Working Paper 24-020

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Holly Dykstra, Shibeal O' Flaherty, and Ashley Whillans*

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Abstract

Behavioral interventions often focus on reducing friction to encourage behavior change. In contrast, we provide evidence that adding friction can promote long-term behavior change when behaviors involve repeated costly efforts over longer time horizons. In collaboration with the Oregon Department of Transportation, we conducted a field experiment (N = 27,227) to test whether adding friction during an initial sign-up process for a new carpooling platform increases usage. Our results support this possibility: while a more effortful sign-up process led to a 25% decrease in sign-ups to the carpool platform, overall intensity of usage increased. Importantly, these results were only partly explained by selection effects: using an intention-to-treat (ITT) analysis, participants who were randomly assigned to the more effortful sign-up process took 1.6 times more carpool trips per day on average during a four-month period as compared to those in the less effortful sign-up process. Of the 9,417 observed trips, the more effortful sign-up group took almost 800 more trips. These effects persisted at eight months, where the ITT estimate was a 33% increase in trips per day. These results suggest that adding friction may be an overlooked strategy that could help to promote behavior change.

Keywords: behavior change; friction; workplace initiatives; carpooling; sustainability

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1 Introduction

Despite their best intentions, people often fail to follow through on personally beneficial actions such as exercising, eating healthy, and saving for retirement (e.g., Rogers and Milkman, 2016). One explanation for this well-documented intention action gap is present bias—people tend to focus on doing what is easiest or most attractive in the moment. To overcome this tendency, behavioral science has developed interventions that largely focus on simplifying the decision-making environment (for a review, see DellaVigna and Linos, 2022). The logic behind this approach is that by making decisions easier, people are more likely to act in accordance with their long-term goals and motivations (Duckworth, Milkman and Laibson, 2018). This approach assumes that reducing upfront effort reliably encourages long-term behavioral change. In contrast to this typical approach, we argue that increasing effort—such as by making an initial action in the service of a personally important goal more difficult—may be an unexplored strategy that can increase commitment when a goal involves sustained behavior change.

The traditional approach, based on a robust literature in psychology and behavioral economics, suggests that making a decision-making process simpler improves long-run outcomes. For example, automatically opting employees into a 401k plan can increase the amount of money that employees save for retirement (Choi et al., 2002; Carroll et al., 2009). Similarly, making the application process for social safety net programs easier increases their take-up rate (Bhargava and Manoli, 2015). Notably, this research has typically examined how defaults and simplification can shift behavior in settings where individuals no longer have to exert effort to continue to engage in the focal behavior. When someone is defaulted into a 401k plan, unless they change the default, money is automatically deducted from their paycheck each cycle. Once someone has enrolled into a benefits program, they begin to receive the payments without having to engage in subsequent actions to continue to receive them.

However, in other settings, automatic enrollment might not be the best approach for encouraging long-term follow-through. For example, if a policymaker wanted to encourage consumers to sign up for a savings account that they had to actively contribute money into (in contrast to that money automatically coming out of their paycheck), automatic enrollment might not be the best approach. If opted in, consumers might not know or remember the option to save is there, or they might not feel psychologically invested if they did not feel like it was their decision to sign-up for the savings account to begin with. As a result, they may fail to consistently engage with the platform. In such cases, friction during the sign-up process might encourage follow-through by drawing attention to the behavior and by increasing a sense of ownership over the action. Indeed, meta-analytic evidence suggests that defaults are generally less effective in the context of pro-environmental decisions, where individuals often have to engage in repeatedly costly actions like taking more time to recycle or compost (Jachimowicz et al., 2019). Based on this research, we argue and provide initial support for the possibility that when actions require more consistent commitment, making an initial sign-up process more difficult can more effectively encourage people to follow through with their intended actions.

Past research has not directly tested this idea. However, a growing economics literature explores ordeal costs and commitment contracts to suggest that friction can encourage people to follow through with a personally important action. Commitment devices are an arrangement—entered into by an individual of their own accord—designed to help people achieve a difficult goal such as saving or exercising more frequently (Rogers, Milkman and Volpp, 2014). In one study (Beshears et al., 2020), people willingly opt for illiquid retirement saving accounts that prevent them from accessing their funds over liquid accounts that allow them to access their money at any time. While previous research has examined whether people self-select into using a commitment device, which makes it harder for them to fail at their stated goal, we make the initial achievement of the goal more difficult, and examine the downstream consequences of this choice architecture on personal behavior. This subtle difference allows us to differentiate between increasing the difficulty of initial sign-up efforts on long-term follow-through from efforts that make it more difficult for people to fail.

Why might exerting more upfront effort encourage follow-through? While we are not able to disentangle the specific mechanisms of the reported results, previous research provides reason to believe that increasing initial effort can encourage long-term behavior change. Exerting more effort during an initial action could increase follow-through by increasing psychological ownership (Norton, Mochon and Ariely, 2012) or attention to the action (Rogers and Milkman, 2016). For example, research shows that reminders and planning prompts can increase follow-through by increasing the cognitive accessibility of the intended action. Research on the sunk-cost fallacy also suggests that people value items or actions more highly when they have spent time or money on them (see Roth, Robbert and Straus, 2015 for a review). Finally, the literature on cognitive dissonance, self-perception theory, and escalation of commitment suggests that people should be more likely to follow through with an action made through an effortful sign-up process to reduce cognitive dissonance and create consistency across their intentions and behavior (Hinojosa et al., 2017; Maki et al., 2019; Staw, 1981).

In this paper, we report the results of a large-scale field experiment testing the viability of this "buy-in effect" strategy. In this experiment, we vary how difficult an initial sign-up process is for people to engage in a personally relevant decision that requires long-term follow-through: carpooling to the office. We focus on encouraging people to carpool to work for practical and conceptual reasons. Practically, individuals report a desire to commute to work in sustainable and active ways (Kristal and Whillans, 2020). Additionally, many workplaces offer carpooling matching services as an employer-sponsored benefit to reduce the costs associated with driving and parking (United States Environmental Protection Agency, 2005). Transportation is also one of the largest consumer-generated sources of greenhouse gases (United States Environmental Protection Agency, 2023). Thus, understanding how to change commuting behaviors is important from a societal perspective.

Conceptually, commuting behavior is an action that people engage in on a regular basis. From our theoretical perspective, increasing how difficult it is to engage in the first step of a process is more likely to produce positive behavioral impacts for an action that requires long-term persistence. Thus, making an initial sign-up process more effortful might be particularly likely to change people's commuting behaviors because these behaviors involve repeated costly efforts over longer time horizons. Importantly, standard incentives and nudges designed to make carpooling easier or more attractive—such as framing interventions—have not been effective at changing commuting patterns (see Whillans et al., 2021 for a recent review of behaviorally informed attempts to change commuting behaviors). Thus, it is possible that making the first step of the decision-making process more difficult could encourage follow-through in this setting. To the best of our knowledge, no field experiments have explored the impact of making a sign-up process more effortful on long-term commuting behaviors.

Our experiment takes place in collaboration with the Oregon Department of Transportation

(ODOT), which is the state-wide transportation department responsible for providing alternative modes of transportation to employers across the state of Oregon. In 2019, ODOT was migrating from an old carpool platform to a new platform, and wanted to increase the likelihood that individuals who were signed up on the old platform would sign up to and engage with the new platform. Both platforms serve the function of matching people with others who are interested in carpooling to the office. Importantly, given that individuals have to sign up for these services, the population of interest in our experiment consists of a motivated group of people who are interested in carpooling to and from work. For these individuals, carpooling represents a personally relevant goal or priority.

This migration from an older carpool platform to a newer carpool platform provided us with the opportunity to vary the email messages that were sent to individuals to encourage them to sign-up for the new platform. During this process, we designed and implemented a field experiment that targeted 27,227 individuals. Our experimental design, which we describe in more detail in the next section, involves randomly assigning these individuals to one of two email treatment conditions: a *Less Effort* condition or a *More Effort* condition. In the *Less Effort* email condition, participants only had to complete one step to access the new carpooling platform: create a new password. In the *More Effort* email condition, participants had to complete two steps to access the platform: enter their general account information, including personal and organizational information, and create a new password. Our primary research question was whether adding friction upfront could create a buy-in effect for the desired behavior change, and therefore increase later action. We examine behavior over a four-month period.

Consistent with this possibility, we find that effort in the sign-up process matters, both for initial sign-up to the carpool platform and for longer-term usage rates. Rates of sign-up follow the expected direction from previous work in the psychology and economics literature: the more effortful sign-up process leads to a 1.3 percentage point ($p < 0.01$) decrease in sign-up rates from a baseline sign-up rate of 5.2%, a decrease of about 25%. However, consistent with our hypothesis, we find that those in the *More Effort* treatment group who sign up to the carpool platform use the platform more intensively, taking about 2.6 times more trips per day ($p < 0.01$).

To ensure this is caused by a buy-in effect rather than simply a selection effect—where only the most motivated individuals complete the more effortful sign-up process—we look at ITT usage rates, where we compare trips taken by everyone in our original control and treatment groups. In this analysis, we find that the *More Effort* treatment group takes about 1.6 times as many trips per day ($p < 0.01$). In more concrete terms, out of the 9,417 total trips taken by individuals during our study period, those in the *More Effort* group took 795 more trips than those in our *Less Effort* group, saving about 8,975 miles of travel, even though fewer participants initially signed up from the *More Effort* group. The drop in initial sign-up rates was overcome by the increase in usage rates. Finally, we see smaller but still significant effects persist at eight months, after the disruption of the U.S. holiday season, where it could be expected that carpooling patterns might revert back to their baseline (Acland and Levy, 2010).

These empirical results show that increasing upfront effort or imposing friction costs can increase long-term behavior change in the context of commuting. Moreover, our results indicate that individuals who were assigned to the *More Effort* group demonstrated behavior change that was sustained over a four-month period. In contrast to past research which has largely focused on the reduction or removal of friction to encourage behavior change, this study provides evidence that a one-off intervention that increases friction at initial sign-up can translate to action and sustained behavior change over time.

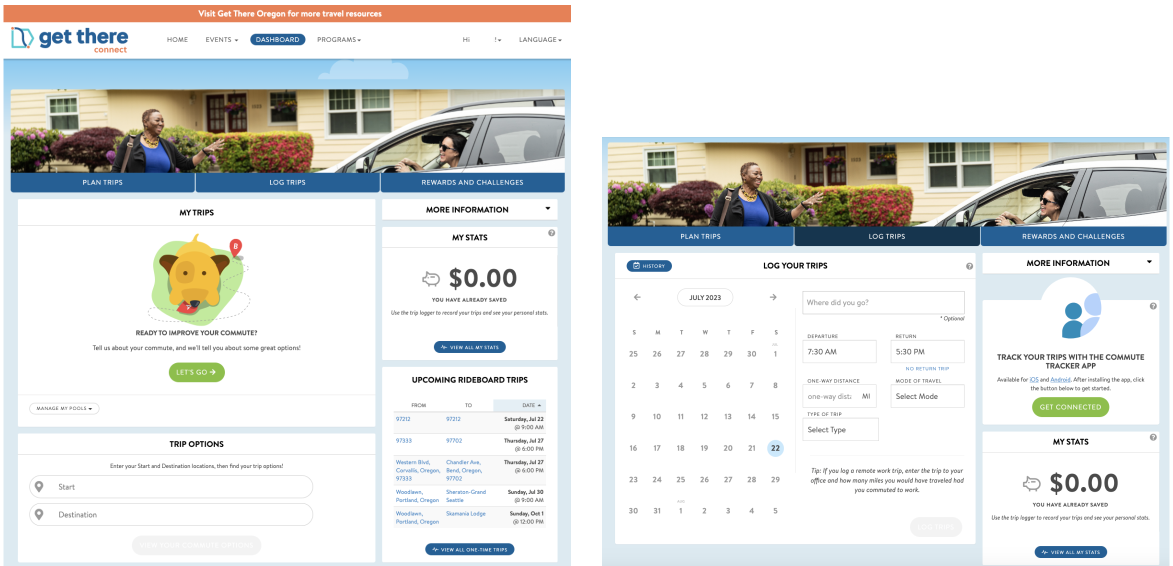
Our paper proceeds as follows. In Section 2, we outline our experimental design; in Section 3, we present our findings; and in Section 4, we discuss how these findings relate to research in behavioral science and discuss generalizability concerns and other limitations.

2 Experimental Design

2.1 Background

Our field partner, ODOT, is a department of the state government of Oregon that is responsible for systems of transportation. Prior to 2019, ODOT had a carpool platform called Drive Less Connect. This platform, available to everyone who lives or works in Oregon and often promoted by major employers in the state, helps match individuals with others driving in a similar direction to carpool to work together. In 2019, ODOT decided to migrate to a new carpool platform called Get There Connect, under the rebranded name of Get There Oregon. RideAmigos owns, develops, and runs the software for this platform. ODOT subscribes to the software system and makes it available to the people of Oregon to promote carpooling and multimodal trips.

Figure 1: Get There Platform Screenshots



Note: These screenshots show what the online Get There platform looks like from the user perspective. The image on the left shows the home page. The image on the right shows the interface for logging trips.

Once a user signs up to an account and logs in, they are brought to a dashboard where three tabs are presented: 'Plan Trips', 'Log Trips', and 'Rewards and Challenges' (see Figure 1). Each of the three tabs present several options for people to find and log carpool trips, or to view summaries of their trips to date. To find a carpool match, a user must click on 'Trip Options' which allows them to enter their start and destination locations, after which the platform will generate a list of potential carpool trip matches.

We used the migration process from the old to the new carpool platform as an opportunity to run a naturalistic field experiment. Switching to the new Get There platform required active participation from participants to complete the sign-up process. In collaboration with ODOT, we designed two versions of an email and sign-up process to encourage existing users of Drive Less Connect to sign up for the new platform.

2.2 Less Effort vs. More Effort Treatment

Both versions of the emails were designed to be very similar but to highlight the difference in effort required to migrate an account (see Figure 2). The only difference in the email language used across the treatment conditions was that the *Less Effort* version of the email informed existing users that their account was already set up for them, and that they could simply click on the link included in the email (with a link to a "Reset Password" button) to access their account. In the *More Effort* version of the email, existing users were told that they would need to create their account (with a link to a "Create Your Account" button).

In the *Less Effort* treatment, after clicking the "Reset Password" button, existing users completed one step to access their accounts: creating a new password. After entering in their new password, they saw a prompt indicating that they had received a verification email to their email address. In the *More Effort* treatment, after clicking the "Create Your Account" button, existing users had to create a new account. This process involved two steps. First, users had to enter their general account information, which included their first name, last name, email address, as well as being prompted to create a new password. Then, they moved onto a second page, where they were asked to input additional information, including their home zip code, work zip code, and organization. After submitting their information, existing users saw the same prompt indicating that they had received the verification email to their email address.

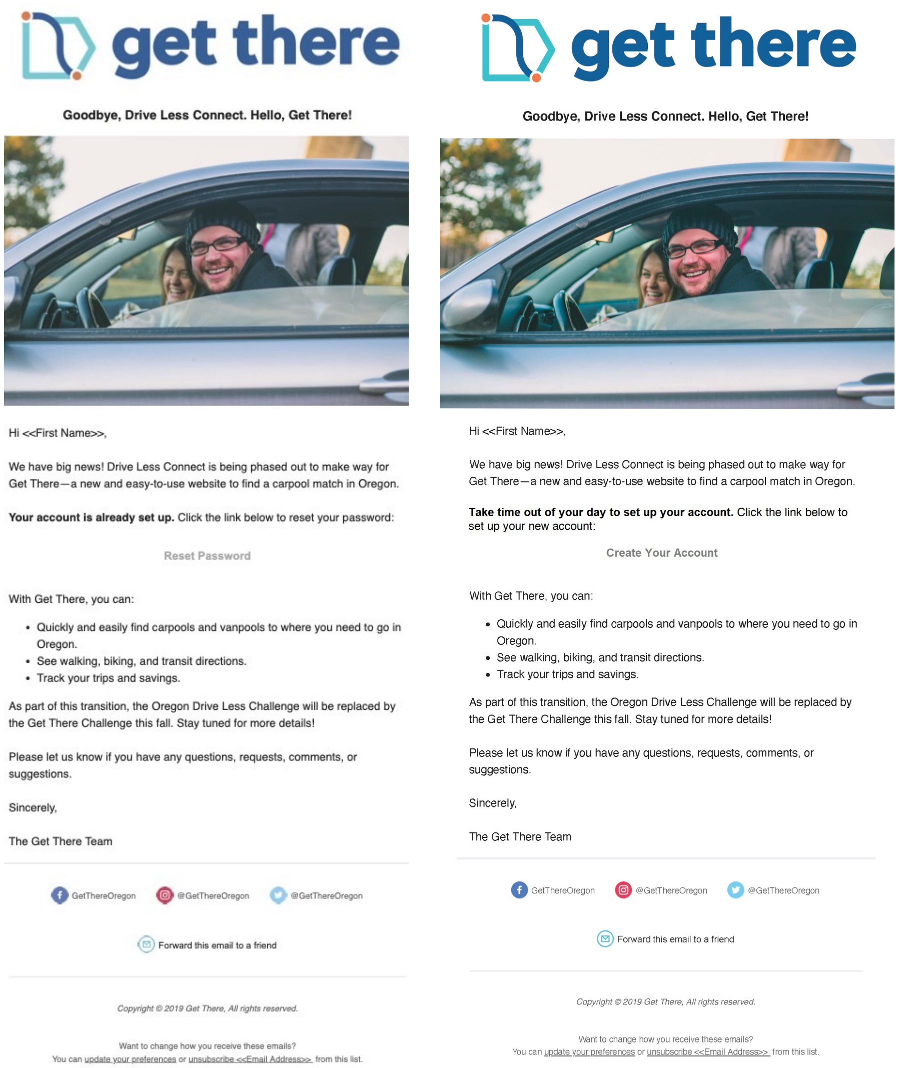
Critically, all existing users received identical toolkit information. The only major change was how much effort existing users had to exert in order to sign up for the new carpooling platform. This design decision ensured that the treatment condition was only manipulating effort, and not changing other factors that could impact the subsequent use of the carpooling system, such as objective information about the program or perceptions about program quality.

2.3 Sample and Randomization

We specifically targeted inactive users of ODOT's old carpool platform, who had previously signed up for the old platform but had not logged in for at least six months, and who were therefore less likely to sign up for the new platform themselves. ODOT was interested in increasing the percentage of these inactive users—who accounted for approximately 87% of all users—who successfully transferred over to and engaged with the new platform once it launched. See Figure 3 for the number of participants assigned to the *Less Effort* and *More Effort* groups, as well as the associated timeline.

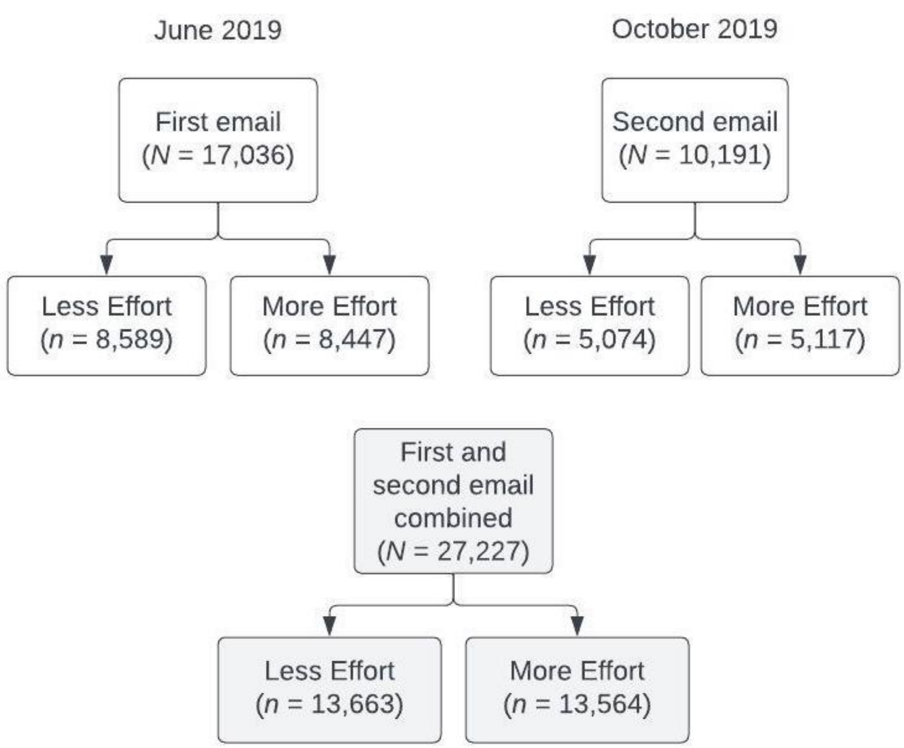
The treatment timeline ran as follows:

Figure 2: Treatment Emails



Note: These screenshots show the treatment emails that study participants received. The email on the left shows what those in the *Less Effort* treatment group received, while the email on the right shows what those in the *More Effort* group received.

Figure 3: Intervention Timeline and Sample Size



Note: This figure shows the timeline and total sample sizes for the two separate treatments.

First batch randomization: June 2019: After accounting for unusable email addresses (e.g., no longer in use, syntax errors, or already unsubscribed from communications), we had a sample of $N =$ 17,036 participants (with $n =$ 8,589 assigned to the *Less Effort* group, and $n =$ 8,447 assigned to the *More Effort* group). The dataset of inactive users was exported by our organizational partner, and randomization was conducted by their vendor on this list by assigning each individual a randomized integer, and then ordering users by that integer and exporting the bottom half. These emails were sent on June 29, 2019. After implementation, we discovered that one treatment group—the *More Effort* group—did not receive the emails until two days later, July 1, 2019. For that reason and others, our main outcome measure accounts for the number of days that participants were on the platform, as described in Section 3.

Second batch randomization: October 2019: Due to an administrative error, our organizational partner discovered that not all inactive users were originally pulled from the old database, so we conducted a second round of randomization and treatment on these users who had not yet been contacted by us. After again accounting for unusable email addresses, we had a total sample of N = 10,191 participants, with n = 5,074 assigned to the *Less Effort* group, and n = 5,117 assigned to the *More Effort* group in this round. These emails were sent on October 7, 2019.

We collected study outcome data after four months, which included data from June 29, 2019 through October 31, 2019, to coincide with a time of consistent commuting before the U.S. holidays interrupted commuting patterns. Because of the administrative error described above, we analyze our results including an indicator variable for receiving the email in October as a robustness check, which can be found in Section 3 below. We also received eight-month follow-up data through February 29, 2020; these results are described in the same section.

The descriptive statistics across the entire ITT sample of $N =$ 27,227 participants are reported in Table 1. We recorded an average of .346 trips per person (9,417 in total) and 2.96 miles per person (80,581 in total), who spent an average of 3.29 days on the platform during our study timeframe. $n =$ 1,205 participants signed up to the carpool platform (approximately 4.4% of the overall sample), who took an average of 7.81 trips, traveled 66.9 miles, and spent an average of 74 days on the platform.

We conduct a balance check to test whether groups were equal with respect to the data we had

Table 1: Descriptive Statistics

	Count	*Mean (ITT)*	*Mean (Signed Up)*	*Min.*	*Max.*
Trips	9,417	.346	7.81	0	158
Miles	80,581	2.96	66.9	0	3,695
Days on Platform	89,461	3.3	74.2	0	124
Observations		27,227	1,205		

at baseline, which was gender and race.[1,2] We also checked balance with respect to being in our first and second round of treatments, that is, whether they received an email in June or October. See Table 2 for results. The balance of White study participants was slightly different, 76.5% vs. 77.8% ($p < 0.01$), so we control for this variable in robustness checks in Section 3.

2.4 Outcomes

We collect data on study participants' activity on the Get There carpool platform, which was already being collected by RideAmigos: date of first sign-up, number of carpool trips, number of miles. We measured outcomes four months after the initial emails were sent. We also have data on participants' home and work zip codes.

3 Experimental Results

We analyze the effect of our experiment on initial sign-ups to the carpool platform and follow-through usage. We measure follow-through usage through two main outcomes of interest: the average number of trips per day and the average number of miles per day, with per day meaning the number of days on the carpool platform since the user first signed up. We use the per day outcomes as a measure of intensity of usage because we do not want our results to be biased by how long within our finite timeframe they were active. This per-day measure of our outcomes also makes it easier to compare our findings with other commuting studies, which may have different timeframes (Mertens et al., 2022). Finally, as discussed in Section 2.3, there were two administrative errors that meant that certain groups received the treatment email at different times, making this adjustment important.

[1] This data was not held by our partner. Instead, we used the R package predictrace to predict gender using first names and race using last names based on U.S. Census and Social Security Administration Data (Kaplan, 2023). While R's predictrace was noted the best performing algorithm in a recent review paper, it is still not very reliable (Lockhart, King and Munsch, 2023). However, we expect errors to be evenly distributed across treatment groups, which suffices for our purposes here.

[2] We only have geographic data for those who actually signed up to the platform, which we therefore expect to vary by treatment group, and cannot use as a baseline check.

Table 2: Balance Table

Variable	(1) 0 Mean/(SE)	(2) 1 Mean/(SE)	(1)-(2) Pairwise t-test Mean difference
October	0.371	0.377	-0.006
	(0.004)	(0.004)	
Female	0.563	0.556	0.007
	(0.004)	(0.004)	
Race and Ethnicity			
American Indian	0.000	0.000	0.000
	(0.000)	(0.000)	
Asian	0.042	0.039	0.004
	(0.002)	(0.002)	
Black	0.013	0.013	0.001
	(0.001)	(0.001)	
Hispanic	0.054	0.049	0.004
	(0.002)	(0.002)	
Two or More Races	0.000	0.000	0.000
	(0.000)	(0.000)	
White	0.765	0.778	-0.014***
	(0.004)	(0.004)	
Number of observations	13663	13564	27227

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table shows the mean and standard error of each of the pre-sign-up variables. These include whether the study participant received the email in June or October, as well as their gender, race, and ethnicity. The last column shows the p-values of a pairwise t-test of the mean difference.

Throughout our main results, we estimate the following bivariate ordinary least squares (OLS) equation:

$$y_i = \alpha + \beta Effort_i + \epsilon_i \tag{1}$$

where y is one of our three outcome measures: sign-ups, trips per day, or miles per day for a given user i. *Effort* is an indicator for being in the *More Effort* treatment group, and its coefficient, β, measures the average difference between the more effortful and the less effortful sign-up process.

To measure the effect of our treatment on initial sign-ups, we use the entire sample of participants that we randomized into treatment groups. We measure the results on follow-through usage

Table 3: Estimated Treatment Effects of Effort

		Conditional on Sign-Up		ITT	
	(1)	(2)	(3)	(4)	(5)
	Sign-Ups	Trips per Day	Miles per Day	Trips per Day	Miles per Day
Effort	-0.013***	0.248***	1.823***	0.008***	0.059***
	(0.002)	(0.043)	(0.307)	(0.002)	(0.013)
Constant	0.051***	0.095***	0.729***	0.005***	0.037***
	(0.002)	(0.009)	(0.093)	(0.000)	(0.005)
Observations	27,227	1,205	1,205	27,227	27,227

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Each column presents the results of a bivariate OLS model with an indicator for being in the *More Effort* group as the independent variable. Column 1 estimates the effect on an indicator for having signed up to the carpool platform. Columns 2 and 3 estimate the treatment effects on the number of trips taken per day on the platform and the miles driven per day, conditional on having signed up. Columns 4 and 5 estimate the treatment effect on trips per day and miles per day on the entire sample.

in two ways: (1) conditional on having signed up, and (2) using the entire sample of participants that we randomized into treatment groups for an ITT measure. Because our interest is in the overall intensity of carpool usage, this latter measure lets us measure the overall effect of our intervention on usage, and also lets us understand the effect of our treatment without including the effect of selection into sign-ups.

3.1 Effects of Effort on Initial Sign-Ups

To understand the effect of effort during the sign-up process, we first analyze the effect of effort on signing up to the carpool platform. Results can be found in the first column of Table 3. We find that a more effortful sign-up process reduces the likelihood of signing up to the platform: participants in the *More Effort* treatment group are 1.3 percentage points less likely to sign up, off of a baseline of a 5.1% sign-up rate for the *Less Effort* group ($p < 0.01$). This corresponds to a 25% decrease in sign-up rates when the sign-up process requires more effort.

3.2 Effects of Effort on Follow-Through Usage

Next, we analyze the effect of effort in the sign-up process on follow-through behavior using the number of trips taken per day and the number of miles driven per day on the carpool platform during our study period.

We first analyze the effect of effort on usage conditional on having signed up. In other words, given that an individual has created an account, how much do they use it? Results from the 1,205 study participants who signed up are shown in columns two and three of Table 3. Participants in the *More Effort* group generally use the carpool platform more intensively. While those in the *Less Effort* group take an average of approximately 0.1 trips and 0.7 miles per day, those in the *More Effort* group take about 0.25 more trips per day and drive about 1.8 more miles per day ($p < 0.01$). Thus, participants who signed up for the carpool platform through the more effortful process made more use of it. But is this because of a buy-in effect, or is this simply evidence of a selection effect, where only the most motivated people signed up in the *More Effort* treatment group in the first place?

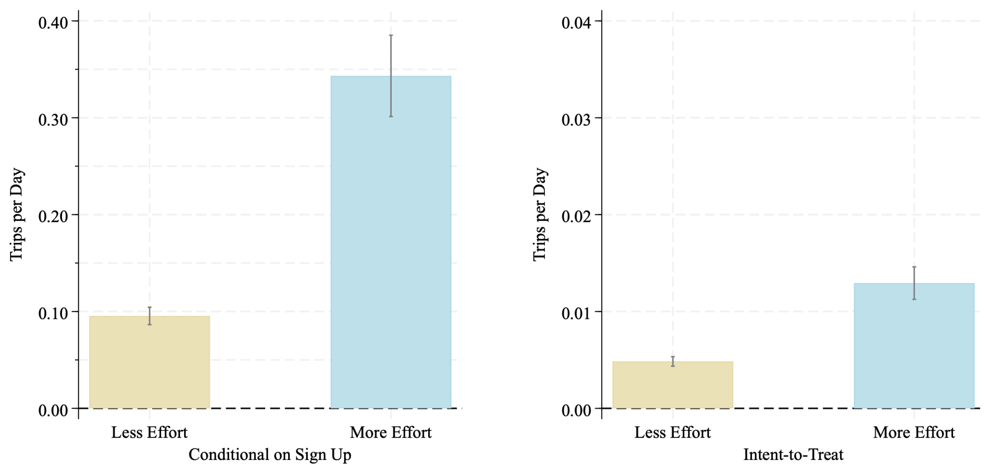
To test this question, we estimate the same OLS model but this time with our entire sample, not conditional on initial sign-up. This gives us the ITT estimate of our treatment. Results are shown in columns four and five of Table 3, and the trips per day results are shown again graphically in Figure 4. Follow-through usage is still higher in the ITT estimate of the effect of effort. Participants assigned to the *More Effort* group take an average of 0.008 more trips per day and drive about 0.06 miles more per day than those in the *Less Effort* group off of baselines of 0.005 and 0.04, respectively, providing evidence of a separate buy-in effect ($p < 0.01$).

Looked at differently, the *More Effort* group took more trips overall, despite there being fewer participants who signed up to the platform: while 694 signed up from the *Less Effort* group, only 511 signed up from the *More Effort* group. During our 122-day study period, we observed 9,147 total trips; out of these, the *More Effort* group took 5,106 trips, while the *Less Effort* group took 4,311, meaning that *More Effort* group took 795 more trips overall than our *Less Effort* group. The drop in initial sign-up rates was overcome by the increase in usage rates.

3.3 Intent-to-Treat Estimate as Evidence of a Buy-in Effect

These results suggest the existence of a buy-in effect. If the more intensive usage of the*More Effort* group was only due to a selection effect, we would expect the ITT treatment effect—the impact of being assigned to the *More Effort* treatment on the number of trips taken—to result in fewer trips per day than being assigned to the *Less Effort* treatment, and thus analogously, in fewer overall trips taken. To see this, consider that our sample of participants can be divided into three groups: the always-takers, who sign up to the platform regardless of their treatment group; the never-takers, who never sign up to the platform; and the complacents, who sign up when it is

Figure 4: Estimated Treatment Effect of Effort on Trips per Day



Note: These graphs show the average number of trips per day taken by treatment group. The graph on the left shows this outcome for participants who ever signed up to the carpool platform, while the graph on the right shows this outcome for the entire sample. The lines represent standard error bars.

easy to do so in the *Less Effort* group, but do not sign up when the process is more difficult. By having the same number of always-takers in both treatment groups, but losing the complacents in the *More Effort* group—which we see through the reduced number of sign-ups—we would expect to also see fewer trips taken in the *More Effort* group than in the *Less Effort* group in the ITT analysis. This is because the addition of the complacents can only increase the number of trips taken, and we would expect them to take at least some trips. However, what we find is that the number of trips increases, suggesting that the always-takers in the *More Effort* group are using the app more intensively, in a way that compensates for the reduction in the extensive margin through the loss of the complacents.

3.4 Robustness Checks and Further Analysis

We run a series of robustness checks on these findings. First, we re-run our main regression models to include the individual-level variables in our data as controls. This ensures that our results are due to the treatment itself, rather than due to some other observable difference between the treatment and control groups in case randomization was not effective. We have an indicator for receiving the treatment email in June or October, for having the same home and work zip codes, and for gender, race, and ethnicity. The data on zip codes is only available post-treatment from participants who signed up to the carpool platform, so we can only include that variable for the models conditional on sign-up.

Results are shown in Table 4. We observe that the main treatment effects remain unchanged.[3] We also find that women sign up to the platform and generally use the app the same amount as men. We do, however, see a relationship between having the same home and work zip codes and the intensity of usage: *Same Zip Code* is correlated with driving fewer miles, which makes sense if the variable works as a proxy for commuting distance. We also find a relationship between receiving the treatment email in October and our outcome variables. Participants who received the email in October are less likely to log in, perhaps because they had less time to do so since our study period was over at the end of the month, but those who did log in were slightly more likely to use the app, possibly suggesting a seasonality to platform usage.

Next, we also re-run our main results using a Poisson regression model in order to take into account the discrete, non-negative nature of the count outcome variables (see results in Appendix A, Table A1). The direction and significance of the results remain unchanged.

We also investigate the sensitivity of our results to the presence of outliers. It is possible that there are a small number of participants who use the platform much more than others who are driving our results, so that our findings do not reflect an average change in usage, but rather the behavior of a small group. To address this possibility, we re-run our regression models under increasingly restrictive one-sided winsorization of usage at the 99th, 95th,and 90th percentiles of usage for participants who signed up. These percentiles are 1.9, 0.82, and 0.54 trips per day, and 24.9, 6.9, and 3.4 miles per day, respectively. These results are shown in Table 5. Although the coefficient estimates reported in these columns are smaller in magnitude than in our un-winsorized sample, as expected, they remain significant at each level of winsorization.

[3]There was no robust interaction between the two demographic variables that we measured at baseline—gender and race—and condition assignment. Thus, these results are not discussed further.

Table 4: Estimated Treatment Effects of Effort with Controls

		Conditional on Sign-Up		ITT	
	(1) Sign-Ups	(2) Trips per Day	(3) Miles per Day	(4) Trips per Day	(5) Miles per Day
Effort	-0.015***	0.251***	2.013***	0.007***	0.058***
	(0.003)	(0.054)	(0.365)	(0.002)	(0.014)
October Email	-0.021***	0.203***	1.331***	0.002	0.009
	(0.003)	(0.078)	(0.481)	(0.002)	(0.016)
Same Zip Code		-0.032	-1.254***		
		(0.043)	(0.291)		
Female	-0.000	0.057	0.470*	0.002	0.020
	(0.003)	(0.037)	(0.277)	(0.002)	(0.013)
Race and Ethnicity					
American Indian	-0.042***			-0.009***	-0.066***
	(0.005)			(0.002)	(0.013)
Asian	-0.005	0.019	-0.088	-0.000	-0.010
	(0.007)	(0.089)	(0.656)	(0.004)	(0.028)
Black	-0.009	0.641	3.271	0.019	0.092
	(0.010)	(0.675)	(3.711)	(0.026)	(0.146)
Hispanic	-0.016***	-0.106***	-0.544	-0.005***	-0.032*
	(0.005)	(0.040)	(0.519)	(0.001)	(0.017)
Two or More	-0.060***			-0.005***	-0.042***
	(0.003)			(0.001)	(0.008)
Constant	0.061***	0.010	0.294	0.003	0.023*
	(0.003)	(0.040)	(0.264)	(0.002)	(0.013)
Observations	23,115	1,002	1,002	23,115	23,115

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Each column presents the results of a multivariate OLS regression model with independent variables for treatment group, whether they received the treatment email in June or October, an indicator variable for having the same home and work zip codes, as well as indicator variables for gender, race, and ethnicity. The base group for race and ethnicity is white. Columns 2 and 3 estimate the treatment effects on the number of trips taken per day on carpool platform and the miles driven per day, conditional on having signed up. Columns 4 and 5 estimate the treatment effect on trips per day and miles per day on the full sample.

Table 5: Estimate of Treatment Effect on Usage (Winsorized)

	99th		95th		90th	
	(1)	(2)	(3)	(4)	(5)	(6)
	Trips per Day	Miles per Day	Trips per Day	Miles per Day	Trips per Day	Miles per Day
Effort	0.209***	1.809***	0.181***	1.456***	0.173***	1.101***
	(0.024)	(0.278)	(0.016)	(0.159)	(0.013)	(0.093)
Constant	0.092***	0.667***	0.075***	0.456***	0.059***	0.305***
	(0.008)	(0.073)	(0.005)	(0.036)	(0.003)	(0.019)
Observations	1205	1205	1205	1205	1205	1205

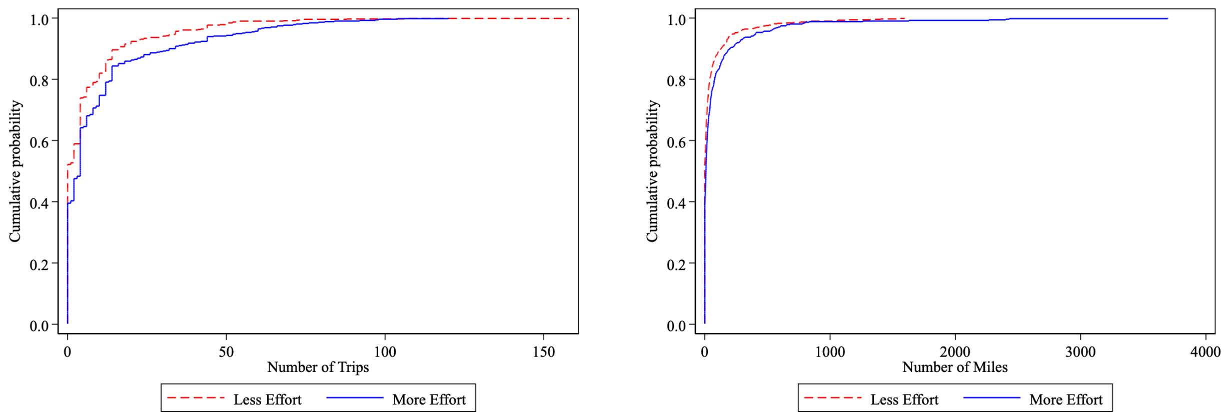
Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Each column presents the results of a bivariate regression model with an indicator for being in the *More Effort* group as the independent variable. The outcome variables are trips per day and miles per day. Each outcome variable was winsorized at the 99th, 95th, and 90th percentiles.

We further investigate the presence of outliers in Figure 5, by plotting the cumulative distribution functions (CDFs) of the number of trips and miles on the carpool platform for those who signed up. For both outcome measures, we see a sustained difference in intensity of usage across the distribution, instead of only in the lower or upper tails. This provides further evidence that outliers or noise are not driving the observed differences across treatment groups. We confirm this formally with non-parametric Kolmogorov-Smirnov tests of the null hypothesis that the treatment distribution was drawn from the control distribution, which we reject ($p < 0.01$).

Finally, after our main study was over, we received follow-up data from ODOT that ran eight months through February 29, 2020. Compared to our main results, this follow-up included the U.S. holiday season, which we expected to interrupt commuting patterns, but before the Covid-19 pandemic fully disrupted these patterns. Results are shown in Table 6.

We see essentially the same effects on sign-ups, consistent with few people joining the new carpool platform during this timeframe. We see overall lower but still significant treatment effects for both our group of people who ever signed up as well as our ITT group, where participants assigned to *More Effort* group took 33% more trips per day than those in the *Less Effort* group

Figure 5: Cumulative Distribution Functions by Effort



Note: These figures plot the CDFs of number of trips and number of miles for participants who signed up to the carpool platform. Formal Kolmogorov-Smirnov tests of the equality of treatment and control distributions reject the null for each ($p < 0.01$).

Table 6: Estimated Treatment Effects of Effort at 8-Month Follow-up

		Conditional on Sign-Up		ITT	
	(1) Sign-Ups	(2) Trips per Day	(3) Miles per Day	(4) Trips per Day	(5) Miles per Day
Effort	-0.0125***	0.0634***	0.4702***	0.0015***	0.0098
	(0.0025)	(0.0108)	(0.1459)	(0.0006)	(0.0070)
Constant	0.0524***	0.0852***	0.7180***	0.0045***	0.0376***
	(0.0019)	(0.0055)	(0.0910)	(0.0003)	(0.0050)
Observations	27,227	1,257	1,257	27,227	27,227

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Each column presents the results of a bivariate OLS model with an indicator for being in the *More Effort* group as the independent variable, with all outcome variables calculated through February 29, 2020. Column 1 estimates the effect on an indicator for ever having signed up to the carpool platform. Columns 2 and 3 estimate the treatment effects on the number of trips taken per day on the platform and the miles driven per day, conditional on having signed up. Columns 4 and 5 estimate the treatment effect on trips per day and miles per day for the entire sample.

($p < 0.01$). We find it promising that we still see a persistent effect after eight months, especially because we expected the holiday season to disrupt commuting patterns. In general, an avenue for further research is to study how long until a new habit is fully embedded and resistant to breaks in habits or external shocks (Milkman et al., 2021; Walker, Thomas and Verplanken, 2015).

3.5 Discussion

Why might adding friction encourage effortful follow-through? While this field experiment allows us to show the existence of a buy-in effect, we cannot provide evidence on the underlying mechanisms behind this effect. Nevertheless, we present a short discussion on possible mechanisms and competing hypotheses to motivate future work.

First, making an initial sign-up process more difficult could increase the subjective value of the action. Past research shows that when people exert more effort to complete a behavior, they feel a greater sense of psychological ownership over the action and value it to a greater extent, which could increase their likelihood of following through (Norton, Mochon and Ariely, 2012). This finding is also consistent with the literature on the sunk cost fallacy, which occurs when people value items or actions more that they have already spent time or money on (see Roth, Robbert and Straus, 2015 for a review).

Second, a more difficult sign-up process could also increase people's attentional focus on the intended action, causing people to follow through with the behavior at a higher rate. This argument is consistent with research showing that reminders and planning prompts can increase follow-through by increasing the cognitive accessibility of the personally important behavior, as well as with the literature on habit formation: once people engage in an initial behavior, the memory of the behavior has a higher likelihood of being activated and repeated (Rogers and Milkman, 2016; Bordalo, Gennaioli and Shleifer, 2020).

Third, cognitive dissonance theory may explain why increased upfront effort during the sign-up process could result in behavioral follow-through. This theory suggests that whenever a person holds two cognitions (ideas, attitudes, beliefs, opinions, or observed own behaviors) that are psychologically inconsistent, this results in an unpleasant state—dissonance—until they are able to resolve this state by altering their cognition (Hinojosa et al., 2017). People who exert more upfront effort may be more likely to engage in behavioral follow-through in order to reduce dissonance with the initial upfront effort. In a similar vein, people look to their behavior to infer their self-concept and strive to maintain consistency across their beliefs and behaviors, a concept known as self-perception theory (Maki et al., 2019). Thus, engaging in a more effortful sign-up process could encourage people to follow through because the behavior becomes seen as more personally important. Finally, this finding could also be explained by literature on the commitment bias or the escalation of commitment, which describes an individual's tendency to remain committed to past behaviors (Staw, 1981). What these theories all have in common is that they argue that exerting more effort upfront could increase the accessibility of the action and/or the perceived importance of the action, thus facilitating follow-through.

In addition to the buy-in effect, there are two competing hypotheses about the effect of a more difficult sign-up process, which were not consistent with the results of our study. The first is that the drop in the number of people signing up to the platform due to the more effortful sign-up process would lead to an overall reduction in the number of trips taken. In other words, an easier sign-up process is better, because more people using the platform means the platform would be used more. This is not consistent with our results, where we find that the drop in the extensive margin—the number of people signed up—was compensated by an increase in the intensive margin, the usage rates of those who signed up, resulting in a higher overall number of trips taken. The second hypothesis is the self-licensing effect, where engaging in a positive act makes someone feel like they have the license to engage in poor behavior after. In this case, exerting more effort to sign up to the carpool platform might make people feel like they have already engaged in an environmentally-friendly action, so that they are less likely to engage in another effortful action afterwards (Ottelin, Cetinay and Behrens, 2020). However, this is also not consistent with our results, where we find the effect going in the opposite direction: those who sign up in the more effortful process then use the platform more intensively.

In light of the possibility of competing hypotheses, our results advance behavioral theory in a few important ways. Specifically, and in contrast to recent reviews of the behavioral science literature, simplifying the decision-making process might not be the best strategy for increasing behavior change in a relatively motivated population. Indeed, we show that making a process more difficult still encourages more people to use the new carpool platform than making a process easier, even when accounting for the possibility of selection effects. Second, recent research suggests that there is a role of morale licensing such that people are less likely to follow-through with pro-environmental behavior when they feel like they have already engaged in pro-environmental behavior. In contrast to this possibility, people who engaged in the effortful action were *more* likely to follow through with a behavior—carpooling—that can have important consequences for environmental behavior. Future research is needed to explore more the mechanisms behind why adding friction resulted in more follow-through and did not crowd out the subsequent willingness to engage in the behavior.

4 Conclusion

This study provides novel insights regarding the addition of friction to promote sustained behavior change—specifically, encouraging individuals to follow through on carpooling. Our findings have implications for research in economics, psychology, management, and public policy.

First, to our knowledge, this is the first instance where a behavioral intervention has been designed with the intention of adding friction to promote long-term behavior change. While most research focuses on making programs as easy as possible, this study provides early evidence that adding friction during the sign-up process can encourage follow-through. These findings highlight an important area of further research: how to effectively add or remove friction to encourage behaviors that involve long-term sustained effort, like commuting.

Second, our research has implications for policy and management. Contrary to past guidance, it might be fruitful for policymakers and managers to consider contexts and scenarios where building in friction upfront—i.e., getting people's buy in—might lead to increased psychological ownership over the decision, and as a result, increased behavior change. Of course, there are many situations where the addition of friction could potentially be harmful, particularly from an equity standpoint. For example, individuals from underserved communities may face additional barriers to signing up for a benefit or program, and so making it more difficult for them to do so would be harmful. Decision-makers should consider whether the addition of friction—such as the more effortful sign-up process presented here—is appropriate for the given context, and pilot test this accordingly.

Third, increasing carpooling rates as opposed to driving alone has the potential to drastically reduce the amount of greenhouse gases as a result of transportation. The number of miles driven by carpooling by participants in our sample saved approximately 3.04 metric tons in CO_2 emissions, assuming that they would have otherwise commuted by driving alone.[4] Carpooling does not just have environmental benefits; it also has positive benefits for individuals in the form of time and money saved as well as health benefits, including reduced stress (Whillans et al., 2021). Our findings also show that reducing greenhouse gas emissions from driving alone does not require getting all individuals who can possibly carpool to do so: we still see positive net results when we focus on encouraging the more motivated people to engage in the behavior. By nudging these individuals with an intervention that increases buy-in upfront, policy makers can move the needle on greenhouse gases arising from transportation.

Fourth, and crucially, we find evidence of sustained behavior change: our findings of increased carpool trips and miles taken among participants in the *More Effort* group through four months of observation, and we see effects persist at eight months. A recent megastudy conducted to encourage gym attendance found that most of the nudge interventions tested had one-off effects, and that these effects did not persist over time (Milkman et al., 2021); while it tested 54 different interventions to encourage people to exercise regularly, just 8% of these resulted in sustained behavior change after the four-week intervention period had ended. Our findings are particularly powerful in that they show long-term behavior change in a context where behaviors are notoriously difficult to alter.

Our study has a number of limitations, which could be addressed in future research in this area. First, this was a single field experiment in the context of commuting, and we do not yet know the extent to which our findings generalize to other contexts and settings. An important contextual factor to note is that the study was conducted in the year prior to the Covid-19 pandemic, when people were generally commuting to work more often. Now that people are working from home at a higher rate, we do not know how this intervention would play out given current commuting trends.

Second, we could not test further factors that could be driving our findings. Future research could explore the potential mechanisms behind the buy-in effect, and whether there are certain individual-level traits that mediate this effect—for example, pro-environmental identity or prosocial motivation, or an increased felt psychological ownership over the behavior. Similarly, it could also

[4] CO_2 emissions were calculated using https://www.carbonfootprint.com/calculator.aspx. For the calculation inputs, we used the difference in the total number of miles driven via carpooling for the *More Effort* versus *Less Effort* group (8974.93) after four months, and the average car efficiency ratings of cars, which is 24.2 miles per gallon according to data from the U.S. Department of Energy in 2020: https://afdc.energy.gov/data/10310

be the case that contextual factors about the platform make a difference, like the fact that this was a platform they had previously engaged with. Finally, the level and type of effort involved was consistent across both treatments in our study—we do not know yet how and if the effect would persist across different types and levels of effort.

In conclusion, we provide initial evidence from a large field experiment with a major US state transportation program demonstrating that the addition of upfront friction—creating a buy-in effect—is an effective and motivating tool to increase carpooling rates among commuters. We hope this study can lay the groundwork for future research examining behavior change in the context of commuting, as well as for other intractable behavior change contexts. This paper contributes to emerging research on encouraging behavior change in sustainability, as well as to the behavioral science literature more broadly.

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Appendix

A. Additional Table

Table A1: Estimated Treatment Effects of Effort using Poisson

		Conditional on Sign-Up		ITT	
	(1)	(2)	(3)	(4)	(5)
	Sign-Ups	Trips per Day	Miles per Day	Trips per Day	Miles per Day
Effort	-0.299***	1.280***	1.253***	0.982***	0.954***
	(0.057)	(0.154)	(0.171)	(0.164)	(0.180)
Constant	-2.980***	-2.350***	-0.316**	-5.330***	-3.296***
	(0.037)	(0.094)	(0.127)	(0.101)	(0.132)
Observations	27,227	1,205	1,205	27,227	27,227

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Each column presents the results of fitting a poisson regression model with an indicator for being in the *More Effort* group as the independent variable. Column 1 estimates the effect on an indicator for having signed up to the carpool platform. Columns 2 and 3 estimate the treatment effects on the number of trips taken per day on the platform and the miles driven per day, conditional on having signed up. Columns 4 and 5 estimate the treatment effect on trips per day and miles per day on the entire sample.