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The role of habit and residential location in travel behavior change programs, a field experiment

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Abstract Early evaluations of travel change programs demonstrated dramatic success in shifting people out of cars and into transit and active travel. Yet methodological shortcomings of early studies combined with newer more rigorous evaluations have called into question the dramatic early results. In this study, we use a randomized field experiment of incoming graduate students at the University of California, Los Angeles to answer two research questions. First, do travel behavior change programs work? And second, why do they tend to work for movers, but not non-movers? We test two competing hypothesized mechanisms for how travel interventions work: (1) by breaking travel habits during a period of self-reflection (habit pathway), or (2) by improving the transit quality of one's home neighborhood (residential location pathway). We find that a low-cost, informational program effectively altered the travel patterns of movers, but not non-movers. Overall, we find little support for the residential location pathway. Members of the treatment group did not live in significantly different neighborhoods compared to members of the control group. In addition, the treatment remained effective when controlling for residential location. This provides indirect evidence for the habit pathway, by which travel behavior programs influence travel behavior through information provided during periods of reflection. Behavioral change campaigns targeted at recent movers are likely just as effective as campaigns targeting those preparing to move as both groups are undergoing periods of reflection.

Keywords Field experiment · Travel mode choice · Transit use · Student travel · Behavior change

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Introduction

To ameliorate the harmful effects of automobile travel, many cities have introduced behavioral change programs to encourage people to shift from personal vehicles towards transit and active modes. Early program evaluations suggested dramatic effects—reporting, for example, 24–50 percent reductions in driving and between 20 and 50 percent increases in transit use (Cooper 2007). Yet many early evaluations suffered from a number of serious methodological shortcomings and newer more rigorous evaluations have called into question the dramatic early results (Bonsall 2009; Stopher et al. 2009). Based on the tepid results of more recent studies, should we lose faith in behavioral campaigns' ability to affect change?

In this study, we ask both *whether* travel behavior change programs work, and *how* they work. For example, previous studies found behavioral campaigns to be more effective for movers than non-movers (van der Waerden et al. 2003; Klöckner 2004; Stanbridge and Lyons 2006; Chatterjee et al. 2012; Clark et al. 2014, 2016; Müggenburg et al. 2015; Scheiner 2014; Scheiner and Holz-Rau 2013). Why might this be? According to the habit-discontinuity hypothesis, it takes a dramatic life change—such as moving—to bring about a period of reflection that is capable of breaking established travel habits (Verplanken and Roy 2016). We refer to this view as the habit pathway.

An alternative view is that what looks like habits are actually repeated rational behaviors. People may use a car for most trips because other modes are unavailable or less desirable. According to this view, informational campaigns affect movers (but not non-movers) because the treatment helps movers relocate to neighborhoods with better transit service and more opportunities for walking and biking. We call this the residential location pathway.

Which of these pathways explains how behavioral change programs work? Beyond academic curiosity, answering this question has important implications for policy, specifically the timing of targeted behavioral campaigns. Because of a general consensus about the habit pathway, behavioral change campaigns tend to target recent movers (Bamberg 2006; Walker et al. 2015; Verplanken et al. 2008). If the residential location pathway is also at work, then targeting people before a move might make behavioral change campaigns even more effective (Rodriguez and Rogers 2014; Taniguchi et al. 2014). To evaluate the efficacy of travel behavior change programs and the mechanisms by which they work, we provided transportation information to a random sample of incoming graduate students at the University of California, Los Angeles (UCLA). This experimental design overcomes many of the challenges, which we describe below, that plagued early program evaluations. This design also sheds light on why programs tend to be more effective for movers than non-movers. If we find that members of the treatment group live in more transit-rich neighborhoods than those in the control group, we would interpret that as evidence of the residential location pathway. If, however, we find that members of the treatment group use transit more and drive less than members of the control group *when they live in similar neighborhoods*, we would interpret that as evidence of the habit pathway.

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 and best practices for evaluating program efficacy.

Literature review

Issues with previous studies

Several factors hampered the evaluation of early behavioral change programs. Many studies suffer from *self-selection issues* by only evaluating those who voluntarily participated in the program. Volunteers may be more willing and able to change their travel patterns and if so, self-selection could lead scholars to overestimate the treatment effect on the general population. Some evaluations suffer from *incorrect attribution of effects*, where observed changes are falsely credited to a behavioral change program but are in fact due to other causes such as improved transit service. Most pressingly, many analysts *fail to consider the counterfactual*—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. The present study employs an experimental design to overcome self-selection, incorrect attribution of effects, and failure to consider the counterfactual.

In this research, we also sought to minimize evaluation issues related to small sample sizes, cross-contamination, and internal evaluation bias. *Small sample sizes* often hamper researchers' ability to determine if results are robust or statistical anomalies. As a result, we took steps to collect information from a suitably large sample. *Cross-contamination* may occur if members of the control group inadvertently receive the treatment. The treatment in the present experiment was not available publicly, which helped minimize cross-contamination.

Finally, success stories are far more likely to be published or otherwise disseminated than evaluations of unsuccessful programs. As a result of this *publication bias*, a review of the literature is likely to overstate the efficacy of behavioral change programs. Publication bias occurs in both social science and experimental research, with negative results as the most commonly cited reason for lack of publication or promotion (Dickersin 2005).

Conceptual framework

Figure 1 introduces the conceptual framework for this paper. Based on the previous literature, we expect the treatment to affect movers and non-movers differently. The conceptual framework helps illustrate why that may be the case.

We focus on two pathways, or mechanisms, through which information might influence travel patterns differently for movers and non-movers. According to the habit discontinuity hypothesis, travel patterns are habitual and made by a serious of simple heuristics (Gärling and Axhausen 2003; Bamberg et al. 2003; Aarts and Dijksterhuis 2000a, b). Relocating to a new neighborhood causes movers to enter a period of reflection during which they reexamine habitually executed travel patterns. In other words, a residential move could help people to break their established travel behaviors. During this reflection period, materials from behavioral change programs (such as information or motivational content) may provide critical information about new travel options.

Alternatively, others believe that travel is not habitual, but rather a series of repeated rational behaviors (De Vos et al. 2012; Schwanen and Mokhtarian 2005). In other words, most individuals drive for nearly every trip, not because of habit, but because driving is the most convenient option in their current environment. According to this view, in order to change travel patterns, we do not have to help people break habits, we have to help them

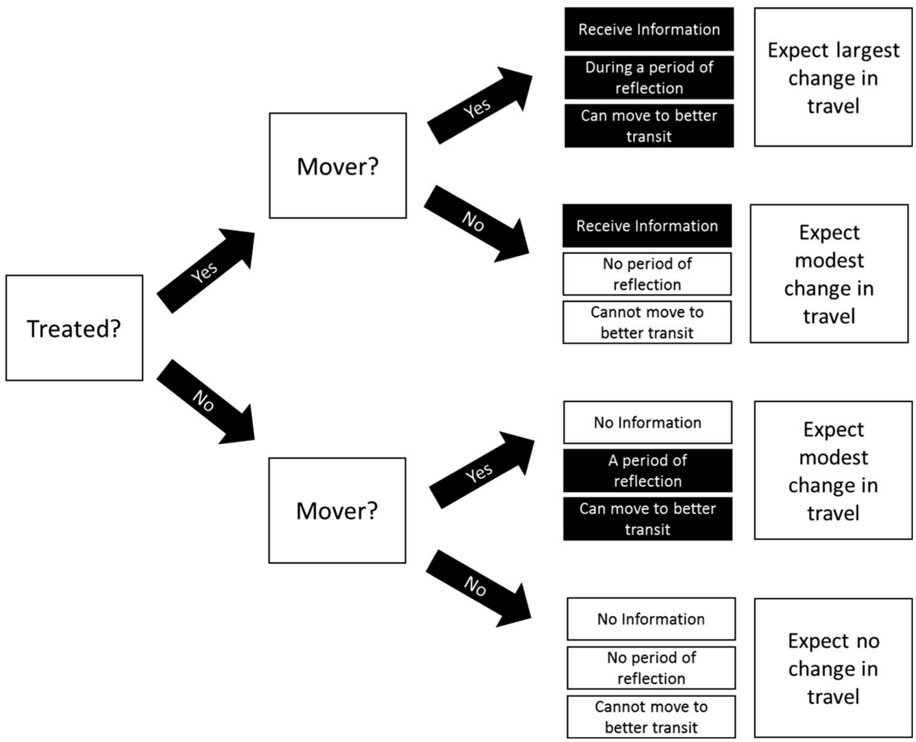

Fig. 1 Conceptual framework: How do informational campaigns change travel patterns for movers? Note: Black boxes indicate the expected causal pathways in each scenario

move to a more amenable environment. Under this pathway, which we term the residential location pathway, behavioral change programs may affect movers by influencing decisions about where specifically to move. According to this view, behavioral campaigns lower search costs and enable households to select homes in areas that match their travel preferences, such as near transit stops or within walking distance of work or school.

Both pathways have a clear explanation for why behavioral change campaigns fail to affect non-movers. According to the habit pathway, non-movers do not enter a period of reflection and are therefore less open to new information about travel patterns. According to the residential location pathway, by definition non-movers do not have an opportunity to move to neighborhoods with better transit or more opportunities for safe walking and cycling.

Methodology

Study area

Behavioral change programs are thought to work best where non-automobile travel options are abundant but are not widely recognized (Bonsall 2009). Los Angeles epitomizes this type of location. Many incoming students at UCLA likely assume that they will be unable to travel comfortably without a vehicle. Yet the campus is well-served by public transit[1] and just one-quarter of students commute to campus by car (UCLA 2014). Search costs for transit information were high prior to this experiment and as a result many incoming students likely underestimated transit service on campus.[2]

Treatment

In this experiment, the treatment is a UCLA Transportation Guide, developed exclusively for this study.[3] As Fig. 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 depicts expected travel time to campus (< 20, 20–40 and, 40–60 min), which were obtained by consulting route schedules.

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 detail 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 facilitate apartment searches.

Experimental design

In May 2015, we obtained email contact information for all incoming graduate students (N = 3166) 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, n = 1583) or a control group (did not receive information, n = 1583). In July, students in the experimental group received a UCLA Transportation Guide via email. In October, the entire incoming class of graduate students (n = 3122[4]) was invited via email to complete a short transportation survey.[5]

[1] As of 2015, three transit agencies serve the UCLA campus: the Santa Monica Big Blue Bus served UCLA with six routes and Los Angeles County Metropolitan Transportation Authority (LA Metro) and Culver City Bus together provided another five routes. In 2014, about 3650 students (12% of commuting students) held subsidized public transit passes for one of these three agencies (UCLA 2014).

[2] UCLA's Transportation Services previously provided web-based transit information, but bus routes were shown on separate maps, 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.

[3] 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.

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

[5] An October survey allowed enough time for students to 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.

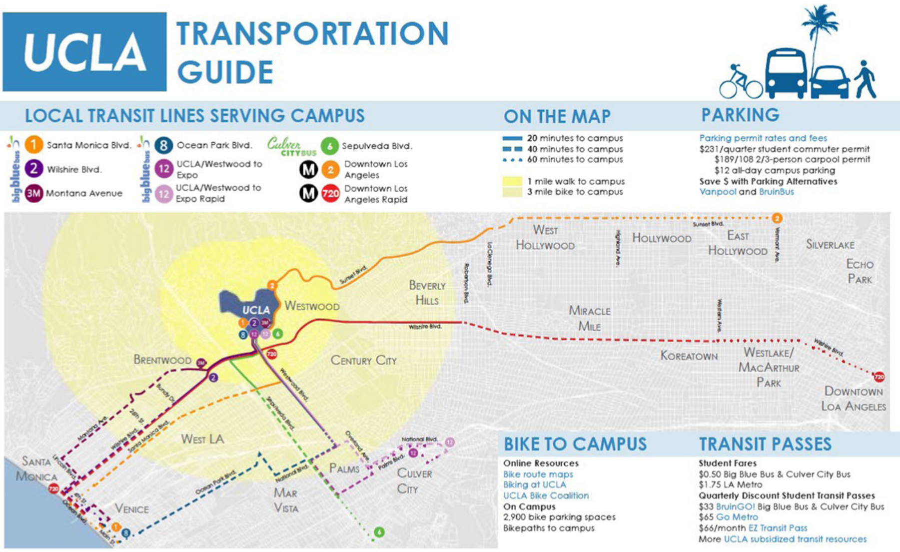

Fig. 2 The treatment: UCLA transportation guide

Survey instrument

In designing a survey, scholars must trade-off two competing desires: (1) collect rich, comprehensive data and (2) obtain a suitably large sample. As we noted above, previous evaluations were constrained by small sample sizes (Bonsall 2009; Rodriguez and Rogers 2014). As a result, we prioritized sample size and sacrificed survey detail. While this choice increases the risk of issues related to non-response bias, we felt that random assignment of the treatment lessened the need for multiple demographic questions. Nevertheless, we recommend that scholars consider this trade-off carefully.

The survey instrument—an online survey sent via email—was distributed in October 2015.[6] To encourage participation, we emphasized the limited effort required to complete the "two-minute transportation survey". Survey respondents were also 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. For simplicity, we focused on a single type of trip: the journey to school. Students were asked, "To get TO CAMPUS in a typical week, I: (travel mode) (number) time(s)." The number of trips per week ranged from zero to "five or more times." We believe that a weekly measure reflects travel patterns more accurately than a daily measure. For example, total miles driven to campus is a function of distance and frequency, and we find that students who live far from campus tend to visit campus less frequently.

We analyze three travel modes: transit, automobile, and active travel (walking or biking).[7] For each mode, we measure travel in three ways: (1) the share of students who

[6] 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.

[7] Survey travel mode response options included: Drive (alone), Drive (carpool), Passenger (carpool), Ride public transit, Walk, Bike, Other.

always use that mode, (2) the share who use that mode at least once in a typical week, and (3) the number of trips to campus by that mode per week. We also included a tenth measure: miles of travel by automobile to campus per week.

A shortcoming of our focus on school travel is that we do not have information on other trips and it is possible that students who walk or ride transit to campus may drive more to other activities. On the other hand, an intervention for one type of trip may make respondents *more* willing to use transit for non-school trips by increasing students' familiarity with the system or by lowering the marginal cost of additional trips. If so, the effects of a narrowly targeted travel change program may spillover to amplify travel change benefits.

To analyze residential location, we asked students to provide their street number, street name, city, and zip code.[8] Of our respondents, 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.

Study participants

We received 810 completed surveys from first-year graduate students (response rate: 25.9%). For reasons outside our control, we could not track which students received the treatment and which students did not. Instead, we relied on self-reported recollection of the treatment to identify the experimental and control groups. This led to three groups: those who remembered seeing the Guide (the experimental group, n = 296), those who did not see the Guide (the control group, n = 348), and those who could not remember whether they saw they map (the unknown group, n = 166). We excluded the unknown group from analysis. While this method of identifying the treatment and control groups is far from ideal, it was the only approach available to us.

Because treatment was randomly assigned, we would expect members of the treatment and control groups to be similar. This was generally true. According to a series of t-tests, members of the treatment group and control groups were equally likely to own a car, be a licensed driver, and to have commuted by car to their previous job or school. There were, however, two primary differences between the groups: women were slightly underrepresented in the experimental group (47 vs. 59%, $p < 0.05$) and members of the experimental group were more likely to have moved in the past 6 months (79 vs. 69%, $p < 0.05$). Such systematic differences can bias the results and lead us to overstate the effectiveness of the treatment. For that reason, in the subsequent analysis we control statistically for differences between the treatment and control groups.

We only analyze respondents with complete information for all of the dependent (travel mode) and independent variables (mover status, residential location, and sex). This leaves a sample of 561 respondents, 260 in the experimental group and 301 in the control group.

[8] 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.

Analytical approach

This paper has two aims: first, to evaluate whether a behavioral change program at UCLA worked while addressing many of the issues plaguing previous evaluations; and second, to determine why behavioral change programs might work for movers, but not non-movers. To tackle the first aim, we estimated ten regression models, one for each dependent variable described above. We refer to these models as Model 1. Each model includes an interaction term between treatment status and mover status (which allows the effect of the treatment to differ for movers and non-movers), as well as a control variable for sex (because there were slightly more females in the treatment group than in the control group). We estimated logistic regression models for the dichotomous dependent variables (share using each mode ever or always) and a Poisson regression model for the number of trips per week and miles driven by automobile.

Because it can be difficult to interpret coefficients for models with interaction terms, we present the average marginal effect of the treatment in Table 1. Full model results are presented in Tables 3 and 4 (along with Model 2, described below). We present the marginal effect of the treatment for the sample as a whole (Experimental group vs. Control group), for movers only (Experimental group, moved vs. Control group, moved), and for non-movers only (Experimental group, did not move vs. Control group, did not move).

For the second aim, we sought to differentiate between the habit pathway and the residential location pathway. First, we compared the residential location of the treatment and control groups. If the residential location pathway is at work, we would expect students in the treatment group to live closer to bus stops and have more bus lines nearby than those in the control group. For this reason, we use a one-tailed test for those variables. We had no expectation a priori whether the treatment would increase or decrease distance to campus, so we use a two-tail test. For all three tests, we analyzed movers and non-movers separately.

Table 1 The marginal effect of the treatment and the marginal effect of moving

	Marginal effect of the treatment					Marginal effect of moving	
	Among full sample	Among movers only		Among non-movers only		Among full sample	
Transit always (%)	+ 5.3%	+ 7.5%	*	− 2.6%		+ 8.9%	**
Transit ever (%)	+ 6.0%	+ 7.8%	*	− 0.2%		+ 29.8%	***
Transit trips (#)	+ 0.18	+ 0.24	*	− 0.02		+ 1.14	***
Drive always (%)	− 3.6%	− 6.0%	*	+ 4.9%		− 54.1%	***
Drive ever (%)	− 2.7%	− 4.6%		+ 3.9%		− 49.8%	***
Drive trips (#)	− 0.04	− 0.20	**	+ 0.50	*	− 1.58	***
Drive miles (#)	+ 1.04	− 0.84	***	+ 7.69		− 23.88	***
Walk/bike always (%)	− 3.7%	− 3.8%		− 3.0%		+ 13.5%	***
Walk/bike ever (%)	− 5.0%	− 6.7%		+ 1.2%		+ 41.1%	***
Walk/bike trips (#)	− 0.14	− 0.15		− 0.13		+ 1.50	***

*0.10; **0.05; ***0.01

Results of multivariate regression comparing travel patterns of the treatment group to the control group

Next, we added residential location variables to each of the ten regression models described above. We refer to these models as Model 2. The three residential location variables are: distance to campus (log transformed), transit richness, and distance to a bus stop. By comparing the coefficients for Model 1 and Model 2, we can determine whether the habit pathway, the residential location pathway, or both are at work. If the treatment coefficients were significantly smaller (or insignificant) in Model 2 relative to Model 1, that would be evidence for the residential location pathway. If the treatment coefficients remain significant in Model 2, we can conclude that, above and beyond any changes to residential location, the treatment works by directly influencing travel behavior. To test the size of the marginal effect across models we followed the advice of Mize et al. (in progress)[9] by fitting a joint model (using the gsem command), estimating the marginal effects for each model separately (using the margins command), and comparing the magnitude of the effects (using the mlincom command).

The regression results are presented separately for transit variables (see Table 3) and the drive variables (see Table 4). We do not present the active travel models because the treatment did not affect active travel in a statistically significant or meaningful way.

Caveats

Before presenting the results, we acknowledge important caveats of this work. We may overestimate the true effect of the treatment for two reasons. First, some members of the experimental group may have ignored the emailed Guide or may have promptly forgotten it. If so, our approach incorrectly assigns those respondents to the control group or unknown group even though these students were ineffectively treated. Future research should record who did and did not receive the treatment. Second, students who responded to a survey solicitation via email may be more likely than the average student to respond to an emailed informational campaign. If so, the true effect of the treatment on the general population of graduate students may be lower than that reported here. Finally, as we discussed above, scholars should carefully trade off the need to collect demographic information to check for differential non-response and the need to collect a large sample.

Results

Table 1 presents the marginal effects of the treatment for the full sample, movers only, and non-movers only. Recall that these results control for gender and that full model results are available in Tables 3 and 4 below. For the sample as a whole, the treatment did not affect travel patterns in a meaningful or statistically significant manner. The treatment did, however, increase transit use and decrease driving for movers. By contrast, the treatment did not affect the travel patterns of non-movers. When it comes to active travel, the treatment did not affect travel in a statistically significant manner for either movers or non-movers.

Specifically, among movers, the treatment increased the share of respondents who always or ever use transit by roughly eight percentage points and increased transit trips to campus by 0.24 trips per week on average. When it comes to driving, the treatment reduced

9 Mize, Trenton, Long Doan, J. Scott Long. (in progress) “A General Framework for Comparing Marginal Effects Across Models”.

the share of students who always drive to campus by 6.0 percentage points and reduced vehicle trips to campus per week by 0.2 trips.

Table 1 also depicts the marginal effect of moving for each of the ten dependent variables. In general, movers tended to travel much differently than non-movers. Relative to students who did not move, those who moved were much more likely to ride public transit (ever or always) or use active modes (ever or always) and were much less likely to drive to campus (ever or always). On average, students who moved made 1.1 more trips per week by transit and 1.5 more trips by active travel compared to non-movers. Movers also made 1.6 fewer trips per week by vehicle and drove 23.9 fewer miles per week to campus.

We turn now to aim 2, to weigh the evidence for the habit pathway and the residential location pathway. As Table 2 shows, we find no differences in the residential location of respondents in the treatment and control groups. This was true when we analyzed movers and non-movers separately (pictured) and for the sample as a whole (not pictured). This finding is contrary to the residential location pathway, in which we would expect treatment to affect travel by influencing where people choose to live.

Table 2 also demonstrates that regardless of their treatment status, students who moved live in more transit-rich locations, live closer to bus stops, and live closer to campus than non-movers. With one exception, each of these differences between movers and non-movers was meaningfully large and statistically significant.

Tables 3 and 4 allow us to compare the marginal effect of the treatment in Model 1 (with no residential location variables) and Model 2 (with residential location variables). Overall, adding residential location variables did not affect our estimate of treatment efficacy in a statistically significant or meaningful manner (not pictured). This suggests that

Table 2 Residential location characteristics by treatment status and mover status (n = 561)

	Control group	Experimental group	Sig. (treatment vs. control)
Bus stops within ½ mile			
Movers	31.3	29.9	n.s.
Non-movers	22.9	17.9	n.s.
Sig. (movers vs. non-movers)	***	***	
Distance to a bus			
Movers	0.17	0.22	n.s.
Non-movers	0.45	0.26	n.s.
Sig. (movers vs. non-movers)	***	n.s.	
Distance to campus			
Movers	3.9	4.5	n.s.
Non-movers	24.5	19.7	n.s.
Sig. (movers vs. non-movers)	***	***	
Count			
Movers	220	214	
Non-movers	81	46	

Statistical significance determine by t-tests for differences in means. The tests for number of transit stops and distance to a bus are one-tailed and the test for distance to campus is two-tailed. Stars denote statistical significance: ***indicates $p < 0.01$

n.s. indicates not statistically significant at $p < 0.10$

Table 3 Regression results for transit variables (n = 561)

	Transit always				Transit ever				Transit trips per week			
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2	
Treatment effect												
Movers only	+ 7.5%	*	+ 6.7%	*	+ 7.7%	*	+ 7.8%	*	+ 0.24	*	+ 0.24	*
Non-movers only	− 2.6%	n.s.	− 3.5%	n.s.	− 0.8%	n.s.	0.0%	n.s.	− 0.02	n.s.	− 0.02	n.s.
Full model												
Treatment												
Control group, moved	0.33	n.s.	0.77	*	1.19	***	1.19	***	0.84	***	0.821	***
Exp. group, did not move	− 0.24	n.s.	− 0.33	n.s.	− 0.01	n.s.	0.00	n.s.	− 0.03	n.s.	−0.02	n.s.
Exp. group, moved	0.78	**	1.18	***	1.51	***	1.51	***	0.96	***	0.95	***
Residential location												
Distance to campus			0.36	***			0.10	n.s.			0.05	n.s.
Distance to bus stop			− 1.06	*			− 0.98	*			− 0.61	***
Transit richness			− 0.17	n.s.			0.07	n.s.			0.04	**
Control variable												
Female (base: male)	0.42	*	0.42	*	0.34	*	0.34	*	0.13	**	0.13	*
Constant	− 2.10	***	− 2.38	***	− 1.45	***	− 1.58	***	− 0.33	**	− 0.37	**
Model Fit												
Pseudo-R2	.021		.056		.053		.066		0.39		.048	
Chi Squared	11.39		30.81		41.12		51.31		94.58		116.83	
p	0.023		0.000		0.000		0.000		0.000		0.000	
AIC	551.1		537.6		741.9		737.8		2325.7		2309.5	

Distance to campus is log transformed

(*0.10; **0.05; ***0.01)

Table 4 Regression results for drive variables (n = 561)

	Drive always				Drive ever				Drive trips per week				Miles driven per week			
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2		Model 1		Model 2	
Treatment effect																
Movers only	− 6.0%	*	− 7.2%	*	− 4.6%	n.s.	− 7.2%	*	− 0.20	**	− 0.25	***	− 0.84	n.s.	− 1.87	**
Non-movers only	+ 4.9%	n.s.	+ 2.9%	n.s.	+ 3.9%	n.s.	+ 2.9%	n.s.	+ 0.50	*	+ 0.46	n.s.	+ 7.69	n.s.	+ 5.55	n.s.
Full model																
Treatment																
Control group, moved	− 2.20	***	− 0.92	***	− 2.00	***	− 0.92	***	− 0.85	***	− 0.11	n.s.	− 1.35	***	− 0.23	n.s.
Exp. group, did not	0.24	n.s.	0.20	n.s.	0.23	n.s.	0.20	n.s.	0.206	*	0.19	n.s.	0.25		0.13	n.s.
Exp. group, moved	− 2.64	***	− 1.36	***	− 2.23	***	− 1.36	***	− 1.08	***	− 0.40	***	− 1.48	***	− 0.53	***
Residential location																
Distance to campus			1.10	***			1.10	***			0.59	***			1.61	***
Distance to bus stop			0.23	n.s.			0.23	n.s.			− 0.05	n.s.			− 0.04	n.s.
Transit richness			0.08	n.s.			0.08	n.s.			0.02	n.s.			0.03	n.s.
Control variable																
Female (base: male)	− 0.07	n.s.	− 0.34	n.s.	− 0.20	n.s.	− 0.34	n.s.	− .075	n.s.	− 0.09	n.s.	− 0.01		− 0.14	n.s.
Constant	0.81	***	− 1.03	**	1.28	***	− 1.03	**	0.82	**	− 0.59	***	3.29	***	− 0.47	**
Model fit																
Pseudo-R2	0.194		0.344		0.134		0.268		0.082		0.173		0.188		0.637	

Table 4 continued

	Drive always		Drive ever		Drive trips per week		Miles driven per week	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Chi Squared	129.84	200.98	100.83	200.98	168.51	354.78	3645.39	12,329.26
p	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
AIC	549.0	565.5	659.7	565.5	1892.1	1711.9	5143.1	3862.7

Distance to campus is log transformed

(*0.10; **0.05; ***0.01)

the treatment did not work by changing characteristics of the residential location of respondents. Nevertheless, residential variables improved the fit of all of the models and generally perform as expected. For example, distance to a bus stop is negatively associated with all three measures of transit use. In other words, the closer one lived to a transit stop, the more they used transit.

Conclusion

Like other authors (Bamberg 2006; Walker et al. 2015; Verplanken et al. 2008), we find that a low-cost, informational intervention effectively altered the travel patterns of movers, but not non-movers. The analysis demonstrates the importance of methodological rigor in analyzing behavioral change programs. The results presented here are more sober than the dramatic findings from early behavioral change programs (Cooper 2007), but nevertheless demonstrate the potential for such programs to alter travel patterns. The potential to change travel behavior is alive and well, but policy makers should implement interventions at critical life events, such as just before or after a move.

In addition to testing behavioral program efficacy using a robust experimental method, a central aim of this work was to explore how and why treatments tend to affect movers, but not non-movers. We focused on two causal pathways: the habit pathway and the residential location pathway. Overall, we find little support for the residential location pathway. Members of the treatment group did not live in significantly different neighborhoods than members of the control group. In addition, the treatment remained effective when controlling for residential location. This provides indirect evidence for the habit pathway, by which travel behavior programs influence travel behavior by providing information during periods of reflection.

These results have important implications for behavioral change programs. Because the treatment does not appear to work via the residential location pathway, behavioral change campaigns targeted at recent movers are likely just as effective as campaigns targeting those preparing to move as both are undergoing periods of reflection. This is good news because identifying recent movers is far more straightforward than identifying people preparing to move. The bad news is that the efficacy of behavioral change campaigns depends on how many people move; the more movers in the target population, the more successful the campaign is likely to be. Other life changes may not be sufficient to enter a period of reflection. After all, all of our respondents experienced a major life change—starting graduate school—but the treatment only led movers to change their travel patterns.

Because the results presented here reflect the outcome for a particular population (first-year graduate students) at a particular location (UCLA), results with other populations or in other locations may differ. Will similar treatments work for non-student populations or in different locations? And do behavioral change programs targeting one type of trip (such as a school trip) have spillover effects to other types of travel? In the future, a multisite case study or research about all types of travel following a behavioral change program could shed light on these outstanding uncertainties.

Compliance with ethical standards

Conflict of interest On behalf of all authors, the corresponding author states that there is no conflict of interest.

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