THE RIGHT PLACE AND THE RIGHT TIME TO CHANGE TRAVEL BEHAVIOR: AN EXPERIMENTAL STUDY

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ABSTRACT
In an effort to reduce driving, meet environmental goals, and encourage travel by other modes, universities, firms, and cities have implemented an astounding variety of behavioral change programs. However, despite the proliferation of travel behavior programs, post-program evaluations remain ad-hoc. As a result, our collective understanding of what types of programs are most effective remains limited. Even when program evaluations are conducted, many suffer from methodological shortcomings that call into question the dramatic results reported. In this study, we seek to both address methodological shortcomings common in behavioral change program evaluations and to understand for whom travel behavior change programs alter behavior and in what context. To do so, we conduct a true experiment by randomly assigning incoming graduate students at the University of California, Los Angeles (UCLA) to an experimental and control group. Students in the experimental group received information on car-free travel options to campus prior to the academic year. We find that students who received information were significantly less likely to always drive to campus and more likely to take transit than were those who had received no information. Treatment effect was stronger among students who changed residences within the past six months and resulted in substantial reductions in vehicle miles traveled and emissions. These findings support our hypothesis that behavioral change programs can alter travel and that targeting behavioral interventions during life transition periods is more effective than behavioral programs enacted during periods of stability.

KEY WORDS
Experimental research, travel demand management, habit, student travel, sustainable travel

1 INTRODUCTION

Transportation planners and policymakers in the 21st century often encourage people to walk, bike, and ride transit to mitigate a variety of social and environmental ills ranging from congestion to air pollution. Pro-transit and active transportation policies are particularly prevalent at many universities, which employ trip reduction and transportation demand management programs to support campus sustainability goals (Schmitt, 2013b), attract environmentally conscious students, ease opposition to new construction on campus (Schmitt, 2013d), and avoid building costly new parking structures (Schmitt, 2013a, 2013c).

In an effort to reduce driving, meet environmental goals, and encourage travel by other modes, universities, firms, and cities have implemented an astounding variety of behavioral change programs, also known as Voluntary Travel Behavior Change programs (Chatterjee & Bonsall, 2009). However, despite the proliferation of programs, post-program evaluations remain ad-hoc, which hampers our collective understanding of what types of programs are most effective at engendering car-free travel behaviors. Even when evaluations are conducted, many suffer from a number of serious methodological shortcomings that call into question the dramatic reported results (Bonsall, 2009; Stopher, Clifford, Swann, & Zhang, 2009). Some recent program evaluations that employ an experimental approach find travel behavior programs to be ineffective at changing travel patterns (see for example: Tørnblad, Kallbekken, Korneliussen, and Mideksa (2014)). Experimental results raise important questions about the validity of initial success stories and may lead policymakers to dismiss mobility management programs altogether.

Despite negative findings, we should not be too hasty to dismiss behavioral change programs. Once established, travel habits are notoriously difficult to change (H. Aarts & A. Dijksterhuis, 2000; H. Aarts & A. P. Dijksterhuis, 2000; Bamberg, Ajzen, & Schmidt, 2003; Gärling & Axhausen, 2003), but people are more willing to alter habits when they experience a major life change such as moving or changing jobs (Klöckner, 2004; Stanbridge & Lyons, 2006; van der Waerden, Timmermans, & Borgers, 2003). At the same time, behavioral change programs are unlikely to encourage people to leave their cars in areas with limited alternatives or in areas were the majority of people are already car-free. Together, this suggests that behavioral change programs may work, but only at the right time and place.

In this study, we seek to understand when and where behavioral change programs successfully alter behavior. To do so in a way that addresses methodological shortcomings from early evaluations, we conduct a true experiment. Incoming graduate students at the University of California, Los Angeles (UCLA) were randomly assigned to an experimental and control group. UCLA is ideal for a behavioral change program because incoming students are unlikely to be familiar with the excellent transit serving campus. In addition, incoming graduate students face a number of life changes and may be more susceptible than the general population to altering their travel patterns. A UCLA Transportation Guide was developed specifically for this experiment and was distributed via email to members of the experimental group in July, 2015. Relative to many other behavioral change programs, the treatment used here is low-cost. Twelve travel variables and three measures of residential location were analyzed to estimate the effect of the treatment on mode choice, trips to campus, vehicle miles traveled (VMT) and emissions. Finally, we explored how the treatment affected students differently depending on their previous residential location (changed residential location) or their automobile resources (automobile ownership, driver's license, and previous travel mode).

The following section contextualizes this study in the broader literature. Sections three and four detail the study methodology and results, respectively. Section five closes with a discussion and implications for future travel behavioral change programs.

2 BACKGROUND

2.1 TYPES OF BEHAVIORAL CHANGE PROGRAMS

A wide variety of behavioral change programs—which vary in their aims, target populations, and treatments—have been implemented and evaluated by transportation planners and policymakers. Behavioral change programs typically aim to make travel patterns more sustainable by either reducing vehicle miles traveled or miles driven alone (Fujii & Taniguchi, 2005), or encouraging public transit (Bamberg, 2006; Bamberg & Schmidt, 2003; Fujii & Kitamura, 2003)

Program target populations range from broad to specific. Broad-based programs are implemented at a neighborhood or even city-wide scale (see for example Cooper (2007) in King County, Washington USA; Beale and Bonsall (2007) in a suburb of Leeds, England; or Bachman and Katzev (1982) in Portland, Oregon) but are difficult to evaluate given hard-to-define target populations and evaluation efforts that typically focus on household or individual program participants (Cooper, 2007). By contrast, interventions that more narrowly target populations tend to be more straightforward to evaluate. Narrowly-targeted behavioral change programs are often deployed at the employer or university-level as both organizations often have incentives to reduce the number of people who drive alone to work and to meet VMT or emissions reduction goals. Abou-Zeid and Ben-Akiva (2012), Abou-Zeid, Witter, Bierlaire, Kaufmann, and Ben-Akiva (2012), Tørnblad et al. (2014), Duque, Gray, Harrison, and Davey (2014) evaluate employer-based programs in Switzerland and Boston, Sweden, Oslo, and New Orleans respectively. Universities, which also face pressure to reduce driving, have targeted university students in behavioral campaigns (see for example Fujii and Kitamura (2003) at Kyoto University and Rodriguez and Rogers (2014) at North Carolina State University and the University of North Carolina.

In addition to varying by target population, behavioral change programs also differ by treatment type. The simplest and least expensive programs offer information or promotional materials (see Rodriguez and Rogers (2014)). More intensive programs tailor these materials to each individual or household (Tørnblad et al., 2014). Other programs offer transit passes for a single day (Beale & Bonsall, 2007), month (Abou-Zeid & Ben-Akiva, 2012; Fujii & Kitamura, 2003; Thøgersen, 2009) or semester (Bamberg & Schmidt, 2003) to further incentivize participants to ride transit. Some programs go even further. The InMotion program in King County, Washington used a process known as *community based social marketing* to reduce driving to meet citywide VMT reduction goals. The extensive program included incentives, personalized travel planning, yard signs, and other merchandise (Cooper, 2007). InMotion participants also pledged to travel via public transit for two or more trips per week for two weeks (Cooper, 2007). Commitment as a behavioral change device is often tested alongside other conditions: commitment alone, a free bus pass alone, or commitment and a free bus pass (Bachman & Katzev, 1982; Matthies, Klöckner, & Preißner, 2006). Due to the methodological challenges described below, however, it remains unclear which components of behavioral change programs work.

Cost figures are difficult to find for many behavior intervention programs, but it is easy to imagine that they diverge widely. For example, tailored marketing programs likely cost more than un-differentiated marketing and a month-long transit pass costs more than a day pass. The costs can quickly escalate. The InMotion program cost between $12.82 and $56.77 per household and between $153 and $542 per participant (Cooper, 2007). By contrast, the treatment implemented in this study is very low-cost: a map of transit lines leading to UCLA and information on alternative travel modes delivered to participants via email.

2.2 THE CHALLENGE OF EVALUATING BEHAVIORAL CHANGE PROGRAMS

The evaluation of behavioral change programs is nearly as varied as the programs themselves. Many early programs were found to be remarkably effective at reducing driving and increasing transit use. For example, Cooper (2007) reports that the InMotion program in King County increased the share of respondents using public transit by 20 to 50 percent and decreased the share driving by 24 to 50 percent. Findings like these led to increased scrutiny of evaluation methods (Bonsall, 2009; Stopher et al., 2009). Overall, several factors may hamper the evaluation of program effects including self-selection bias, incorrect attribution of effects, failure to consider the counterfactual, a self-reporting bias, cross-contamination, small sample sizes, internal evaluation bias, and publication bias.

First and foremost are *self-selection* issues. In many evaluations, only people who voluntarily participated in the program were evaluated, which may overstate the treatment effect.

Another problem is an *incorrect attribution of effects*, in which observed changes are falsely credited to a behavioral change program but are in fact due to other causes. This arises as many behavioral change programs are implemented in conjunction with other initiatives, typically service improvements or the opening of a new transit line. This error tends to overestimate effects, because some people would have changed their behavior in response to service improvements even without the behavioral change program.

Relatedly, many analysts *fail to consider the counterfactual*. The counterfactual is the situation that would have occurred had the participants not received the treatment (i.e., had not received travel information). Program evaluators cannot directly observe the counterfactual because someone cannot be both treated and not-treated. An experimental research design is the gold standard to address this issue. The experimental group receives the treatment and a control group does not receive the treatment; therefore, as long as the treatment group and the control group are similar before the treatment, we can attribute any subsequent observed differences between the groups to the treatment. Moreover, because both the experimental and the control groups experienced the same broader changes in society (e.g., changes in fuel prices or transit service), all of the differences between the groups can be attributed to the treatment. Some previous travel behavior change programs include a control group (see for example: Bachman and Katzev (1982); Rodriguez and Rogers (2014); Matthies et al. (2006); Verplanken and Roy (2015)).

Self-reporting of travel patterns may lead analysts to over-estimate treatment results. Self-reporting is particularly problematic if the program required participants to pledged to travel more sustainably during the program as participants may feel pressure to overstate their performance due to the pledge.

While the preceding critiques focus on risks of over-estimating the treatment effect, it is also possible to underestimate the treatment effect, particularly if members of the control group inadvertently receive the treatment. The risks of so-called *cross-contamination* are greatest when the treatment is available through some means other than the experiment. For example, Rodriguez and Rogers (2014) used an experimental design to evaluate the effect of an apartment guide (information about travel and housing options) on subsequent travel patterns. The authors included a control group and, to prevent cross-contamination, asked the university transportation services to temporarily stop distributing the map to incoming students. Nevertheless, the guide was still available in the university transportation services office, increasing the risk of cross-contamination.

Another issue with many program evaluations is *small sample sizes* (Stopher et al., 2009). Some evaluations had as few as 30 participants (Abou-Zeid & Ben-Akiva, 2012). Analysts need larger samples to determine whether treatment effects are likely to persist or are statistical anomalies. Even larger samples are required when analyzing heterogeneous treatment effects (i.e., how the treatment affects distinct groups differently).

Moreover, in addition to the methodological issues raised above, Bonsall (2009) notes the potential for *internal evaluation bias* and calls for the importance of third-party evaluations. Many early evaluations with promising results were conducted by the private companies or public agencies in charge of administering the program, which could bias the results (Bonsall, 2009).

Finally, success stories are far more likely to be published or otherwise disseminated than evaluations of unsuccessful programs due to *publication bias* and a natural tendency to promote successes more than failures. For these reasons, reviews or meta-analyses of previous behavior change programs are likely to overstate the efficacy of interventions.

2.3 WHAT DO WE KNOW ABOUT BEHAVIORAL CHANGE PROGRAMS?

Given the above-mentioned methodological concerns, what do we know about behavioral change programs? We focus here on three central findings: first, behavioral change programs work best in areas where non-automobile travel options are abundant, but not widely used. Second, behavioral change programs are most effective when the participants are undergoing a major life change. Finally, two schools of thought—travel habit and incomplete information—yield insight into how behavioral change programs work to affect behavior.

2.3.1 WHERE DO THEY WORK?

Behavioral change programs are unlikely to be effective where there are few alternatives to the automobile; motivational messages, clear maps, and participant pledges cannot overcome poor service quality. Conversely, providing information is also unlikely to change travel behaviors where alternatives to the automobile are well-known and high-quality. In such areas, people likely know about the quality of the service and have already factored that into their travel decisions. As a result, providing information is likely most effective between these extremes, where new arrivals are misinformed or unaware about the available travel options. In reviewing previous programs, Bonsall concludes that behavioral change programs work best, “where the quality of non-car modes is unappreciated” (Bonsall, 2009, p. 308)

Few studies have tested the efficacy of an identical intervention across multiple sites. When cross-site comparisons have been made, the sample sizes are generally too small to determine whether the differences between the groups were the result of chance or intervention. Nevertheless, two studies provide suggestive evidence that an identical intervention can work effectively in one area but be less effective in others. For example, Abou-Zeid and Ben-Akiva (2012) found that 30 percent of Boston students switched to transit after receive a free bus pass. An identical intervention was ineffective in Geneva, Switzerland potentially because the target site appeared to be poorly served by transit: "17 percent of the sample [had] work schedules… outside the hours of operation of public transportation" (Abou-Zeid & Ben-Akiva, 2012, p. 51). Program participants simply cannot commute by public transit if service does not run when they travel to or from work.

Rodriguez and Rogers (2014) provided an informational packet to half of incoming students at two North Carolina Universities. The treatment led students at one university (North Carolina State University) to drive between 6.6 and 10.0 fewer kilometers each day. However, the treatment had no effect on the travel patterns of students at the other university (University of North Carolina Chapel Hill). Rodriguez and Rogers (2014) speculates that the observed difference in efficacy is due to a collection of factors including parking prices and transit availability. Another possibility is that cross-contamination was more prevalent at the University of North Carolina Chapel Hill than at North Carolina State University —meaning that more students in the control group had access to the informational packet and were therefore "treated" despite being in the control group—which could lead the authors to underestimate the true treatment effect.

2.3.2 When do they work?

While travel habits are notoriously difficult to change (H. Aarts & A. Dijksterhuis, 2000; H. Aarts & A. P. Dijksterhuis, 2000; Bamberg, Ajzen, et al., 2003; Gärling & Axhausen, 2003), exogenous stimuli—including disruptions, incentives, or new information—can change peoples' travel patterns (Fujii & Gärling, 2005; Fujii & Kitamura, 2003). Behavioral change campaigns appear to be most effective when the targeted population experiences a dramatic change in their situation—either they change jobs (Walker, Thomas, & Verplanken, 2015), schools (Rodriguez & Rogers, 2014), residential locations (Bamberg, 2006; Bamberg, Rölle, & Weber, 2003; Verplanken & Roy, 2015), or there is a prolonged change in transportation services (Fujii & Gärling, 2005). Moreover, evidence from mobility biographies and the life-course perspective reveal that changes in travel patterns often correspond to larger life changes such as the birth of a child (Lanzendorf, 2010). Together, this research suggests that *when* people are targeted may be as important as how they are treated.

2.3.3 How do they work?

Why might people who move be more willing to change their behavior? One school of thought, known as the habit discontinuity hypothesis, suggests that travel patterns are habitual and that habitual drivers reconsider their travel options only when they are forced into a period of reflection—when starting a new job, moving to a new home, or during a prolonged change in transportation services such as a bridge closure (Verplanken & Roy, 2015). The habit perspective suggests that individuals do not normally behave rationally when making day-to-day travel decisions. While they made a rational decision sometime in the past, they developed a simplified heuristic over time (Gärling & Axhausen, 2003).

Another interpretation is that individuals and households are indeed rational, but they may lack complete information about neighborhoods and their associated travel options. This lack of information may limit a household's ability to make optimal decisions about where to live and how to travel (Simon, 1972). Put simply, people cannot use transit service if they do not know that it exists. Search costs may be particularly high when moving to a new area. Under this framework, informational campaigns change behavior by lowering the search costs for discovering and understanding new travel options.

3 METHODOLOGY

3.1 STUDY AREA

Behavioral change programs work best where non-automobile travel options are abundant but are not widely recognized (Bonsall, 2009). For this reason, Los Angles may be the ideal setting for an intervention. The general public tends to regard Los Angeles as the car capital of the world, and many incoming graduate students likely assume that they will be unable to travel comfortably without a vehicle.

Despite these perceptions, the UCLA campus is well-served by alternative transportation options. Three transit agencies serve the UCLA campus: the Santa Monica Big Blue Bus serves UCLA with six routes and Los Angeles County Metropolitan Transportation Authority (LA Metro) and Culver City Bus together provide another five routes. In 2014, about 3,550 students (undergraduate and graduate) held subsidized public transit passes for one of these three agencies. With over 11,700 (86 percent) graduate students living off and commuting to campus, graduate students represent about 20 percent of all commuters to UCLA's campus. Of graduate and undergraduate students commuting to campus, about 26 percent drive to campus (UCLA, 2014).

Despite being well-served by transit, an incoming student may not recognize the abundance of transit options because search costs for transit information were high prior to this experiment. UCLA's Transportation Services did provide some transit information, but each bus route was provided on a separate map, which hampered students' ability to form a holistic understanding of the transit system. In addition, students had to proactively seek out information, which they may not have known existed. For many students, the informational packet may be the first concrete evidence that UCLA is well-served by transit. UCLA Transportation Services, which has a history of employing an experimental method to evaluate innovative programs (Brown, Hess, & Shoup, 2003), agreed to partner with us on this project.

3.2 EXPERIMENTAL DESIGN

Figure 1 outlines the experimental design used in this study. In May 2015, email contact information was obtained for all incoming graduate students from the UCLA Registrar's Office. We selected incoming graduate rather than undergraduate students because they are more likely to live off-campus (UCLA, 2014) and thus have increased housing choice and commuting needs. The students were randomly assigned to an experimental group (received information) or a control group (did not receive information). In July, students in the experimental group (n=1,583) received a UCLA Transportation Guide via email. Materials were sent in July when students begin to search for apartments for the start of classes in September. The control group did not receive the treatment.

In October, the entire incoming class of graduate students (n=3,122) was invited via email to complete a short transportation survey. An October survey allowed enough time to elapse since the start of the academic year (September), in which students could explore alternative travel options and develop a travel routine. We chose to survey students in October rather than in November or December when students are busy preparing for exams and final projects and may be less likely to respond to the survey.

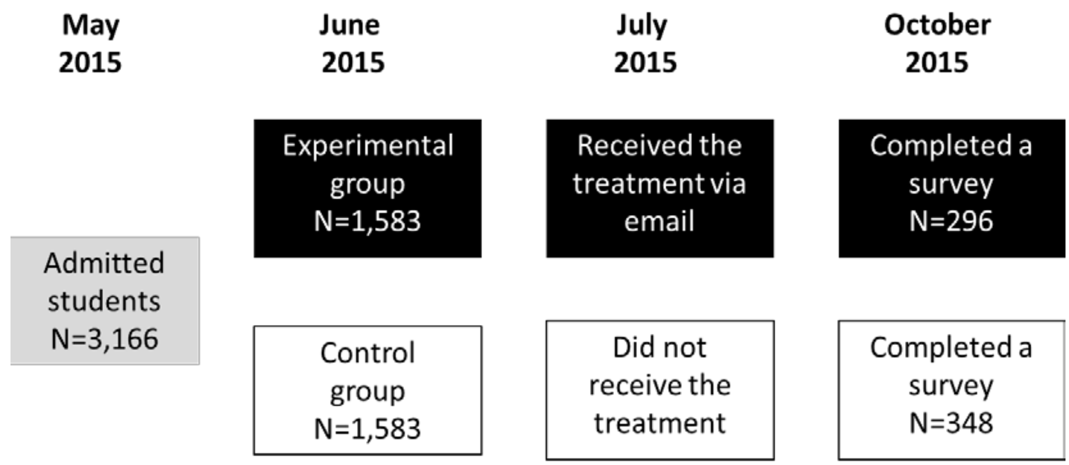


FIGURE 1 EXPERIMENTAL DESIGN

Note: 44 students who accepted admission in May 2015 did not matriculate in the fall.

3.3 TREATMENT: TRANSPORTATION INFORMATION GUIDE

In this experiment, the treatment is a UCLA Transportation Guide, developed exclusively for this study. UCLA Transportation Services staff members offered feedback on early versions of the Guide and it was made available to Transportation Services once the study was completed.

As Figure 2 shows, the Guide features a map of the UCLA campus and surrounding neighborhoods. The map includes each of the nine transit routes that serve campus. To maximize legibility, the map includes the same route branding (numbers and colors) as the transit agencies. Each route includes expected travel time to campus (less than 20 minutes, 20 to 40 minutes and, 40 to 60 minutes), which were obtained by consulting the route schedules of each transit agency.

The map includes a one-mile walk shed and three-mile bike shed in yellow to illustrate general distances. The scale of the map precluded the inclusion of specific walk and bike routes. The walk and bike sheds were calculated using Euclidian distances rather than network distances to highlight symbolic options rather than exact routing.

To further illustrate transportation options at UCLA, the Guide includes information on biking, parking, and transit passes available to students. Blue text represents interactive links that students could click to access more information. Finally, the map names different neighborhoods in Los Angeles to aid new students in aligning apartment searches with the Guide.

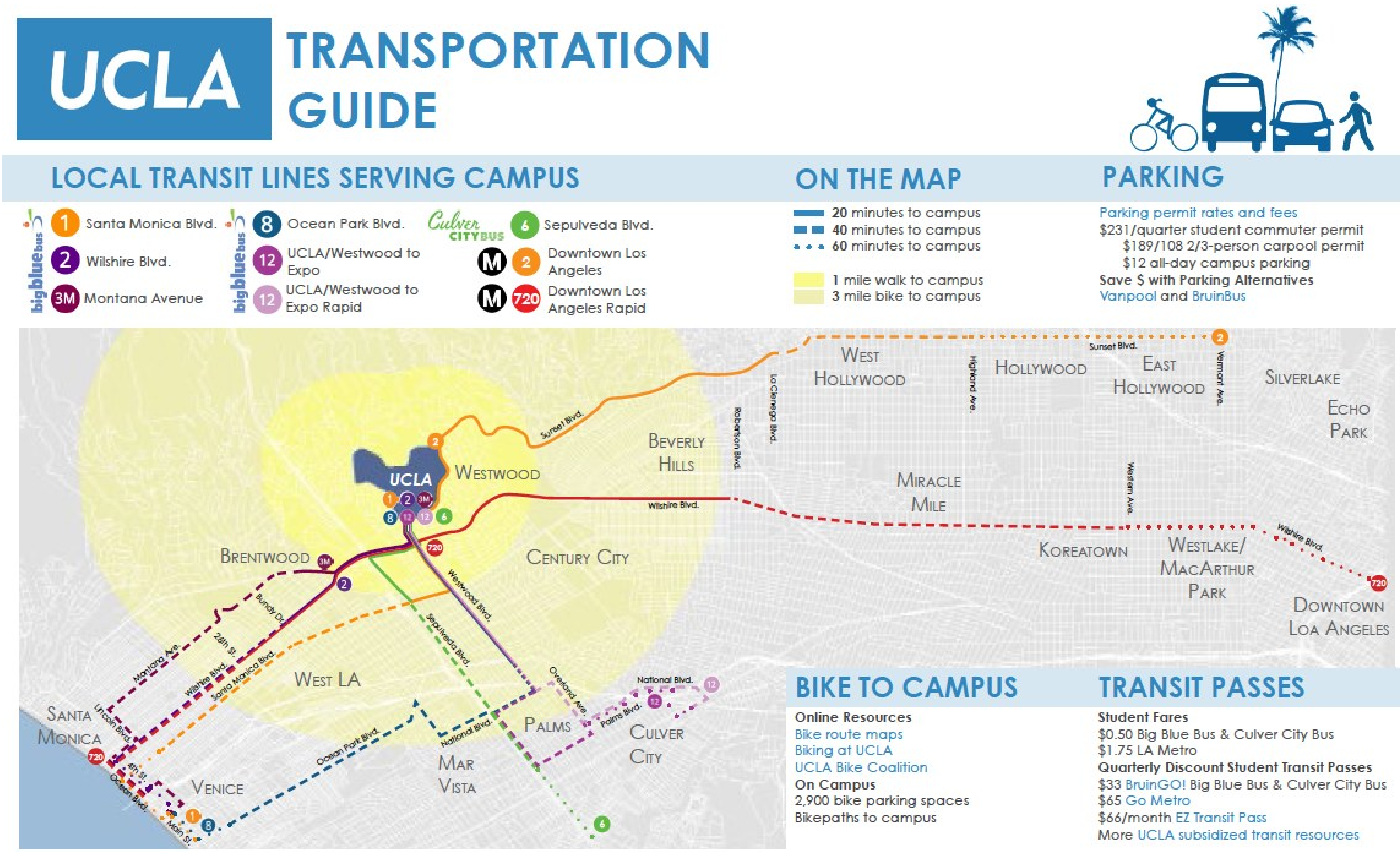


FIGURE 2 THE TREATMENT: A UCLA TRANSPORTATION GUIDE

3.3.1 Limiting cross-contamination
A field experiment is only valid if the analyst is confident that members of the control group did not inadvertently receive the treatment. As we mentioned above, we developed the Guide specifically for this experiment. Even with this precaution, it is still possible that members of the control group accessed the treatment somehow (e.g., a fellow student emailed it to them). To account for the possibility of cross contamination, we asked students at the end of the survey whether they remembered seeing the map. We excluded students from analysis if they "could not remember" seeing the map.

3.4 SURVEY INSTRUMENT
The survey instrument—on online survey sent via email—was distributed in October 2015. As we noted above, previous evaluations were constrained by small sample sizes (Bonsall, 2009). We judiciously limited the number of questions in the survey, sacrificing detail to maximize the sample size. In our email to students we emphasized the limited effort required to complete the "two-minute transportation survey". Our experimental design aided our quest for brevity; we were able to exclude many demographic variables because we do not have to control statistically for demographic characteristics as we would in an observational study using cross-sectional data. To further encourage participation, survey respondents were offered an incentive: a raffle for one $25 gift certificate. Survey questions were grouped into three categories: travel to school, residential location, and personal characteristics. UCLA Transportation Services staff reviewed the survey instrument and the survey was piloted with a limited number of second and third-year graduate students prior to implementation.

3.4.1 Travel Outcome variables
For simplicity, we focused on a single type of trip: the journey to school. A shortcoming of this approach is that we do not have information on other trips. It is possible that students who walk or ride transit to campus may drive more to other activities. We categorize students by their travel mode over a typical week: always drive (33%), drive sometimes (12%), always ride transit (17%), always walk or bike (15%), and use a mix of non-automobile modes (23%). A weekly measure reflects travel patterns more accurately than a daily measure because, as we describe in greater detail below, the number of weekly trips to campus is negatively related to distance from campus. As a result, total miles driven to and from campus in a week is not just a function of distance, but also a function of trip frequency.

3.4.2 Residential Location outcome variables
Next, we asked students about current residential location. Respondents were asked to provide their street number, street name, city, and zip code. We explained that this information would not be shared and would be used to determine their travel distance to campus. Because of potential concerns about privacy, we encouraged concerned students to provide the nearest cross-streets rather than a street number. To maximize overall survey response rate, respondents were not required to answer residential location questions. Of our 644 respondents, 600 (93 percent) provided their full street address or an identifiable cross street. We used ArcGIS to geocode the addresses and to calculate three residential location variables. First, we determined the travel distance to campus using the shortest network distance between a respondent's home and campus. Second, we calculated the network distance between a respondent's home and the nearest transit stop. Finally, to gauge the transit-richness of each

respondent's neighborhood, we counted the number of transit stops within one-half mile (network distance) of a respondent's residence.

Some respondents (n=20) provided a residential address 100 miles or more from the UCLA campus. Are these students traveling great distances or did they mistakenly report a permanent or parental address? These twenty students report coming to campus just once per week, which suggests that many of these students provided an accurate, current address and consolidate their classes to minimize their trips to campus. Despite this suggestive evidence, we explored a number of cut-off points for excluding outliers in our distance to school analysis. Because we could not determine the transit richness of the remote students' neighborhoods because our bus stop data was restricted to Los Angeles, we excluded remote students from the transit richness analysis. Remote students are included in the analysis of travel mode and number of trips.

3.4.3 Personal characteristics

To assess whether the treatment effect varied based on personal characteristics, we asked respondents about previous travel mode and previous residential location. We also asked whether the respondent was licensed to drive and whether they owned (or co-owned) a vehicle. The treatment could conceivably also shape whether the respondent owned a car and, to a lesser extent, whether the respondent was licensed to drive. Indeed, as Table 1 demonstrates, respondents in the treatment group were slightly less likely than those in the experimental group to own a car or to be licensed, albeit the differences were not statistically significant.

3.5 STUDY PARTICIPANTS

We received 812 completed surveys. We excluded two surveys because the respondents were not first year graduate students. Of the remaining 810 respondents, 296 respondents remembered receiving the treatment (the UCLA Transportation Guide). These respondents are included in the experimental group. Another 348 respondents reported that they did not receive the treatment. We refer to them as the control group. The 166 students who could not remember whether or not they had received the treatment could not be assigned to the treatment group or the control group and were excluded from analysis.

The treatment was randomly assigned, so members of the experimental and control group should be similar. Table 1, which provides information on the personal characteristics of individuals in the control and experimental groups, confirms that the two groups were indeed similar, although we observe slight differences as well. Members of the experimental group were slightly less likely than the control group to have moved in the past six months. Women were also slightly underrepresented in the experimental group relative to the control group.

Table 1 also provides reassurance that our method of identifying the treatment and control group did not selectively include or exclude particular types of students. The 166 students who could not remember whether they had received the treatment were broadly similar to members of the treatment and control groups.

Table 1 Personal characteristics by experimental group

	Control group (N=348)		Experimental group (N=296)		Unknown group (N=166)	
	Point estimate	Standard error	Point estimate	Standard error	Point estimate	Standard error
Previous residential location						
Did not move	31%	0.46	**21%**	**0.41**	33%	0.47
Moved from elsewhere in LA	10%	0.29	9%	0.28	10%	0.30
Moved from elsewhere in CA	18%	0.38	22%	0.42	13%	0.33
Moved from elsewhere in the U.S.	19%	0.40	24%	0.43	20%	0.40
Moved from outside the U.S.	22%	0.42	25%	0.43	24%	0.43
	100%		100%		100%	
Previous travel mode						
Previously drove	51%	0.50	50%	0.50	53%	0.50
Previously used transit	20%	0.40	20%	0.40	16%	0.37
Previously walked or biked	17%	0.37	18%	0.38	22%	0.41
Previously used a mix of modes	12%	0.32	13%	0.33	10%	0.30
	100%		100%		100%	
Female	59%	0.49	**47%**	**0.50**	**47%**	**0.50**
License	86%	0.35	84%	0.37	83%	0.38
Car	63%	0.48	59%	0.49	61%	0.49

Note**:** Values in bold were significantly different from the control group (p<0.05). LA is Los Angeles, CA is California, U.S. is United States.

3.6 ANALYTICAL APPROACH

Most travel behavior research requires complex statistical analysis to isolate a treatment effect and rule out other potential explanations for observed travel patterns. The experimental design employed here obviates the need for complex statistical analysis.

We began analysis by comparing the travel patterns of the control group and the treatment group. We follow the advice of Gerber and Green (2012) and determine the treatment effect by estimating a regression model with a single explanatory variable: treatment (yes/no). This approach allows us to estimate a 95 percent confidence interval for the treatment effect. The specific functional form of the regression model depends on the nature of the dependent variable: logistic regression for dichotomous outcomes (travel mode used in the past week), Poisson regression for count outcomes (number of trips to campus by each travel mode and number of bus stops within a half mile of home), ordinary least squares regression (miles by mode per week), negative binomial regression for over-dispersed count variables (distance to UCLA and distance to bus stop).

Given a widespread interest in encouraging sustainable travel modes, we consider how the treatment affected mode choice and thus VMT and emissions. The treatment could reduce miles driven to campus through three distinct pathways: 1) reduce the number of people driving to campus, 2) reduce the distance traveled to campus by drivers, or 3) decrease the number of trips to campus by drivers.[1] We use these treatment effect associated with each pathway to calculate total reductions in VMT. (see Table 2).

Finally, we also assessed whether the treatment affected various groups of students differently. To determine these so-called heterogeneous treatment effects, we added an interaction term to the regression model for each travel mode: drive always, drive sometimes, transit always, walk/bike always, and mix of non-automobile modes. We tested for heterogeneous effects among three dimensions: 1) residential relocation in the past six months (yes/no), 2) car ownership (yes/no), and 3) previous travel mode to work or school (drive/not drive). And finally, we assessed if and how the treatment affected residential location.

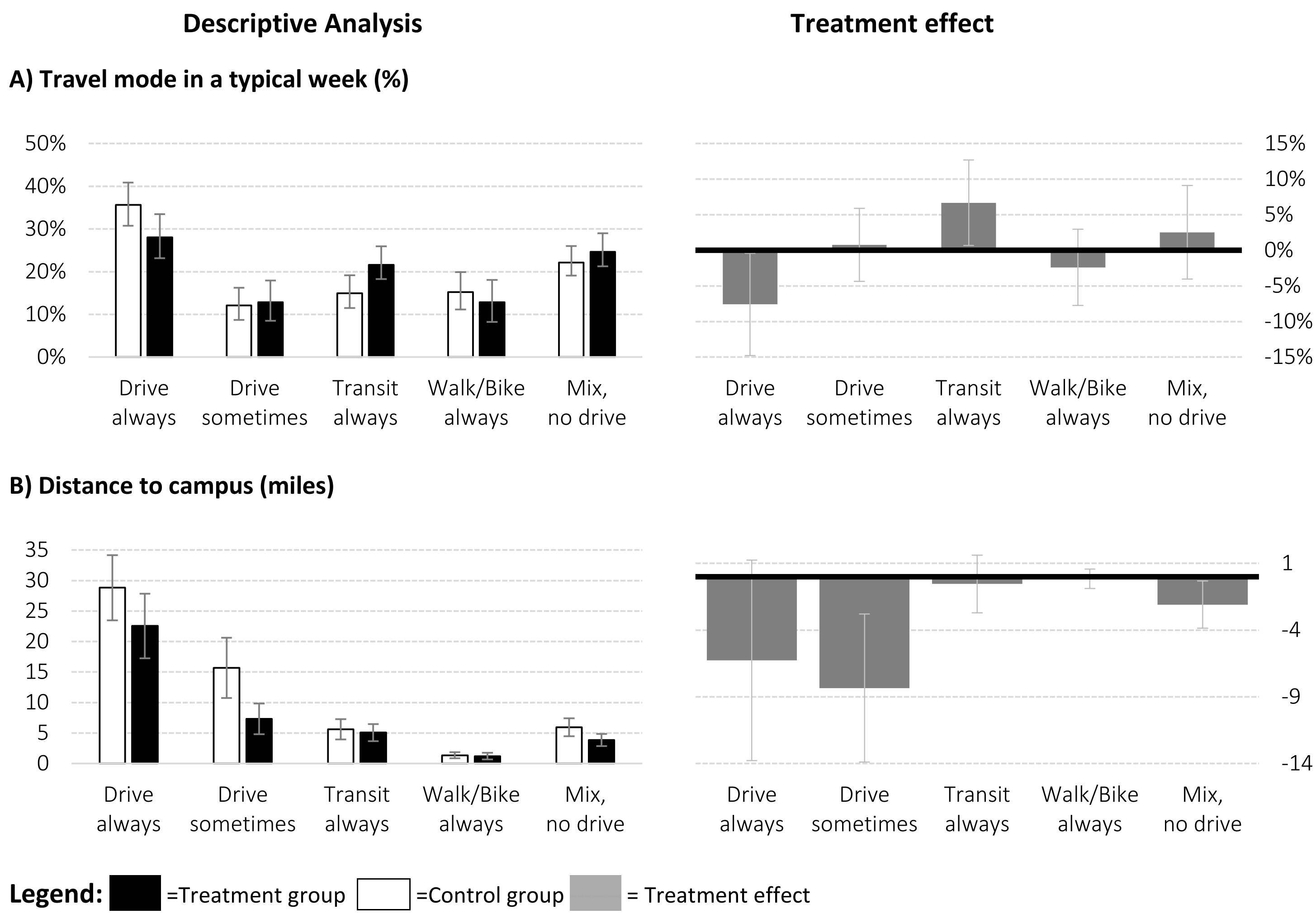
4 RESULTS

Figure 3 presents the results of the analysis for these three pathways with descriptive results on the left and model results on the right. The error bars reflect the 95 percent confidence interval around the estimate.

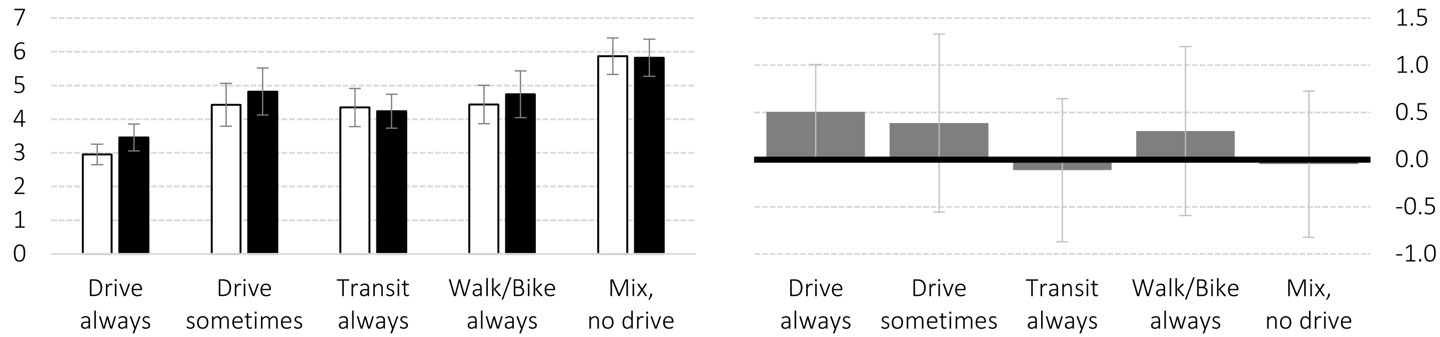
4.1 REDUCING DRIVING TO CAMPUS

The treatment decreased the proportion of students who always drive to campus from 36 percent in the control group to 28 percent in the treatment group. This difference of 7.6 percentage points was statistically significant [0.4 to 14]. The treatment had no discernable effect on the share of students who drove sometimes.

[1] A fourth pathway is possible. The share of miles by automobile by those who sometimes drive could differ between the treatment and control groups. However, this was not supported by the data.


Descriptive Analysis
Treatment effect
A) Travel mode in a typical week (%)
50%
40%
30%
20%
10%
0%
Drive always
Drive sometimes
Transit always
Walk/Bike always
Mix, no drive
15%
10%
5%
0%
-5%
-10%
-15%
Drive always
Drive sometimes
Transit always
Walk/Bike always
Mix, no drive
B) Distance to campus (miles)
35
30
25
20
15
10
5
0
Drive always
Drive sometimes
Transit always
Walk/Bike always
Mix, no drive
1
-4
-9
-14
Drive always
Drive sometimes
Transit always
Walk/Bike always
Mix, no drive
Legend:
=Treatment group
=Control group
= Treatment effect

C) Trips to campus per week (N)



Legend: =Treatment group =Control group = Treatment effect

FIGURE 3 TREATMENT EFFECT

Note: Treatment effect is regression coefficient for the variable indicating inclusion in the experimental group. Error bars reflect the 95 percent confidence interval. In (A), the share of respondents using each travel mode sums to more than 100 percent because some respondents used more than one travel mode over the course of a week. N=296 in the experimental group and n=348 in the control group in (A) and (B). Sample sizes were lower for (C) because not all respondents provided residential location information: sample sizes were n=280 and n=330 for the treatment and control groups respectively.

In addition to reducing the share of students who always drive to campus, the treatment decreased the average distance driven to campus from 28.8 miles in the control group to 22.6 miles in the experimental group. This difference of 6.3 [-3.8 to 13.8] miles was just shy of being statistically significant. Distance to school was sensitive to outliers. The median, which is not sensitive to extreme values, was equal for the control and experimental groups at 10.7 versus 10.3 miles respectively.

Given that drivers in the treatment group live closer on average to campus than the students in the control group, we would expect drivers in the treatment group to visit campus more often because people visit nearby destinations more often than distant definitions (Sheppard, 1995). This was supported by the data. Whereas drivers in the control group visited campus 3.0 times per week on average [2.6 to 3.3], drivers in the treatment group visited 3.5 times per week [3.1 to 3.9]. This finding underscores the importance of using a weekly, rather than a daily, measure of travel of travel behavior; a daily measure could not reveal differences in trips to campus per week and would lead us to overestimate the reduction in driving caused by the treatment.

Table 2 combines the preceding analysis to calculate the overall reduction in driving for the treatment group through each of the three pathways. Average vehicle miles of travel (VMT) is calculated by multiplying across the table:

*Total VMT = proportion of students * trips per week * distance to school * share of miles by automobile*

These values are then summed to find the average VMT of the treatment and control groups respectively. Students in the treatment group drove 23.6 miles per week on average, while students in the control group drove 33.6 miles, a difference of ten miles.

TABLE 2 CALCULATING VMT PER WEEK FOR THE AVERAGE MEMBER OF THE CONTROL AND EXPERIMENTAL GROUPS

	(A) Proportion of students	(B) Trips per week	(C) Distance to school	(D) Miles by auto (%)	(E) Average VMT
Control group					
Drive always	36%	3.0	28.8	100%	30.3
Drive sometimes	12%	4.4	15.7	39%	3.3
Transit always	15%	4.3	5.6	0%	0.0
Walk or bike always	15%	4.4	1.4	0%	0.0
Mix, no driving	22%	5.9	6.0	0%	0.0
Average VMT					**33.6**
Treatment group					
Drive always	28%	3.5	22.6	100%	21.9
Drive sometimes	13%	4.8	7.3	39%	1.8
Transit always	22%	4.2	5.1	0%	0.0
Walk or bike always	13%	4.7	1.2	0%	0.0
Mix, no driving	25%	5.8	3.9	0%	0.0
Average VMT					**23.6**

Note: Column E= A*B*C*D

Table 3 illustrates how individual-level differences in VMT scale up when aggregated across all of the students in the treatment group (n=296) and if they had been applied to all incoming graduate students (n=3,166). The mid-range estimate is based on the point estimate of VMT reduction between the experimental group and the control group—10 miles. The low estimate is half that amount—five miles—and the high estimate is fifty percent greater—15 miles. If the treatment had been applied to all incoming graduate students, total VMT could be expected to decline by about one million miles and 430 metric tons of $C0_2$ in a single year.

TABLE 3 TREATMENT EFFECT: ESTIMATED ANNUAL REDUCTIONS IN VMT AND CO_2 (TRAVEL TO CAMPUS ONLY)

	Per week reductions		Per year reductions	
	VMT (miles)	CO_2 (metric tons)	VMT (miles)	CO_2 (metric tons)
Students in the sample (n=296)				
Low estimate (5 miles per week)	1,480	0.6	48,840	20.1
Medium estimate (10 miles per week)	2,960	1.2	97,680	40.1
High estimate (15 miles per week)	4,440	1.8	146,520	60.2
All incoming graduate students (N=3,166)				
Low estimate (5 miles per week)	15,830	6.5	522,390	214.7
Medium estimate (10 miles per week)	31,660	13.0	1,044,780	429.4
High estimate (15 miles per week)	47,490	19.5	1,567,170	644.1

Note: Annual reductions assumed 33 weeks of coursework (three 11-week quarters). We assumed 411 grams of CO_2 per mile of travel. Source: (Environmental Protection Agency, 2014). Note: this analysis does not include CO_2 from students riding public transit.

4.2 INCREASING TRANSIT RIDERSHIP

The above sections discuss the reduction in driving to campus. If students in the treatment group drove less than students in the control group, how did they get to campus? Figure 3 illustrates that a larger share of students in the experimental group rode public transit to campus than those in the control group. While 22 percent of students in the experimental group always rode transit, just 15 percent of students in the control group did. This difference of 6.6 [0.6 to 12.6] percentage points was statistically significant.

Among students who always rode public transit, there was no difference in the number of trips to campus per week (4.3) or the average travel distance to campus (5.3 miles) between the experimental and control groups.

There were no meaningful or statistically significant differences between the experimental and control groups when it came to walking, biking, or using a mix of non-automobile modes to get to campus.

4.3 HETEROGENOUS TREATMENT EFFECTS

Figure 4 illustrates the heterogeneous treatment effects, that is, how the treatment varied depending on the characteristics of the student. The figure shows that the treatment did not affect the travel mode of students who did not move. While the relationship between the treatment and travel mode was generally stronger for students who moved in the past six months, the only statistically significant change was that the treatment increased the share of students who always use transit.

The figure also indicates that the treatment had no discernable effect on driving or transit use among students who do not own a car or who previously used non-automobile modes to get to work or school. By contrast, among students who own a car or previously drove, the treatment reduced the share who always drive and increased the share who always use transit or use a mix of non-automobile modes.

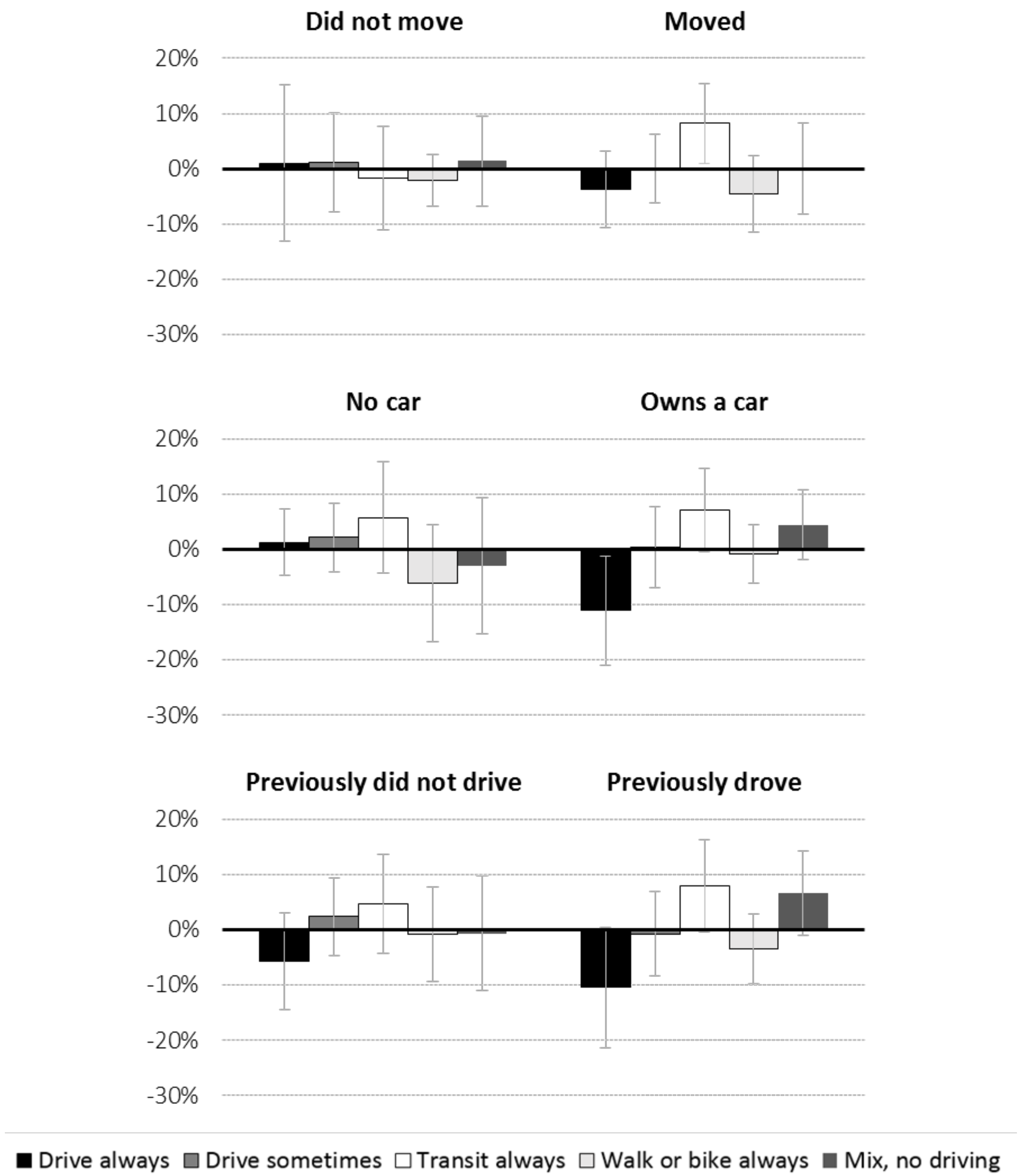


FIGURE 4 HETEROGENEOUS TREATMENT EFFECT ON TRAVEL MODE IN THE PAST WEEK

Note: Treatment effect reflects an interaction term between experimental group (yes/no) and one of three variables: Moved residential location in the past 6 months (yes/no), Owns a car (yes/no), or previously drove to work or school (yes/no). Error bars reflect the 95 percent confidence interval. Sample sizes for the experimental and control groups, respectively: Moved (240

& 233) and did not move (63 & 108); Previously drove (146 & 173) and did not previously drive (147 & 163); Owns a car (177 & 219) and does not own a car (119 & 129).

4.4 DID THE TREATMENT AFFECT RESIDENTIAL LOCATION?

Members of the treatment group were more likely to have changed residential location than members of the control group (78.7 versus 68.9 percent), a statistically significant difference of 9.7 percentage points [3.0, 16.5]. While the treatment could have spurred some students to move, it is also possible that students who planned to move may have been more interested in the treatment, may better recall receiving the treatment, and therefore may be more likely to be included in the treatment group rather than the control group (did not receive the treatment) or the unknown group (did not remember treatment). Whether the treatment caused students to move remains an open question.

When all students were included, the treatment was associated with living 5.3 miles closer to campus [3.3, 7.2]. However, analysis of mean distance to campus was very sensitive to outliers (i.e., students reporting residential locations far from campus). Several thresholds for identifying outliers were considered: highest 5 percent (exclude if greater than 41 miles), highest 2.5 percent (exclude if greater than 97.9 miles), highest 1 percent (exclude if greater than 134 miles), or outside of Los Angeles County. Regardless of the method used, no relationship existed between distance to school and treatment after excluding outliers.

Interestingly, according to some model specifications, the treatment appeared to increase distance to school for some students, namely those without a car or a driver's license, while the treatment decreased distance to school for students with a car or a license. Similarly, some model specifications indicated that the treatment increased distance to school for students moving from outside the United States. These results are tentative, however, because they were extremely sensitive to model specification.

The treatment may have led some students to select housing closer to campus, which may lead some students to visit campus more often than they would have without the treatment. This was supported by the data. Students in the control group made 4.2 trips to campus per week on average, whereas members of the experimental group made 4.5 trips—a difference of 0.3 trips per week [0.01, 0.7][2].

The treatment was not related to the other measures of residential location: distance to a bus stop or the number of bus stops within a half-mile of the home. This finding is somewhat unsurprising given the relatively uniform transit richness of many neighborhoods in Los Angeles popular among students owing to more affordable rents.

5 DISCUSSION

While an experimental approach like the one used here has a number of benefits, the approach is not without its limitations that may lead us to incorrectly identify the true effect of the behavioral intervention. In particular, we may overestimate the true effect of the treatment for a number of reasons. Foremost among these is that students who responded to a survey solicitation via email may be more likely than the average student to respond to an informational campaign delivered via email. If so, the true effect of the treatment on the general population of graduate students may be lower than that

[2] Based on the results of a Poisson regression with no control variables.

reported here. Second, we may overestimate the true treatment effect because we relied on self-reported recollection of the Guide to identify the experimental and control groups. Some members of the experimental group may have ignored the emailed Guide or may have promptly forgotten it. Our approach incorrectly assigns those respondents to the control group even though these students were treated and the treatment was ineffective for them. Future research would benefit from recording who did and did not receive the treatment to better determine the true treatment effect. A final caveat is that results for graduate students may not be generalizable to the general population.

With those caveats in mind, the single most important takeaway from this study is that by providing neighborhood and travel information to recent movers, policymakers may be able to modify travel behavior and achieve goals such as VMT and emissions reductions. Importantly, the behavioral intervention was only effective at changing the travel patterns of students who moved residential location. Along with evidence from mobility biographies (Lanzendorf, 2010; Scheiner & Holz-Rau, 2013) and travel habits (Bamberg, 2006), the finding that the treatment did not affect travel patterns among students who did not move provides further evidence that policymakers should target people when they change residences or are in the midst of other dramatic life changes.

Targeting recent movers will likely make behavioral change campaigns more cost-effective. This study demonstrates that low-cost, well-timed interventions like the UCLA travel guide can lead to relatively large reductions in VMT. The UCLA travel guide cost very little to produce and distribute and reduced VMT among the treatment group by nearly 100,000 miles over the school year, which equates to a cost of one cent per mile. By comparison, Cooper (2007) estimates that the InMotion program in Seattle, which included incentives, yard signs, posters, and personal communication, reduced VMT by an average of 2.4 miles per person per week. According to Cooper, VMT reductions in the InMotion program cost between $9.59 and $17 per mile.

We speculate that the treatment did not affect the travel patterns of students who do not because those students would have sought out information about non-automobile modes on their own with or without the Guide. The treatment made that search process easier and helped those car-free people move further away from campus than they would have otherwise. The finding that the treatment changed behavior for students with automobile resources is encouraging and suggests that driving habits may change when circumstances change and new information is provided. Similarly, the finding that the treatment increased transit use, despite similar levels of transit access between the control and experimental groups, supports the conclusion that people may choose transit, but first they need to know where it is and where it can take them. Information campaigns, therefore, can reduce search costs for information and may increase the likelihood that people choose car-alternative modes.

While students who received the treatment rode transit more to campus, they also walked and biked slightly less—an unexpected result. However, we speculate that this finding—like that for transit—can be attributed to the Guide filling an information void and thus increasing students' ranges of residential and travel choices. Students unfamiliar with the housing and transportation options in Los Angeles may select housing close to UCLA and walk to campus rather than purchase a car or navigate an unfamiliar transit system. The treatment introduced incoming students to housing and transportation options further from campus, where housing is more affordable. As a result, these students walked less to campus, but rode transit more. Together, the treatment's effect on walking/biking and transit use suggests that providing information fills a knowledge gap in available transportation options, which expands residential and travel choices.

The negative relationship found between distance from campus and number of trips taken per week to campus reinforces the importance of using weekly rather than daily measures of travel. Rodriguez and Rogers (2014) found that providing an Apartment Guide to undergraduate students in North Carolina reduced miles driven by 4.1 to 6.3 miles per day. Rodriguez and Rogers (2014) use some simple assumptions (5 trips per week and 37 weeks per year) to estimate the reduction in miles driven per year. Yet, the number of miles driven per week is not only a function of distance and travel mode, but also trip frequency, which we have demonstrated is negatively related to distance. For this reason, Rodriguez and Rogers' (2014) method of estimating annual savings is problematic. In fact, the treatment could conceivably *increase* the total number of miles driven to and from campus each week if the increase in frequency outpaced the reduction in distance. Our weekly measure provides a more accurate representation of annual decline in driving.

Our results reveal the necessity of a good experimental design. All students, regardless of whether they were in the treatment or control group, were less likely to drive and more likely to use transit at UCLA than they were at their previous job or university. If our research design had not included a control group, we would have mistakenly attributed all travel changes to the treatment. The presence of a control group separates this study from previous studies of relocation and travel behavior change, which are limited in their ability to draw causal inferences given the lack of randomization and prevalence of self-selection (Bonsall, 2009; Stopher et al., 2009)). Moreover, to better understand and improve future program efficacy, it is essential that policymakers and scholars share not only their success stories, but also the failures. Together, understanding why and how different programs succeed or fail can encourage cross-agency learning and help develop more consistently effective programs.

What can policymakers expect from travel behavior programs moving forward? First, interventions like the one used here work best in areas where transportation options exceed expectations (Bonsall, 2009), epitomized by the study area used here (UCLA campus). As a result, similar interventions in other locations may be less effective than the current study. On the other hand, policymakers may find that behavioral interventions are even more effective if they are offered to admitted students rather than incoming students. When choosing between programs, many admitted students likely consider their housing and transportation options along with the quality of the program, funding options, and other factors. Therefore, providing travel information to admitted students may attract students who prioritize car-free travel. Second, this study reveals the importance of targeting specific populations for travel behavior change programs, movers in particular. Movers are particularly susceptible to changing travel given new environments, options, and a reassessment of travel alternatives. Finally, this study demonstrates that information can change how people travel even among those living in similar built environments with similar access to transit. Not all travel behavior programs must be elaborate or expensive; simple information campaigns can encourage travel by bike and transit if they are targeted at the right place and the right time.

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