+293
Displacement (catchment area)	334	238	29	94	397**	+322

Note: Planned contrasts test whether rail Time 2 is greater than the other three cells the Bonferroni adjustment to $p < .05$ for five tests

**
 $p < .01$